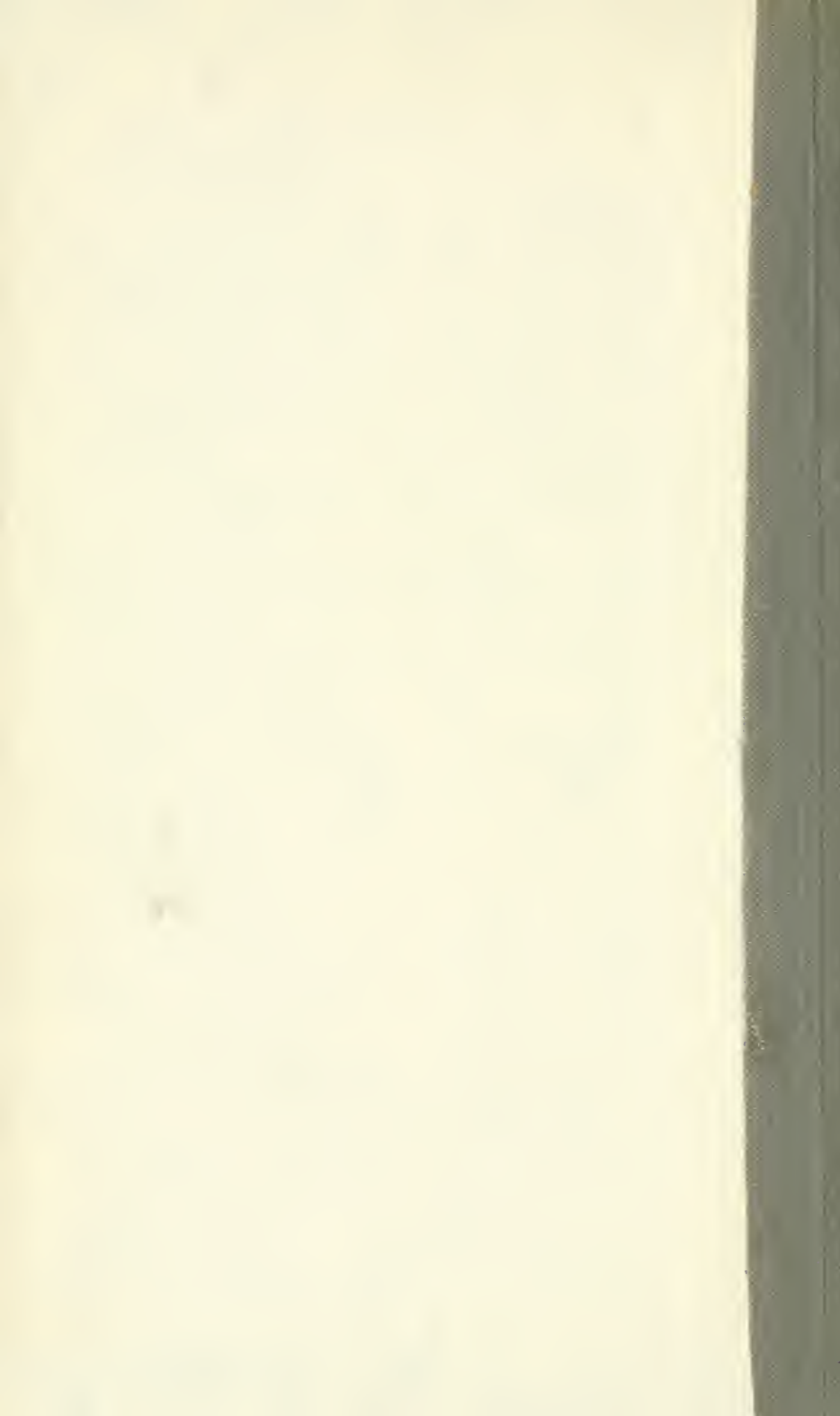
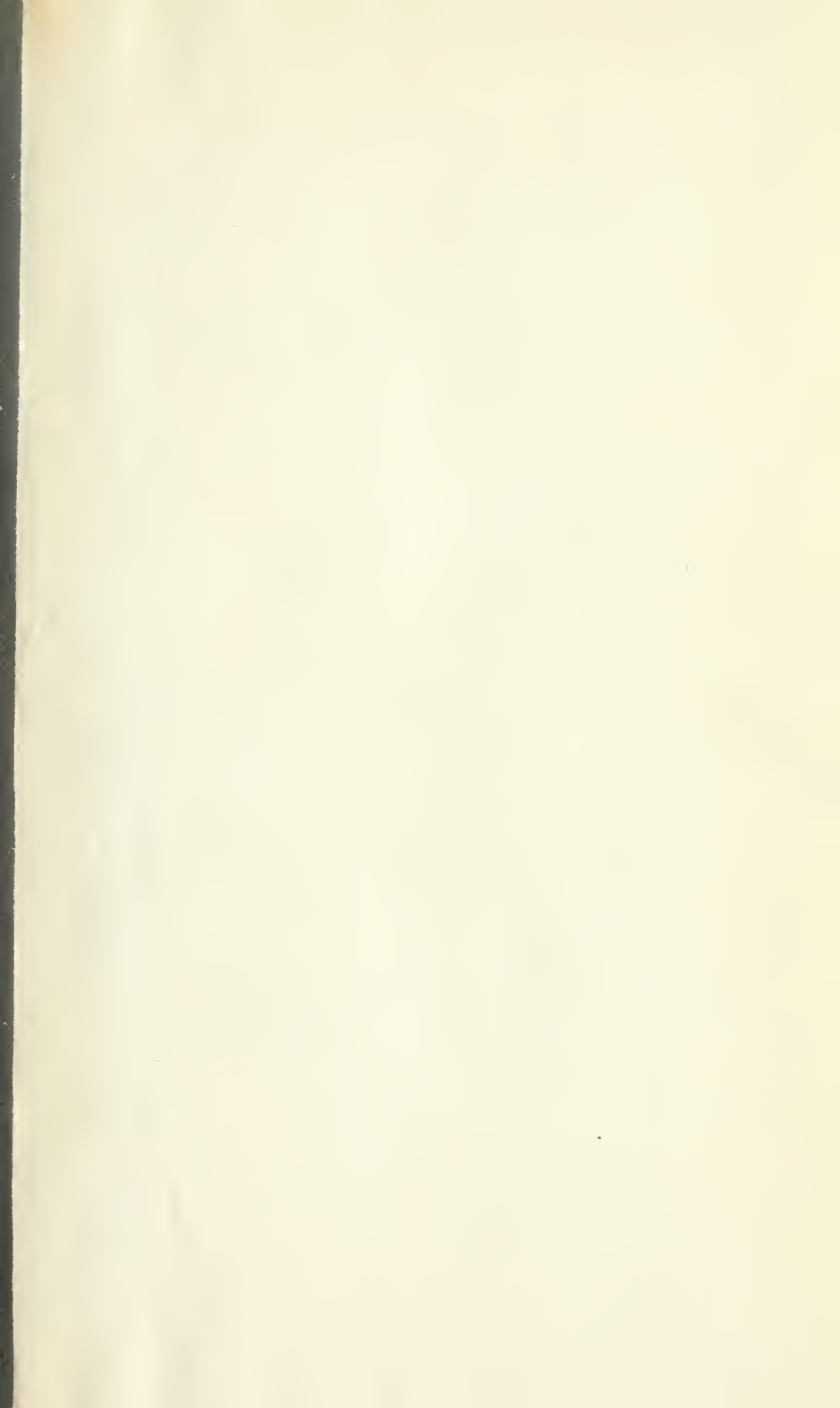


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EDITED BY

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ORIGINAL COMMUNICATIONS.

THE NORMAL STANDARD OF WOMAN FOR PROPAGATION.*

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BEFORE entering upon the discussion of the subject, it may be proper to give a brief definition of the leading terms used in the heading of this paper. The term *normal* implies that there exists some rule or guide for reference. In anatomy and physiology it implies sound structure and healthy function, but in medicine, generally, the term is used to designate a state of health, and the term abnormal is applied to deviations from that state. As there may be different degrees of health, or

¹ In the perusal of this paper, we would bespeak the "good-will" of the reader on three considerations: 1st. The whole subject is so large and complicated that only a mere outline of facts and arguments can be presented in a short article; 2d. Though the topics discussed may be familiar, the stand-points for examination are quite different from what are usually taken in lectures and books; and 3d. The line of discussion here pursued, in favor of a general law of propagation, constitutes only one out of many others connected with the laws of physiology, in their relations to the various questions affecting the great subject of population.

slight deviations from a perfect state, standards artificial or imperfect may be set up, and the term normal may, with much propriety, be applied to them. So in the application of physiology to propagation, there may be differences in organization, or in the objects to be obtained, as there may be different physiological standards or types. While custom and convenience might sometimes allow such use and interpretation of language, the phrase normal standard is here intended to mean strictly the highest standard or most perfect development which physiology can present. Moreover, the phrase normal standard, as here used, is not intended to apply simply to this or that organ in its best or highest development, but to all parts of the body—to every organ in the human system—that each should be perfect in structure, and each perform its complete function. This standard, that it may be normal both in structure and function throughout, must be based upon a physical system evenly and well developed in every part or organ, so that each can perform its respective functions in harmony with all the rest. While we may not find this perfect physiological standard, we find approximations to it in great numbers—some much nearer than others; but deviations from it may be found in endless variety. Still, the general law, the normal standard, must have its basis in the highest or most perfect developments of the body as regards its anatomy and physiology. That there does exist a great difference in organization amongst women, with reference to propagation, all experience proves. All books treating upon this subject admit it, and give particular instructions founded upon those peculiarities. That, in these differences of organization, one kind of development, one form of body, or one class of organs being better developed than another, is more favorable for the fulfilment of this law of propagation, requires no long array of arguments to prove. What then is the anatomical or physiological development most favorable? Does it consist wholly in the construction of the pelvis or of the organs of that region? In the great law of propagation are there not many other conditions or objects to be considered, besides parturition or the mere mechanism of labor? And may not these other considerations have been too much overlooked, on account of the great prominence given to one particular part of the physical system? In order to under-

stand correctly and observe properly a general law of nature, all its conditions, requirements, and objects must be carefully taken into the account, that the designs of each may be fully secured. Not only the constitution and health of woman, the peculiar effects of gestation and the physical changes occasioned by it, must be considered, but her qualifications for nursing and taking care of offspring, together with the organization and character of that offspring. This last-named point is one of no small moment in the account: it cannot be ignored without invalidating, more or less, the premises or theories upon which it is attempted to base a law of this character. For in all great organic laws or processes of nature, she is universally thoughtful to make provision for the interests of all, so that no one part suffers without due compensation to some other part. In our investigations on this subject, and attempts to arrive at some definite results, many things must therefore be taken into consideration.

In this field of inquiry may be found two extremes. A class of writers, commencing with Malthus, about one hundred years ago, in discussing the subject of population, attempted to define its laws and account for all its changes, without much regard to the structure or agency of the human system. In their view, nearly all the causes that lead to an increase or decrease in population, existed independently of, and outside of the body itself, such as food, climate, government, etc.

According to their theories, man, the most important agent, and factor of all, had but little or no power in directing and controlling these causes. But great changes have of late taken place in the views and theories of writers on population. The teachings of modern science, especially of physiology, have turned the attention of inquirers more and more to the human body, and have led to the conclusion that the laws which governed its existence and continuation, must surely have their origin and support from this source.

Another and much larger class of writers have taken an extreme position in physiology, confining their attention almost wholly to the first principles of life, and the exclusive agency of the generative organs. Aristotle and Harvey may be named as proper representatives of this class. And, since Harvey, nearly all writers on this subject have dwelt chiefly

upon the nature and agency which the reproductive organs alone exercised in the matter of propagation. Very little consideration has ever been given to the relations or agency which other parts of the system, or the whole organism of the body bear to them. In considering the designs of nature in the matter of propagation, it would naturally be expected that all parts of the system must sustain important relations to it, but most medical writers on generation, pregnancy, parturition, etc., have devoted their attention too exclusively to the reproductive organs and the pelvic region. So in that part of physiology represented under gynæcology, or rather obstetrics, writers and teachers have confined their observations to, and discussed the subject principally from, one point of view, viz., the *pelvic region*. However important may be the agency of these organs, they sustain most intimate relations to other parts of the system, and cannot, it is believed, perform their functions completely without calling to their aid, more or less, the assistance of other organs. Thus, in attempting to ascertain and define the highest standard required for propagation, it may be found necessary to consider, not only these organs, whose peculiar province it seems to be, and which are most intimately concerned in the process, but also all other parts of the system; for, as all the members constitute but one whole, and each is linked to the others by a chain of sympathy, if by no stronger tie, so the full development of all must be essential to the perfection and full play of each. Besides, it is not the mere continuation of the race that should be sought, but its improvement and perfection. Moreover, in determining the best type or standard of the physiology of woman for child-bearing, it should constantly be borne in mind that this is her *normal state*—that this harmonizes with her whole organization. The leading features and controlling forces of her organism were evidently intended for this purpose. Not only her physiology, but the history and character of her diseases, her comparative health and longevity, demonstrate that the production of offspring is among the primary objects of her creation. The observance of this law has, moreover, been found absolutely necessary for the most complete development and perfection of woman's organization. All this might be inferred from her physiology alone; but it has been abundantly confirmed by sta-

tistics gathered upon a large scale, and by comparing a large number of married women who have had families, with both married and single women, who never have had offspring. This is also deducible from the fact that, wherever barrenness or sterility exists among married women, it indicates something wrong, some defect in structure or irregularity in function. Whether this arises from imperfection in structure, or some weakness or settled disease, it is considered abnormal; and the greater and more extended these difficulties are, the less likely is recovery, and the wider is their divergence from a true type. In seeking, therefore, after the best and most perfect development of woman, with reference to propagation, all such complaints as accompany barrenness or sterility must be left out of the question.

If, then, the production of offspring is a primary design in the organization of woman, upon what particular type or development, condition or feature of the system is the law found to operate best, or in its highest degree? That there is a difference, a wide difference, in the fecundity of women, must be admitted; a difference, physiologically, in the susceptibility to conception, in the effects of pregnancy, in the ease and safety of delivery, in the physical qualities for nursing, in the constitutional healthiness of offspring. In what, then, does this difference consist? Can it be confined wholly to the reproductive organs or to the pelvic region alone? To settle the question we naturally seek some standard to which we may appeal; and both nature and analogy would lead us to the conclusion that such a standard or model certainly exists somewhere, and that we shall not seek for it in vain. Reasoning, *a priori*, we should naturally infer that it would be found in the highest type or most perfect organization in structure and function; for such is the nature, importance and complication of forces required in propagation, that, for its successful results, it seems to demand the aid of every part of the system. This is certainly the first, the highest, and the most important law in the whole animal economy.

If we study the operations of nature in the framing and government of organic bodies, we never find great principles or laws based upon any particular parts of the system or class of organs, neither upon inferior or imperfect structures. All

the primary laws of nature and the fundamental principles of science, are exemplified in, and illustrated by models of faultless forms and full development. The laws that govern the human system cannot be an exception to this rule. If nature has established such a law of propagation, it is of the highest importance that it be known and understood. While the recognition and knowledge of it would be fraught with the greatest possible interest and benefit to the community at large, it must prove of incalculable value to the medical profession. The law here proposed will be found, we believe, to rest, not upon mere theory or vague speculation, but upon positive facts; and, if so, to lead, in the broadening of our knowledge and our researches to results of a practical and most valuable character.

In considering the foundation of this law, and the advantages to be derived from it, our remarks will be confined to a few points of view only, presenting a meagre outline or brief synopsis of the subject. As the field of inquiry is comparatively new, and but little can be gleaned from medical works bearing directly on the subject, we would bespeak, in its discussion, the charitable consideration of our brethren.

While many facts and arguments may be deduced from the general principles of physiology in favor of such a law, there are four distinct points of view around which they may properly be gathered, and, in this way, be brought out and illustrated in a clear and more forcible manner. These points are: The *pregnant state, parturition, or the mechanism of labor*; the *qualifications* of a nurse, and the *character of offspring*. If there is a general law of propagation, a normal standard in the organization of woman, based upon the principles of physiology, it will certainly aid us in a better understanding and knowledge of those important changes through which she must pass in child-bearing. Let us then briefly review the leading facts or phenomena in each of these changes, and see what lessons they teach.

One of the most eventful and trying changes the human system can possibly pass through, is that of

PREGNANCY.

This state causes many physical changes—some simple and safe, others complicated and occasionally dangerous. The pri-

many changes cannot properly be considered actual disease, but rather functional derangements. In works treating of diseases of women, we generally find a long chapter, headed "Diseases of Pregnancy," discussing from forty to fifty distinct complaints arising from this source. But pregnancy in itself cannot be considered strictly a morbid or diseased state, inasmuch as propagation, in its normal effects, must harmonize with the principles of physiology. Montgomery, one of the most distinguished writers on this subject, makes this significant remark: "If, with a few, pregnancy has deserved the name of a nine months' malady, fully an equal number suffer little or no inconvenience, and with some it is a period of decided improvement in health; moreover, it appears, from all experience, that women who bear children generally enjoy more even health, and are less disposed to disease, than those who lead a life of celibacy, or who, having married, remain unfruitful." Now, why should there be this difference? why should some women suffer so much from the pregnant state—others so little—and others still improve by it in their health permanently? It may be said that this depends upon *difference in constitution*—the *pregnant* state, in one sense, agrees with the constitution of some women, but disagrees with that of others. What, then, is that agreement—what is the type or character of those constitutions with which the pregnant state harmonizes? Is there not some law or standard by which these can be tested or explained? In the very nature of things, there must be, in these things, the observance or violation of law. Such changes cannot come from chance.

Every experienced physician knows full well that there is a great difference in women as to the effects of pregnancy, and that these effects are various and occasionally very marked. Sometimes the change may affect this organ—sometimes that; and again, almost every organ in the system becomes more or less affected. In some cases the very first stage of this change operates unfavorably; it may induce a little nausea or slight headache, or it may result in the most violent inflammation or convulsions. While some women may be benefited in their health from the change, and their constitutions actually improved by child-bearing, with others it is the commencement of suffering and disease, resulting in impaired health and not unfrequently

a broken-down constitution. Now why should there be these differences, why these disturbances? What are the causes, the constitutional weaknesses, the particular predispositions? If propagation is physiologically a *normal* function of woman, why these pathological changes? What laws have been violated? and why should there be such a marked relation or sympathy between this change in the reproductive organs and other parts of the body? The very fact that one organization is found more favorable for child-bearing than another, implies that there may be another still more favorable; and, if so, let this change or improvement be carried to a standard of organization, where the least bad effects possible arise from the pregnant state. What, then, is that standard, and where is it to be found? Is it not based upon perfection of structure and harmony of function; or, in other words, upon the perfect anatomy and physiology of the whole system, and that on this basis is founded a great general law of propagation?

Let us make an application of this principle to different types or kinds of organization. In tracing out the

EFFECTS OF THE PREGNANT STATE,

we find some difficulty, from the fact that by this change in the uterus, it works in three ways—by attraction, by sympathy, and, gradually, by mechanical pressure. One of its effects is to change the circulation and the direction of the nutritive force. Thus, where in certain parts of the system there has been over-action or excessive excitability, perhaps a strong predisposition to, if not the actual existence of disease, the pregnant state, in changing the circulation by withdrawing from these organs a certain amount of blood and nutrition, actually improves the health, and in some instances undoubtedly prolongs life. Here an attempt is made by a natural law to correct weaknesses and restore health, or in other words, to bring about a more even balance or better harmony of action in the whole organization.

There are other cases where the weaknesses or excesses are so great, or the disease has been carried so far, that pregnancy makes the attempt to change this state of organization; and not only fails in so doing, but, perhaps, indirectly aggravates the difficulty. In all those cases, however, where the health

of women is improved by pregnancy, it is accomplished, we believe, by so changing the current of the vital forces of the system as to bring about a more equal circulation, a better balance of organization, as well as harmony of function throughout the whole body. What, then, is the inference, or what lesson does this class of facts teach? Is it not clearly this: that the better and more evenly balanced the structure of the whole body is, and the more perfect the action of its machinery, the less disturbance will be produced by pregnancy, and the less harm or inconvenience result from it?

Let us look at different types of organization. The more nervous and sensitive a person is, the greater and more marked is the effect of pregnancy. In such cases, generally, the change is sooner discovered, and the signs or indications arising from it are more decided and positive. In some cases, where there is a great preponderance of the nervous temperament, pregnancy, by changing and equalizing the action of the nervous system, may improve the general health and constitution. In other cases it may increase and intensify the nervous activity or excitability, and thus affect, more or less, the disposition and temper of the individual. Now and then a case occurs where pregnancy has a singular effect upon a nervous temperament, to disturb and excite the patient, and sometimes even cause mental derangement. In all such cases, if the exact physiology and pathology of the brain and nervous system could be ascertained, we should find some peculiar sympathy in the relations of the nervous system, or some singular idiosyncrasy of organization in these persons. But such cases do not often occur, and are exceptions to the general rule. Such changes of conduct or exhibitions of character do not occur without a cause; and when the cause can be ascertained and satisfactorily explained, instead of conflicting with, or furnishing evidence against general laws or principles, their history and explanation will rather serve to confirm and strengthen the law.

There are many slight disturbances occasioned by pregnancy in the action of the stomach, bowels, heart, lungs and nervous system, which, as far as they prove anything, show a very well balanced organization in those cases, and also that no marked weaknesses, defects or disease existed. But occasionally the stomach is greatly disturbed, which leads to serious and danger-

ous disease. A careful examination into such cases, will show, we believe, a remarkable sympathy or sensitiveness, between the state of the stomach and the action of the uterus or other organs. It may show, too, that the individual had suffered more or less, for a long time previous, from dyspepsia or indigestion.

Sometimes great physical changes are occasioned by the pregnant state: the woman occasionally losing flesh and strength, continues to waste away till she can scarcely go through the regular periods of gestation, the nutrition going mostly to the child, and the whole change being caused by some defective, or unnatural action of the digestive organs; but, more often, the woman gains in flesh and strength, becoming plethoric, and, as it may be said, corpulent. In such cases, the stomach and digestive organs act too vigorously,—manufacture too much nutrition and blood, certainly for the mother, though perhaps at the expense somewhat of the child. This change is decidedly unfavorable, resulting not unfrequently in convulsions or violent inflammation. The causes of such a change are not easy always to understand, but indicate that there must have been some radical defect in the organization, or something wrong in the habits of the individual.

Sometimes the liver and kidneys are so affected by pregnancy as to change the quality of the blood, resulting in anasarca, thereby enhancing the danger of the condition, and sometimes resulting in loss of life. In some cases, it is thought that pregnancy, by sympathy or by some singular influence upon these organs, connected with the process of digestion, produces albuminuria, causing the most dangerous convulsions, and, in some instances, resulting fatally. This disease, whether caused wholly by the pregnant state or not, is one of the most obscure and dangerous of all diseases. It is not easy to trace out its exact relation to pregnancy, or to describe just what pathological changes had taken place in its preliminary stage.

Future researches in pathology will undoubtedly explain these causes and changes—and, we are confident, it will also show, that there were some conditions in the organization, or in the habits and health of the individual, existing prior to pregnancy, predisposing to this disease, so that this state of the sys-

tem operated only as an exciting cause. As yet pathological inquiries have not been carried far enough in this direction ; but, when thoroughly prosecuted, we believe they will show that the sad results of this morbid state or diathesis may be, in a great measure obviated, and, perhaps, show that it is not chargeable to pregnancy alone.

There is another class of complaints, arising from pregnancy, caused by mechanical pressure, interfering with the circulation especially, in the lower extremities ; and sometimes this pressure operates unfavorably upon the natural action of the bowels and stomach, as well as upon the functions of the liver, heart and lungs. This result of the pregnant state cannot well be obviated, or much relieved by any medical treatment, as it arises from a want of proper development of the whole body, or from the too close relations of the internal organs, one to another. The disturbances from this source are more numerous, and their results more serious, we believe, than what are generally supposed.

Again : Is there not a wide difference in the effects of pregnancy as found in different classes, nations and races ? Are they not, as a whole, more marked and serious among the higher classes of society than the lower ; in cities, than rural districts ; and less striking and troublesome still among women living even in a semi-civilized and barbarous state ? In fact, wherever the female organization is the most perfectly developed in all its parts, and the functions of every organ are performed in accordance with its own inherent laws, are not the diseases of pregnancy the least marked and serious ?

Now in all these changes and diseases, a careful investigation will show, that in case there was always a well-balanced organization and a healthy performance of the functions of the internal organs, we should have very few diseases arising from the pregnant state. And all these complaints are found to diminish in number and severity, just in proportion as we find organizations approximating to more perfect standards. If, therefore, propagation is the normal state of woman ; and the more perfect her organization is, anatomically and physiologically, the less are the disturbances or diseases of pregnancy, it certainly points to the fact, or affords evidence that there exists in

nature what may be denominated a great general law of propagation based upon such standards of the system.

For a proper understanding of this law, it is highly important to bear constantly in mind, not only the striking differences in female organization, but to notice particularly the great changes it has undergone in different races, and at successive periods of time. This fact will appear more obvious in the consideration of the next step or process in the development of the law, viz:

PARTURITION.

That there are wide differences among women, in the ease and safety with which they go through this process, all will admit. Now if this process of labor or delivery is natural to woman, is normal physiologically, why is it attended frequently with so much pain and difficulty, and not unfrequently with danger to life? In no other department of the animal economy, where the laws of nature, in a normal state, are observed, do we find such pain, distress, and suffering. Do not these symptoms, therefore, indicate that the laws of organic beings, or the designs of nature, have been in some way violated or perverted? It is true, some women go through the process without much suffering or loss of strength, while to others are allotted nights and days of pain, anguish and distress; and, it would seem that the latter class constitute, at present, the exception to the general rule. Teachers and writers on this subject have taken great pains to ascertain and describe what were the causes of so much difficulty and suffering in parturition, and to inquire what human means or resources of art could be employed to remove these difficulties, and assist nature in this work. To this end the anatomy and physiology of the pelvis have been carefully studied. The relations each part sustains to this process,—what were the precise functions of the uterus,—what should be the presentation of the child,—what obstructions, points of resistance, etc., existed.

No part of the body, probably, has been more carefully studied than that of the female pelvis, and no organs in the whole system perform such important functions as those located in this region. For better understanding and treatment, parturition has been

divided into different stages, and its phenomena classified—such as natural and protracted, tedious and laborious, difficult and complicated labors, etc., etc. Special attention has been given to difficulties attending labor, such as position or wrong presentation of the child, the disproportion between its head and the pelvis of its mother, the imperfect and irregular action of the uterus, the rigidity of the os uteri and the soft parts, the necessity of using instruments, the danger from exhaustion, convulsions, hæmorrhage, etc. These are the points or sources of pain, distress, suffering, and danger. In these eccentric or extreme organizations, the greater are these marked peculiarities of the system, and where we encounter a large part of the difficulties in obstetrics; and the wider these divergences go in any one direction, or the more marked these peculiarities are, the greater these difficulties. On the other hand, the nearer we approach a sound, well-balanced, organization in all its parts, the greater the ease and safety in delivery. Every physician occasionally finds patients that go through this process with comparatively little trouble or difficulty. We find also among women all manner of differences in the process of labor; and these depend mainly upon the kind or type of organization, together with the habits and health of the individual. Now, why these differences, why these peculiarities? Are they not deviations, more or less, from a perfect standard of organization; or in other words, are they not to a great extent abnormal? Are they not the effects or penalties of a law violated, or the result of an artificial—in some respects, an unnatural life?

If a test or direct application of the principles of physiology be here made, it may throw some light upon the subject. Should any class of organs, or some one temperament, such as the nervous, sanguine, or lymphatic, greatly predominate, its effects as a whole will in parturition be found unfavorable. If there is an undue predominance of the nervous system, there will exist far greater sensitiveness or susceptibility to pain; and the process of parturition may produce such a shock upon the brain and nerves, as to render recovery doubtful, if not impossible; if there is an excess of the sanguine temperament, there will probably ensue a greater strain upon the action of the heart, and sudden change in the circulation of the blood, with increased danger of hæmorrhage and inflammation. If

the lymphatic temperament abounds, there is a sluggish state of the system, a lack of force and regularity in the contractions of the uterus, such a deficiency in general vitality and strength, as to render parturition tedious, if not sometimes dangerous from exhaustion. If the muscular tissue greatly predominates in the system, then we find, with violent pains, powerful resistance and rigidity of all the soft parts. It may be, these defects or peculiarities of organization will not show themselves so much in pain and difficulty of delivery, but their effects may become more manifest upon the system afterwards, or upon the character of the offspring.

There is a physiological condition or principle involved in labor or parturition, that is not, we believe, properly considered. We refer to a union or correlation of forces in nature, so that all parts of the system should act in harmony with each other, and in one single direction, when the object to be accomplished requires it. This principle in the study and practice of obstetrics, has been, if we are not mistaken, very much overlooked. Such is the nature and object of propagation in importance and magnitude, that we should expect aid from every part of the system, from every tissue, nerve, tendon and muscle. Parturition is certainly one of the most important and complicated processes in the fulfilment of the law. Now, while certain organs are called on to perform their natural functions, there should be no conflict or resistance from the action of any other part or class of organs. But in an imperfectly developed, and unevenly balanced body, with a want of harmony in the action of all its parts, it is difficult, if not impossible to obtain a union or conjunction of all the forces of nature in the most favorable manner. If the organization of woman, as now found, is a deviation from the normal, perfect standard, it could not be expected that all the forces of nature or the whole organism would aid in the process of parturition, so favorably or to the same extent as they would in a perfectly healthy or normal state. Hence, in considering the causes of pain, the difficulties attending delivery, the force and relations of the whole system should be taken into account, and our attention should not be confined wholly to the pelvic region. It should be borne in mind, too, that we are dealing with imperfect organizations, where a general law cannot be fully applied.

There is another class of facts that have an important bearing upon the subject. It has been remarked that there are not only wide differences among women as to pain and difficulty in parturition, but there are some women, in every community, who suffer comparatively little at child-birth. Now, a careful examination into the structure and functions of the whole organism of such women, we venture to assert, will show few excesses or defects, but, on the contrary, unusually well-balanced, sound, and healthy conditions in every part and organ.

Now let this same principle be borne in mind, as applied to different classes, races, localities, and states of society. It may be difficult to collect here facts upon so large a scale, or to institute such comparisons as would settle any general laws or principles; but still information may, in this way, be gleaned, that will throw much light on the subject. One general fact is very obvious; from medical writers and travellers we learn that woman, living in what is termed a *state of nature*, suffers comparatively but little pain or trouble in parturition; whereas all history testifies that this pain and suffering increases just about in proportion as civilization advances. Thus, in what may be considered a high state of civilization and refinement, not only more pain and distress are attendant on parturition, but increased difficulty and danger.

Among the North American Indians, the inhabitants of Greenland, of Labrador, of the South Sea Islands, and among various classes in South America, of the numerous tribes of Africa and South-Eastern Asia, child-bearing, we are informed, is accompanied at the present day with but little suffering or difficulty. There are undoubtedly individual cases in all these countries attended with distress and danger, but then these are the exceptions. In this general statement we do not deem it necessary to go into details of evidence by giving facts, making quotations from different writers, or furnishing various kinds of evidence. Many writers on obstetrics admit the correctness of these statements; in fact, they are nowhere called in question.¹ Now why should there exist these distinctions or differ-

¹ It is more than probable that pain and difficulty in parturition are artificial, and are the consequences of civilization and refinement. For the human constitution, when not under the influences of these causes, will, *ceteris pari-*

ences in pain, suffering and danger attending a process that is considered a natural, normal condition or function of physiology? In a primitive state of society, among a people living in a plain, simple manner, with habits rude and uncultivated, we find but little distress or trouble attending propagation; but in society advanced in civilization, refinement and culture, we find much difficulty, and not unfrequently danger, attending the fulfilment of this law, and the higher the degree, or the more advanced the state of this civilization, the more painful and hazardous are the chances. The question returns upon us, why this difference? What are its causes? Are they necessary? Can they be explained? Can anything be done to modify or to remove them? The inquiry naturally arises, what is the physiology of women living in the countries referred to, where the law of propagation is so easily complied with? May there not be found among them a better developed physical system more evenly balanced in all its parts or organs, a greater harmony in the performance of all their functions, especially in reference to what may be termed the *primary laws* of nature? Writers admit that there may be found, at the same time, individual cases of women living in these countries subjected to great suffering and difficulty in parturition, and sometimes danger in the process or from its effects. If an examination could be made into those particular cases by a good physiologist, it is presumed that some malformation, some defect in organization,

bus, be found capable of meeting and overcoming without any difficulty the ordinary changes produced by gestation and delivery. Of this abundant proof might be given; for the female savage, wherever found, whether under the scorching heat of an African sun or beneath the rigorous sky of the unfriendly Labrador, brings forth her young without the assistance of an accoucheur or midwife; but the reverse of this almost universally obtain among the females of the civilized world. These differences are most probably occasioned by the changes produced on the human constitution by civilization and refinement.

The mischiefs derived from the sources just mentioned are found to consist in the disposition to or existence of diseases, either general or local or both; in those which may affect the system in general, or those which may be confined to the uterus or pelvis in particular, in the introduction and continuance of certain pernicious customs, habits or modes of life, thereby inducing a preternatural degree of irritability, sensibility, laxity or rigidity—and hence the physical necessity of pain and difficulty in parturition among the greater part of women in a state of civilization and refinement.—Dewees' Essays p. 25.

or wrong presentation of the fœtus, would be found, which might at once explain the difficulty. If sufficient facts could be obtained and comparisons instituted, it would be found, we believe, that it is not in the extremes of savage life or in the lowest stages of barbarism, but among the semi-barbarous or half-civilized, where this law of propagation is most easily or successfully carried out. For there must be certain modes of living, certain states of society, certain types of organization, more favorable to the development of this law than others.

The question might arise, what was the organization of man at his creation? What were the designs, provisions, conditions, etc., with reference to his continuance? Whether we adopt the Scripture account of his creation, or the Darwinian theory, so-called, of his origin, what evidence can there be found that will explain or throw light upon any such general law of propagation? When man was created, according to the Scripture account, there is reason to believe that it was with a perfect anatomical and physiological structure in all its parts or organs, and that there was a perfect harmony in the performance of all their functions. And when the command was enjoined upon the original pair "to be fruitful, to multiply and replenish the earth," the fulfilment of this command, with a perfect organization on the part of the woman, it is presumed was not attended with much pain or difficulty.

But afterwards, in consequence of the disobedience of our first parents, the sacred Scriptures relate that the Almighty said to the woman, "in sorrow shalt thou bring forth thy children." The term *sorrow*, as here used, has received various interpretations. Some writers maintain that it refers exclusively to the mind—to mental acts—such as anxiety, fear, suspense, distress, etc., while others maintain that it implies also physical pain and suffering.

Then, again, the whole transaction is regarded by some as a judgment or curse pronounced upon woman for disobedience, which was to become universal, and continue through all time, without much relief or change: while by others this declaration of the Almighty is interpreted as somewhat conditional in its application,—that sorrow and pain would follow child-bearing, because the laws of the physical system were violated, and that the amount of this sorrow and suffering would depend upon

the manner and extent to which these laws had been violated. This view of the Scripture narrative is the most natural interpretation. It harmonizes not only with the character of God, and our own moral sense of justice, but is confirmed by all the facts of history, as well as the principles of physiology. It implies distinctly that some change would take place in the operation of this law, which would bring sorrow and suffering to woman. It is inferred that there was none or but little trouble of this kind in her primeval state. This change in the law resulted not from an arbitrary or vindictive spirit on the part of the Creator, but depended wholly upon the *violation of physical laws* by human agency, that, just in proportion as man violated the laws of his own being, in the same proportion would there be *sorrow* attending his birth. Thus, in the various changes and deviations from this perfect physiological standard, to which the human body in all ages has been subjected, do we find an endless variety of sorrow, suffering and hardship accompanying child-birth.

If we adopt the evolution or Darwinian theory of the origin of man, the law of propagation proposed in this paper, instead of opposing or conflicting with the doctrines of this theory, we believe very strong evidence in its support may be derived from this source. Without entering into a detailed discussion as to points of evidence, or harmony, we refer only to two topics. The two leading doctrines of this theory are that of "Natural Selection," and the "Law of Variability." Now the principle, or doctrine of natural selection described as applicable to man in a variety of ways, such as the constant "struggle for existence," "the survival of the fittest," etc., corresponds with, in fact, is nothing more nor less than a normal law of propagation. And the doctrine of "variability," or "laws of variation," may be clearly and fully explained by the laws of inheritance. Unless we admit that nature has established some general law of propagation, and set up a normal standard of appeal, it is difficult, if not impossible to understand the almost endless changes taking place in human organization. The laws of variation or inheritance become, then, full of meaning and instruction, and without such a law, or theory, they are to a great extent enigmas.

A careful investigation will show that great changes, or

deviations from a perfect standard, both in structure and function, have taken place, particularly in woman's organization. For illustration, in a high state of refinement and culture, the nervous temperament becomes predominant, thereby greatly increasing the individual sensitiveness to changes, and susceptibility to pain, while at the same time, other parts or organs become so enervated or reduced in vitality, as not to afford the assistance which nature requires in child-birth. Then as to the muscular tissue, so indispensable in such a process, it may become excessive as well as deficient: take individuals or families where, by inheritance and constant exercise, this tissue has become a very prominent portion of the system, great resistance, as well as rigidity will be found in the uterus, and sometimes in the soft parts. Artificial habits continued through several generations may not only have reduced the vital energies of the system, but changed the size and structure of the pelvis itself, so as to interfere seriously with parturition. In what is considered a high state of civilization, there is a large amount of indolence, luxury, false modes of living, injurious styles of dress, and other evil practices, that interfere not only with a natural development and a healthy state of the whole body, but concentrate, in their bad effects particularly, upon the organs in the pelvic region. Much may be done by individuals even in one generation to bring about unfavorable changes in these respects; but when evil habits and injurious practices are continued through several generations, the effects by inheritance become greatly increased and intensified in their form and extent.

There is another point of view from whence important evidence may be gleaned. It is well known that there is a great difference in women as to the amount of prostration produced by the effects of labor, as well as in the length of time and manner of recovery. This depends much on the strength of the constitution, and also on the character of the labor. With some women, the shock is so great, and the exhaustion so excessive, that it requires weeks and sometimes months to recover, and occasionally there are cases which never regain their former strength and health. There are others who go through the process of pregnancy and labor without much exhaustion, or even fatigue, and it is with great

difficulty that they can be confined, after delivery, a week or ten days in bed. And they will go through this process ten, twelve or fifteen times without apparently any injury to health or constitution—in fact, with scarce any loss of time, and not unfrequently, after having a large family, they maintain remarkable health and live to great age. Now, why are there such differences, such exhaustions, such slow recoveries, and, sometimes, permanent injuries of constitution? Why do some women rally so easily and so soon after confinement, and seemingly improve, or at least hold their way, by every repetition of the process? From a careful examination into a large number of such cases we have always found, that such women possessed a remarkably well balanced organization,—not merely good health, devoid of any particular weakness or disease, but a sound body, fully developed in all its parts and organs.

SOME striking facts bearing upon these points may be found in a Report (1871) on the Gynæcology of Iceland, by J. F. Reykjavick, its chief physician.¹ The inhabitants of this island having originated from Scandinavia, Scotland, Ireland and the Western Islands, are represented to possess remarkably sound, healthy bodies, uninjured by the refinements, fashions and luxuries of life. The women are described as of “average height,” “rather tall than stout,” “are generally well proportioned and well formed,” and “have for the most part very regular pelves, although exceptions sometimes occur.” “The women are probably the most fertile of any in Europe,” and “their delivery is in general easy and without danger.” “The most frequent cause of difficulty and protracted labor is the rather common rigidity of the uteri and soft parts.” Instruments “are seldom necessary;” convulsions and puerperal disease, rare; “easy and speedy recoveries” are attributed mainly to a favorable constitution and the forces of nature.

Some marked illustrations on this point of difference in parturition may also be found in an article in this Journal,² by Dr. Joseph Taber Johnson. It was the testimony of Dr. Livingstone, and others, that the women of South Africa “generally suffer less in parturition than Europeans, and, principally, as it appears,

¹ Journal of the Gynæcological Society, Boston, Jan., 1871.

² May Number, 1875.

because they consider it to be an act of nature, and less of disease than do the latter." Dr. Johnson says that this remark of Dr. Livingston "applies almost as forcibly to the negro women in the United States." From the facts and statements presented in this article by Dr. J., it would seem that, in respect to duration of labor, presentation, easy delivery, convalescence, nursing, etc., the negro women have great advantage; in other words, there is found among them much less pain, difficulty, and complication in labor, less call for aid or instruments, less exhaustion, less puerperal disease than among the whites. Says Dr. J.: "Labor seems to progress in these women naturally, and few of them regard themselves as sick, as we understand that word, when they are confined. Their previous modes of life having been so healthful and natural, they have the requisite strength and endurance when the time of need comes. The labor, it seems to me, is normal, the period of convalescence short and uncomplicated, as a consequence of or in proportion to their previous simple and vigorous habits of living."

From remarks made respecting mulattoes, quadroons, and those negro women who have made more progress in refinement, culture and education, though the statement is not positively made, the inference is clearly drawn, that this class experience greater pain and complication of difficulties in parturition.

The differences in size and form of the female pelvis in different nations, and the changes in the form and character of this structure, in the same race through successive generations, from a rude to a highly civilized state, are very important considerations. It is maintained that the foetal head also differs in form and shape; that among a people highly educated the anterior lobes of the brain are larger, and that such change gradually takes place, just in proportion to the advance of civilization. Von Franque, who has perhaps devoted more attention to this subject than any other writer, in accounting for the quick and easy labor in uncivilized nations, says, "we must not forget, in this question, *the influence of culture*, which certainly cannot be estimated too highly; so that, with increase of culture, and super-refinement of customs, not only the most various diseases appear more numerous, but that also, in the

same measure, the labors become more difficult and of longer duration ; that, especially, complications step in, which are conditioned by anomalies of the bony pelvis, and which are in general met with but rarely, almost not at all in uncivilized nations."

While we admit that the changes in the shape and diameters of the pelvis effected by culture, refinement, habits, fashions, etc., of civilization, do greatly increase the difficulties of parturition, may not the changes in other tissues, or parts of the body, from the same causes, increase also these difficulties? The muscular power of the uterus is certainly not dependent upon the size and shape of the pelvis, neither is the strength or power of endurance of the whole body. As the quotation from Von Franque states, "various diseases and other complications"—and may we not add weaknesses too?—have been introduced by these causes, and which greatly increase the pain, difficulty and danger of parturition. In fact, if all these difficulties, including the suffering, exhaustion, hemorrhage, convulsion, puerperal disease, etc., were carefully analyzed, what proportion of these originate solely from the bony structure? While no distinct line can be drawn between a portion of these and their primary cause, yet if a survey of the whole could be correctly made, and their causes defined, we question whether one half of them would be found to arise exclusively from the pelvic bones. But it is in the matter of conception, pregnancy, gestation, lactation, etc., that these changes produce their greatest effects on the physical system, to which allusion will be made at the close of this paper.

It should be borne in mind that the changes here referred to do not grow out of a *true healthy civilization*, but from an *artificial* type, from wrong habits, pernicious customs and fashions, from an *unnatural* culture and refinement, where the laws of health and life are altogether too much violated. It should also be borne in mind that these changes have not been the growth of *one* generation but of *many* ; and thus, by the laws of inheritance, they have become greatly increased and their effects intensified.

Without going further into details on these points, let us sum up what seems to be the general facts upon the subject. It is admitted that there are wide differences among women as

to the amount of pain and difficulty in parturition. It is found that in the ruder portions of society, and among the semi-civilized and semi-barbarous nations, very little pain or trouble, comparatively, is experienced in child-bearing. From the Scripture narrative, we have good reason to believe the organization of woman at creation was such that she suffered little pain from this source; but afterwards a change occurred whereby her liability to pain and suffering was greatly increased. All history shows, that in proportion as the human body has been changed by artificial habits and vicious practices, woman has been subjected to greater and severer pain, as well as difficulty, in child-birth. Facts also show that the further artificial habits, luxuries and fashions are carried, the greater the distress, difficulty and danger in child-birth. Now what lessons do these facts teach? Do they not plainly indicate that there exists somewhere a normal standard, established by physiology for propagation? Do they not teach that the nearer the physical system of woman approaches that standard, the less pain and suffering she endures? If there is, then, such a standard, what is it—in what does it consist? We answer, a well-balanced organization, sound in structure and harmonious in function, where every tissue and organ is found developed to the highest extent that is compatible with the healthy performance of all their functions.

The next stage in the observance of this law is the dependence of the infant for nutrition upon the mother, or, in other words,

THE QUALIFICATIONS OF A GOOD NURSE.

There must be in this respect, between the two, a natural adaption or harmony of relation. According to the laws of nature, when properly observed, we find, that wherever she makes a demand, she is also sure to furnish a supply. Her laws, too, when correctly interpreted are found not only to harmonize with each other, but, are always complete in design—never disjointed or fragmentary. Thus, lactation, in the natural order of things, must follow parturition, as much as that process must necessarily follow the pregnant state. It was evidently intended by the laws of nature that the child, for months at least, should be supplied with nutrition wholly from this

source. No fact in vital statistics is more firmly established than that, in order to save life and promote health, the infant should be nursed at its mother's breast. The ingenuity of nurses and physicians has been taxed to the utmost, the principles of chemistry and the results of experiments have been brought into frequent requisition; but no substitute can be provided equal to pure breast milk. Nature, in her normal state or highest development, we believe, has made ample provision, in the organization of woman, for nursing her offspring. But in order to provide this nourishment pure in quality and abundant in quantity, she must have a well-balanced organization, especially a good development of the lymphatic and sanguine temperaments, together with vigorous and healthy digestive organs. The mammary and other glands should be neither too large nor too small; the powers of mastication, digestion and assimilation must not be deficient, must be equal to the demands which nature makes upon them in this direction. If there is a great predominance of the brain and nervous system, and a constant strain is made upon those parts, thus requiring a large amount of nutrition and exhausting the vitality of the system, there must be a failure in lactation. On the other hand, if the organization of woman partakes too much of the lower animal nature—abounds in flesh—if she is physically large and unduly corpulent—the powers of lactation here fail, the organs of digestion and assimilation may work vigorously, but the nutrition will go to the mother, and not to the child. A careful examination into the physical qualities of women who nurse their offspring best, will show a *natural fitness or adaptation* for this purpose. This same law holds good in the animal creation. There, it has been made a special study for a hundred years or more. There, experiments have been tried without number, and observations made upon the largest scale; no pains or expense have been spared in devising ways and means whereby the best and largest quantity of milk could be obtained from domestic animals for the use of man. But how little interest or attention has been devoted to the subject of obtaining a proper supply of human milk for infantile life? Is not the life of the infant as valuable as that of the adult?

As to this matter of nursing, a variety of opinions has been entertained by different writers. It has long been observed

that there were great differences among women as to their qualifications for nursing ; some furnish an abundance of milk, some only a partial supply, while others are unable to furnish any. Instead of studying into the physiology of women, and inquiring what there was in their organization that made these differences, attention has been devoted almost exclusively to the means of providing an artificial supply. Upon examination into the instructions and directions on this subject, as found in books and lectures, there seems to be something wanting :—the obvious principles or teachings of physiology have not been properly expounded in their application to this function ; neither has it seemed to be considered that the laws which govern, in this respect, the animal creation, are precisely the same as those that govern the human race. In confirmation of our statement, we will make a quotation from an address before a large body of physicians, by a professor of obstetrics and diseases of women in one of the oldest and largest medical schools in the country. Says this professor : “ Why do American born females make such poor wet-nurses compared with the immigrant from Ireland or Germany ? After nearly thirty years of practice I cannot answer the question. That it is the fact, few practitioners in our large towns and cities doubt. Allow that some women with us, as with foreigners, object to being bound to their children’s calls, yet the mass of American females are totally unable to act the wet-nurse with success.” It is not three years since this statement was made and published. This is, we imagine, a more candid comparison than many medical teachers or writers would care to make. But it is the truthfulness of statement, and the explanation offered, to which special attention is here called. Why should there be, in this respect, such difference between American women and the Irish or German immigrant ? Why should New England women of the present day differ from their mothers and grandmothers, who found but little difficulty in nursing their offspring ? Formerly, it was a rare thing in New England for a mother to be obliged to resort to a wet-nurse or to feeding by hand. But now it is certainly within bounds to state, that not half the New England women in cities and large towns can properly nurse their offspring. It has been supposed, however, by some that all or nearly all our American women

can nurse their offspring just as well as not—that the *disposition* only was wanting. But this is found practically a great mistake. While there may be cases, here and there, of this indisposition to nurse, it is a fact, that large numbers who are anxious to nurse, make the attempt, but fail. They find, after repeated attempts, that their milk does not satisfy the child, or that it does not thrive—that there must be deficiency in the quantity or defects in the quality of the nourishment. In many cases, after trying the experiment for weeks or months, they are compelled to give up nursing entirely, while others, depending partly upon nursing, resort also to artificial means for feeding the child. So impressed have writers been on this subject, and also practitioners of medicine, that the nursing of offspring harmonizes with the laws of physiology, and, as a general rule, proves beneficial to the health of the mother, they uniformly advise that the mother should *by all means*, nurse her child. This has always been a favorite theory with obstetricians, and its correctness has been confirmed by the results of experience and observation gathered from all quarters. Such we should expect from the obvious teachings of physiology, and it certainly accords with the common judgment of professional nurses and mothers themselves. But if the principle here laid down is correct, why should nursing be so often attended with pain and difficulty? That there is sometimes a defect in the form of the nipple, and the act of nursing becomes very painful, we easily understand. There are some cases where the act of nursing causes the most painful sensations, extending through the breast to the spine, and, from thence, through almost every part of the body. There are cases, too, where, after a most faithful trial, nursing actually disagrees with a woman and proves, in a variety of ways, unfavorable to her health, so much so that she is compelled to give it up for the preservation of her own life. There are other women, at the same time, with whom it agrees—is found to improve the health through the whole process—that they were never so well as when nursing, even though this process should be repeated from the tenth to the twentieth time. Now, why do we find such difference in the effects of nursing? Why should it ever be attended with pain and difficulty? Why should it injure the health of one and

improve that of another? There must be causes or reasons for those various differences.

Now, while these facts as to the inability for nursing may be found, perhaps, more obvious and common in New England, cases of this kind are not wanting in other portions of our country, both among the immigrant as well as native-born women. Such incapacity has been found to exist, more or less, in all countries, and among all classes and all races. It has not been confined to any age, climate or country, or to any tribe, race or color. But formerly this inability was not so common—occurred only occasionally, and, when partial, did not attract any attention. As long as such cases constituted exceptions to the general practice, they did not create much interest, or lead to remarks or observations on the subject. The same fact is true at the present day in respect to the German, French, English and Irish; a large majority of these women nurse their offspring—those who cannot or do not, constitute the exception. But in New England a gradual change has been taking place: the fact has become more and more apparent that large numbers of women cannot nurse their children, so much so, that in certain localities or classes those who do are beginning to constitute the exception.

The question may very pertinently be asked, why this change? Why this anomalous state of things? Why do we find so many exceptions in the observance of one of the most important functions of the system? What is there here abnormal and unnatural? These inquiries open up the whole question as to what constitutes the physical qualities of a good nurse—not merely in New England, but in all countries, and among all classes and people.

It is evident that this whole matter of the mother's affording proper nutriment to her offspring at birth, and afterwards as long as its nature requires, is governed by some fixed laws. The fact is indisputable, and there can be no question or hazard about it. In the very nature of things these laws must have their foundation and support in physiology. As in other organic functions, so in the secretion of milk, there must exist the requisite organs in good development, and these must have their proper share of aliment and support.

The organs classified particularly under the Lymphatic and

Sanguine Temperament must be not only well developed, but other parts or organs of the system must not be found altogether disproportionate to these. In this case those portions of the body that are predominant require an undue share of nourishment, if it should happen to be the nervous system, and particularly the brain—as this tissue requires relatively a much larger proportion of nutriment than any other—such an organization would be poorly fitted to afford proper aliment for offspring. The more carefully all the physiological developments or conditions requisite for a good nurse are investigated, the more convinced we shall be that they depend not merely upon what may be considered a sound and healthy body, but upon one well-balanced, evenly developed in all its parts. As far, then, as lactation is concerned, this type must be considered its normal standard.

That the human body has undergone changes from time to time, all will readily admit. Many of these changes, occasioned by the artificial habits of life as well as by the fashions of the day, are found not only unfavorable to female health, but must prove decidedly injurious to the race. Nearly forty years ago Sir Astley Cooper made this statement: "It is melancholy to reflect that a life of high civilization and refinement renders the female less able to bear the shock of parturition; it has a tendency to lessen her attention to her offspring and really diminishes her power of affording it nourishment, so that she is often a worse mother in these respects than the female of the middle ranks of life, or even the meanest cottager." This remark was undoubtedly made as the result of extended observation and long experience many years ago; and it implies not merely a change of disposition, but also a change in organization, from the fact that such mothers could not properly nurse their offspring. Sir Astley Cooper observes that the proper development of the mammary glands is often prevented by a constant pressure. We might go further, and say that continued compression of the chest and abdomen is calculated to impair the development and healthy action of the lungs, the heart and digestive organs, as well as those in the pelvis.

If we consider that this compression commences with the girl or young woman, when the system is in a state of growth and most susceptible of change—that it may be continued for a

series of years, and, by the laws of inheritance, intensified; it shows very clearly how such effects upon the system disqualify women for some of the most important duties of maternity. A great variety of causes, other than those here stated, might be adduced to account for physical changes of constitution, or changes which might especially interfere with the lacteal functions. Among these causes may be mentioned, educational pressure, constant excitement, depression of spirits, too much society, hard work, great exhaustion, etc.

In the matter of nursing much depends upon the daily habits of the individual, the kinds and quantity of food consumed, the nature of drinks taken, etc. While these agencies have, for the time being, a marked influence upon lactation, it is the particular type or standard of organization most favorable to nursing, that constitutes the present object of our inquiry. We have stated that in the matter of nursing, there was a great difference between the women of New England at the present day and the early settlers. That there has been here a decided change in female organization within fifty or a hundred years, there can be no question. Formerly, there was more muscle, a larger frame, greater fulness of form, and a better development of all those organs that are classed under the sanguine and lymphatic temperaments. The brain and nervous system relatively were not especially predominant; neither were they taxed continuously or excessively above any other class of organs. Those of the Germans, English and Irish who best nurse their offspring at the present day, possess an organization similar to the one here described. If an inquiry could be thoroughly prosecuted in any tribe, race or people, and the individuals or classes that were found most successful in nursing their offspring could be picked out, we should find that they possessed an organization much alike, and not dissimilar to the one already described.

There is another point worthy of notice. In all medical works treating of nursing we find very minute descriptions of physical qualities requisite for a good wet-nurse. Certain conditions are insisted upon as indispensable, such as well developed mammary glands, strong digestive organs, good health, freedom from diseases, or any particular weakness; she must be neither too thin and spare, nor too fleshy and corpulent; the

nervous temperament is described by several writers as particularly unfavorable. We find a similarity, a correspondence in qualities everywhere described—nowhere opposite or contradictory qualities. In fact, if we should quote the various descriptions or directions given for selecting a suitable wet-nurse, from different writers, in their own language, we should find that they correspond almost precisely with that normal standard of organization upon which we believe the law of propagation is based.

The evidence derived from this source is valuable for two reasons : *first*, these writers have drawn those descriptions (of what constitutes a good “wet-nurse”) from their own experience and observation, without any theory of their own, or any design of contributing evidence to establish a general law ; and *secondly*, these descriptions of what constitutes a good wet-nurse come from a large number of medical writers of diverse character, living in different countries and writing at different periods. Such a remarkable agreement or uniformity in all their statements shows, that the great facts or truths of science wherever carefully studied and collected, not only harmonize with each other, but must have a basis or foundation in the primary laws of nature. And further, in regard to the matter of nursing or affording natural support to the infant, it should be carefully observed, that it bears most intimate relations to other laws. As the laws of nature come to be more correctly and fully understood, we always discover a natural harmony, consistency, or adaptation to specific ends. Scarcely any truth or general principle is more firmly established, than that where nature makes a demand, she invariably furnishes a supply, and *vice versa*. The existence and character of the one presuppose that of the other. There may, it is true, be grades or different degrees, in the matter of demand and supply : but wherever the supply is the most ample or pure, the inference or indication is clearly manifest, that it points to where the law of demand, in its best estate, has its basis and support. The natural necessary inference then is, that the organization which is found best adapted to afford proper nutriment to the infant, must be the best for its production ; or, in other words, must be regarded as the true physiological or normal standard upon which is engrafted a general law of propagation. The con-

ditions best calculated or indispensable to support life must exist necessarily in the organization that produces it. This is a universal law of nature, supported by all experience and observation. Let us repeat it: the physiological conditions in nature found necessary for furnishing the proper nutriment for its productions, must also constitute the same standard of organization upon which nature, in her normal state or highest development, has established the law of production. If, then, all the conditions or qualifications of a good nurse in the best or highest state, are brought together, they furnish virtually the physiological or normal standard of woman for propagation.

The fourth topic for consideration is the

CHARACTER OF OFFSPRING.

While this might be considered a sequel or consequence of the former conditions, arguments may be deduced from this source also to establish the doctrine already laid down. It is scarcely necessary to state, that wonderful differences exist at birth in the physical qualities or constitution of the infant,—that many are born into the world with the seeds of disease, with weaknesses, imperfections, deficient vitality, organs poorly balanced, etc., etc.,—while others inherit a sound healthy constitution,—free, comparatively, from weakness or any actual predisposition to disease, with an organization adapted to enjoy good health and long life? Now what makes this difference? Why are some children born with feeble and diseased bodies, or predisposed to disease and premature death? Why, in civilized society should nearly one-third of all infants die the first year of their existence, and almost one-half under five of years age? Is there not something *abnormal*, *unnatural*, in such mortality of infantile life? How, on the other hand, does it happen that large numbers are born into the world with strong, vigorous and healthy bodies, scarcely ever subjected to disease or suffering, and live till they die from old age? Now, what makes the difference in these two classes? Evidently the difference in the physical stamina or constitutions of the parents. Which, then, of these two classes harmonizes best with the laws of physiology in its normal state? Most clearly the latter class.

The question naturally arises, then, as far as the character of offspring is concerned, upon what type or feature of physiology, should we expect to find a general law of propagation based? Would it not be upon one sound, well-balanced and healthy in all its parts and functions, instead of one imperfect and deranged, possessing the seeds of disease and decay? Such an inference surely accords not only with all our experience and observation, but with the established laws in the orders of the lower animal economy. All the primary laws of nature, or the fundamental principles of science, have their start from, and foundation upon perfect standards. The laws that govern the human system cannot be an exception to this general rule.

There is another view that may be taken from this point. Wherever in nature, we find derangements, imperfections, the seeds of disease, decay and destruction, does it not clearly indicate that some laws have been violated, that there have been deviations from a more perfect standard, or, in other words, that such a state is abnormal, unnatural? As we study the present developments of human nature, we find not only a vast amount of pain and difficulty attending pregnancy and parturition, but that pain, disease and premature decay follow their production. These, too, we find, are to a great extent the common, uniform results and not exceptions to a general rule. A careful review of all the facts connected with the state and organization of infants at birth, with the nature and character of their diseases, together with the early decay and premature deaths of so many, all go to show that if there is a general law of propagation, it is certainly not based upon present standards or models.

This topic—*character of offspring*—might be greatly enlarged upon, as connected with the law of propagation, and the designs of nature. It has been well remarked that the two strongest instincts of man are, 1st, that of preserving life, and 2d, that of transmitting life to others. Now, if nature has established some general law for this purpose, as she undoubtedly has, it should result in the highest development of offspring. It should produce sound, healthy structures, and not an organization, impregnated so much from its very origin with the seeds of disease and premature decay. It is unnecessary here to follow out the argument, that in order to perpetuate the

race as it should be, there must be sound and healthy stock. There is no one thing so important at the present day in the progress of the race, or in the advancement of civilization, as that greater attention should be paid to the observance of this law.

We have now passed in review four distinct points or topics, viz.: pregnancy, parturition, lactation and offspring, which constitute the leading stages, or more prominent events connected with the law of propagation. The facts and inferences gathered from each of these sources, all go, we believe, to show that there exists in physiology a normal standard for this purpose. Now, if by gathering up all the facts and indications to be found in each of these stages or events, we find them all in the main pointing in one direction,—all agreeing with each other, and aiming at the same result, it certainly strengthens the argument, and affords an accumulation of evidence on the subject. As the four heads above mentioned seem to cover the whole ground, if not a single conflicting fact or argument can be gathered from any one of these sources—particularly when the four heads are brought together—it furnishes strong evidence in favor of a general law of propagation.

Several reflections naturally grow out of the present discussion. The subject is altogether too large and complicated to be unfolded in one short paper; all that can possibly be attempted at the present time, is to present a few thoughts and suggestions upon a thesis that would require volumes for a full and thorough discussion. Inasmuch as this essay opens new views on questions which are obscure in their nature, far-reaching in extent, and upon some of which there has long existed a great variety of opinions, the sentiments here advanced should not be judged of hastily; we could wish that no preconceived opinion or prejudice should be allowed to interfere with their calm and dispassionate consideration. The only just and fair method of testing their correctness or falsity is by some definite knowledge of the subject—a knowledge obtained from the study of nature, and the deductions of facts, collected from one's own experience and observation.

The process, by which many of the leading principles of science were first established, has been slow, and attended with opposition and difficulty. The more radical these prin-

ciples were, and the more sweeping in their application, the greater the contention and the strife, and the slower their growth. But, whenever in the history of science any theories or principles had a sure basis in nature, though they might for a time be opposed and be controverted, they were sure ultimately to prevail.

So in reference to the doctrines contained in the present paper, if they constitute a part and parcel of the laws of physiology, opposition and prejudice will in time give way, and their truth and worth will come to be universally acknowledged. In fact, the history of medicine furnishes instances of new discoveries or modes of practice, which, on their first promulgation or introduction, were bitterly opposed and even ridiculed, whose truth and value came in time to be admitted, and which are now acknowledged according to their real worth and importance.

If the views presented in this paper are true, any candid person acquainted at all with the laws of physiology or the principles of medicine, must admit that they are of priceless value. For illustration: In all studies, whether of nature, science or art, there are great advantages in having leading principles or fixed standards to guide us in our inquiries, and present beacon lights in every direction. If, while investigating the facts connected with propagation, such as the complaints of pregnancy, the difficulties of parturition and infantile diseases, we can more clearly understand their causes and what particular laws have been violated, it must afford immense satisfaction, and might, perhaps, enable us to devise new means or agencies for relief. It will show what types or models of female organization are best adapted for propagation, most exempt from pain and trouble, best qualified to nurse their offspring and transmit a sound, healthy stock. It will throw new light on the laws of inheritance, explaining changes which the body may have undergone in past generations, and suggesting what are some of the most fruitful sources of improvement. When we have formed a just conception of the original or normal standard of human nature, according to physiology, and keeping this standard constantly in view—when we see the endless deviations from it, and find that these changes have all taken place in accordance with the laws of inheritance,—then we begin to real-

ize their power, value and importance. Within a few years great interest has arisen in reference to these laws, and inquiries are being pushed in every direction for more light, more instruction in relation to them. No one thing will infuse such interest into these inquiries and furnish so valuable a guide, as the fact that there is fixed in physiology a normal standard of propagation, from which all these laws emanate, and around which they all cluster. In fact, in order to understand correctly those hereditary influences, and trace them out in all their bearings, some such chart or guide is indispensable. For, in default of a standard or of some general principles to guide us, the powerful agencies of heredity cannot be fully comprehended or accurately defined, or judiciously and advantageously applied. Nor does social life, or life in any of its phases, constrain or invoke attention to any sources or agencies affecting the well-being of mankind, at least physically, which operate for good or evil, more powerfully than the laws of inheritance. In fact, while it is impossible to estimate the advantages of these laws when applied to human improvement, their value and application must always be limited, unless we have a perfect normal standard as a guide.

Another reflection connected with this subject is that in attempting to account for the sufferings and difficulties attending child-bearing, and finding that they arise in a great measure from changes in the human body brought about by artificial life and the violation of physical laws, the inquiry naturally arises, what are their remedies? What can be done to relieve or remove them? While we cannot easily or hastily reform the present artificial state of society, or improve the physical developments of the human body, as it would require several generations to make any radical changes in this direction, yet, by understanding the true causes or sources of these pains and difficulties, it may enable us to give instructions or exercise an influence that, in process of time, will tend to improve or modify these agencies, including the laws of inheritance. Inasmuch as all sanitary agencies, such as regular exercise, wholesome food, pure air, good sleep—in fact, every influence, mental and physical, which is calculated to improve the general health of woman—these should all be encouraged as tending to, nay, as essential to the realization of nature's plan and

design. The more perfect the health of woman is, the more evenly balanced her organization, the fewer weaknesses and predispositions to disease will occur, the better is she prepared for the pregnant state, for the process of labor and the duties of maternity. All preparations or treatment that are calculated in any way to bring about a normal standard of womanhood, should by all means be encouraged and brought into requisition. There is no doubt but that much has been and may be done in this way to prepare the system for these changes, and that more or less suffering, disease and danger connected with child-bearing arises from the want of such precautions.

It should, however, be borne in mind that inasmuch as most of these causes of pain and difficulty are the results of violated laws, extending back for several generations, they cannot at once be removed, and the idea that we can have "*parturition without pain*" (as is claimed by some reformers)—especially in the present state of society—is perfectly preposterous. No such desirable boon can be obtained by any "course of diet" or "rules of hydropathy." It has been advocated by some that if the pregnant woman subsisted upon food entirely free from phosphate of lime, the osseous portion of the infant—especially the skull—will become very much modified in hardness—thus making its passage through the pelvis much easier. While in some instances experiments of this kind may have proved apparently successful, in other cases, they have not been attended with the same result, and as to offspring, we believe such a course of diet must prove decidedly injurious.

But attention has not alone been confined to this kind of preparation and treatment: expedients, in great numbers and variety, such as anæsthetics, medicines, instruments, etc., have been resorted to, in order to relieve the pain and difficulties of child-birth. This is all well, but these are all artificial helps, relieving only for the time being; how much better is it to go further back, and remove, if possible, first causes? Let us understand the particular changes in certain parts of the body, from a normal to an abnormal state, whereby the sufferings and dangers of child-birth have been greatly enhanced; let the laws of inheritance be correctly taught, and our duties in relation to them be properly enjoined; let these laws be

generally observed—and, in the course of a few generations, we should undoubtedly perceive a great diminution of these evils, and the condition of woman greatly ameliorated in child-birth.

One of the most beneficent features—and we might say, almost the leading object—of the medical profession, is the relief of pain—the amelioration of human suffering. Whether, under medical treatment, disease can be cured and life prolonged, or not—*one thing* is certain, pain and suffering, in all cases, can be more or less relieved.

While hitherto, in medical practice, *cure* has been the watchword of the profession, let, hereafter, another term, expressing a higher if not a nobler object, stand alongside of it—that is, *prevention*.

A gold medal was very properly awarded lately in London to the writer of an essay on “The Therapeutical Means for the Relief of Pain;” but a richer and more enduring reward, in the thanks of great multitudes, awaits the advanced guard of the medical profession who are laboring to expound sanitary laws, and diffuse a knowledge of hygiene for the *prevention of pain*. So in obstetric practice: while the most protracted study and greatest skill and ingenuity have for years been exhausted in devising means to relieve pain, and save human life, in the most critical periods of woman’s existence, let us turn our attention more to the *primary causes* of this suffering and danger, and earnestly inquire for *preventive* as well as *curative* treatment. Let us fully realize that, if a normal standard of physiology generally prevailed, if its principles and developments were perfectly exemplified at the present day in the human system, woman would suffer comparatively but little pain or danger at such periods.

In bringing this paper to a close, we wish to enlist in its behalf the attention particularly of that portion of the profession engaged in *obstetric practice and teaching*. The history of physiology and pathology, in reference to the diseases of women, together with the improvements in obstetrics, should prepare the mind of every practitioner of medicine for new discoveries or suggestions in this direction. In no other department can the correctness or advantages of the views here presented be so well tested as here—by individual experience and observation. Let every obstetric case be tested by the standard

of organization here described ; let the changes through which the system of each woman passes in pregnancy, parturition and lactation, be compared with the doctrines set forth in this paper, and let the results of such observations in a large number of cases be brought together, analyzed and compared ; then from these data some correct and rational conclusions can be deduced.

Partly by way of illustrating the principles of physiology, as here set forth, but more particularly to show their bearings on population, we present a few facts which first directed our attention to this subject. Many years since, while attending obstetric cases in a mixed population, including English, Irish, Scotch, German, French and American, we were struck with the great differences in the fertility of these women, as well as in the difficulties accompanying parturition. Comparisons were instituted, treatises on obstetrics, works on population, census returns, etc., were examined, and then changes in population. It was found by an examination of town records, genealogical histories, registration reports, etc., that the birth-rate in New England had steadily diminished in every generation, but more rapidly in the last one or two ; that, commencing with an average of eight or more children in each family, it averaged at the present time in Massachusetts only about three ; and that with the strictly American the birth-rate and the death-rate were approximating nearer and nearer together. If one-third of those born die before reaching adult life, and a portion of the living never enter the marriage relation, the question arose, in view of such a record, what will be the result on the increase or decrease of population ? With such a diminution of numbers among the native American, and an increase of offspring in the foreign element of more than double, what could make this change and this difference ? How can we account for such anomalies ? On the other hand, a careful examination disclosed great differences in the organization of these women, which might account in some measure for such a state of things ; the causes of these differences were analyzed as far as practicable, and the relations which they might sustain to an increase of population. It was found that a great change had taken place in the physiology of our New England women, that there had been a marked loss of muscular power

and a great increase of the nervous temperament; that weaknesses, diseases or complaints of one kind and another had greatly increased and become so common, that good health and sound constitutions were the exceptions.

Again, uterine diseases and complaints were found particularly to have greatly multiplied, for which there must be special causes. Among these the compression of the chest and abdomen, from the style of dress and fashions of the day—commenced early and long continued—had had a very injurious effect upon the internal organs, especially upon those in the pelvis, thereby causing displacements, weaknesses and diseases. With a general loss of muscular strength and an increase of nerve tissue, aggravated, too, by other causes, the reproductive organs had become abnormal, incapacitated to perform healthy functions, which, of course, must seriously interfere with conception, gestation and parturition. Add to this a constant strain upon the brain and nervous system, requiring relatively too large an amount of nutrition, we have as a result an undue predominance of the nervous temperament, which in some of its aspects is decidedly unfavorable to the domestic relations, as well as to the laws of maternity. Thus it will be seen that, as far as physiology is concerned, the true law of population must be based upon the same general principles as that of propagation, and that a correct knowledge of the former cannot be obtained without reference to the latter.

PERI-NEPHRITIC ABSCESS IN CHILDREN, WITH A REPORT OF NINE CASES.

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UNDER the impression that the subject which forms the caption of my paper may not be uninteresting to those chiefly interested in diseases of children, as well as to orthopædists, I gladly avail myself of the opportunity offered me for presenting the histories of the following nine cases

of peri-nephritis, seven of which have terminated in abscess, and two in resolution. It being the rule for this affection to terminate in suppuration, I have chosen the above title, and shall class all my cases, for convenience' sake, under that head.

While Rayer, Trousseau, Bowditch, Rosenstein, and Ebstein have written ably on peri-nephritis and peri-nephritic abscess, only one of these observers (our own countryman) has met with the disease in children. All have dealt with the subject as related to adult life, and consequently have had no occasion, as a rule, to differentiate this from incipient caries of the spine or the second stage of morbus coxarius. The interest in joint diseases, I am sure, is extending; and I feel that if I can contribute anything to their diagnosis even, I shall be abundantly recompensed. The difficulty of a differential diagnosis is not an imaginary one, as many of our brilliant lights might intimate. I have seen cases in which the exceedingly clever failed to make a correct diagnosis, and hence I feel that I may be pardoned for committing errors myself. That which galls most bitterly is the fact that, in at least one-half of my own cases, I failed to diagnosticate that which subsequent developments proved to be the true lesion.

At this point I am tempted to digress, and devote a paragraph to that charity of opinion which a study of the cases herein reported has brought more clearly to my view. We are so prone—arrogant creatures that we are—in the examination of a patient, to impute ignorance and stupidity to the doctor last in attendance, that we wonder how it was possible to overlook a disease marked by such indubitable signs; forgetting all the while, that time, more or less long, has elapsed since our brother saw the case; that in the interval a mole-hill may have grown into a mountain; that the testimony given by an ignorant mother may vary according to circumstances, according to the leading questions propounded. We forget that every prudent physician meets with cases which compel him to reserve his diagnosis until the clearing up of certain obscure symptoms. This by the way.

With reference to the rarity of the disease in adult life, authors are not unanimous. Trousseau,¹ in a classical chapter

¹ Clin. Medicine, Eng. Translation, vol. ii., p. 891.

on the subject, regards it as relatively rare; stating, furthermore, that the nature of the cases is liable to escape notice. Niemeyer¹ regards it as a rare affection. Flint, in the fourth edition of his work on practice, devotes three-fourths of a page to an analysis of three cases reported by Bowditch.² A few of the works on surgery give a passing notice. In referring to ten works on diseases of children, I find no space given to its consideration—in fact, scarcely a reference. In a few of the papers on the subject, as collected from current literature, two or three cases are reported as occurring in children. In twenty-one cases collected by Halle,³ in 1833, he found one in a patient sixteen years of age. Rosenstein,⁴ writing in 1870, says that, up to the present time, peri-nephritis has not been observed in childhood. During the same year, Bowditch,⁵ of Boston, reports seven new cases and a resumé of three old ones, and among the seven are two occurring in children. In Ziemssen's "Cyclopædia of Practical Medicine," Ebstein,⁶ writing as late as 1875, seems to have completely overlooked Dr. Bowditch's excellent article, and makes the statement that only one case of peri-nephritis in childhood—Löb's case—has been reported. I am unable to find any since that publication, and the number on record is only three, one of which may not have been primitive. Of course I exclude all those of a secondary nature.

A brief reference to the anatomy of the parts will refresh the memory, and help us better to understand the deformities that may arise, the course a quantity of pus may take, and the pathological changes incident to the neighboring parts. The kidney lies imbedded in several layers of cellulo-adipose tissue, which are in relation anteriorly with the ascending or descending colon, posteriorly with the crura of the diaphragm, and continuous with the cellular tissue of all the organs in this region. The most important relation, however, is with the iliac fascia investing the psoas and iliacus muscles, and the anterior lamella

¹ Practical Medicine, vol. ii., p. 40.

² Bost. Med. and Surg. Journal, July 9, 1868.

³ Des Phlegmons Peri-néphrétiques, Thèse soutenue le Août, 1863.

⁴ Nierenkrankheiten, 2, Auflage, 1870, s. 268.

⁵ First Med. and Surg. Report of the Boston City Hospital, 1870.

⁶ Band, xi., s. 44.

⁷ Jahrb. f., Kinderheilkunde Neue Folge, viii., s. 197.

of the aponenrosis of the transversalis. On the right side, the right lobe of the liver posteriorly impinges upon the peri-nephritic tissue, and the proximity of the gall-bladder serves to anatomically explain certain icteric symptoms observed in some cases. On the left side, the cardiac extremity of the stomach, the lower end of the spleen, and a portion of the pancreas participate in this relationship. Mr. Moxon, in a clinical lecture reported in the *Lancet*,¹ calls especial attention to the contiguity of the sheaths of the vena cava and aorta, and he regards the flatulency and irregular constipation, so marked in the case before the class, as symptoms of paralysis of the abdominal sympathetic, caused by an extension of the thickening process to the sheaths in question.

Before proceeding to the etiology, symptomatology, *et cætera*, I shall present the histories of my cases, an analysis of which will enable me the better to treat of these divisions of the subject.

CASE I.—M. O'B., female, æt. 8 years, was admitted to the hospital, July 16, 1873, with the following history: Health very good prior to June 1st; shortly thereafter, without any known cause, she began to complain of pain in the left knee, subsequently referred to the hip, where the pain became very acute; rapid emaciation, with frequent febrile exacerbations, marked the course of the disease up to the date of admission, when her expression was indicative of great suffering, and the utmost care in preparing her for examination failed to avert the piercing cries which the least motion would occasion. The tongue was furred, pulse 150, temperature high, though not measured with the thermometer. To stand or walk, or even lie with comfort, was out of the question. There was considerable tympanites and undue prominence of the superficial abdominal veins; enormous enlargement of left gluteal region, extending upward beyond the crest of the ilium, normal contour completely effaced; integumental covering elevated in temperature, while palpation thereover gave distinct fluctuation. The thigh was semiflexed on pelvis, and the least attempt at extension gave such pain that further examination had to be discontinued. A diagnosis of aente synovitis of the hip-joint was made. For three days the child refused food, and the pain was relieved

¹ *Lancet*, vol. 1, 1875, p. 602.

by opiates. On the fourth day, the appetite returned, and she ate ravenously. The abscess was not opened, but continued to enlarge, burrowing along the spinal column and down the thigh, relief being afforded August 7th by a spontaneous opening. The discharge of fetid pus was amazingly profuse. Tonics and stimulants were administered unsparingly. August 13th: the abscess threatened a spontaneous opening in the lumbar region; compresses were applied, and succeeded in forcing the pus through the lower opening. August 24th: the discharge had greatly decreased, and the child was going about the ward improving daily. August 26th: fell, rupturing a small artery near the opening of the abscess, and about one half-pint of blood was lost before I could reach the patient. A compress quickly checked all hæmorrhage, and there was no recurrence. From this date the purulent discharge increased, draining from a large sac over the sacrum. During the month of September at least one pint of pus daily flowed from two small openings. In October it became less profuse, and toward the close of November ceased altogether. She was considered well; the limb was in normal position, and there was no impediment to motion in any direction. We realized our error in diagnosis, and felt convinced that the abscess must have originated near the origin of the psoas muscle, making it peri-nephritic. The urine was examined once shortly after admission, and nothing found except an excess of urates. The patient remained six weeks longer, that convalescence might proceed unmolested. Her recovery was perfect.

CASE II.—T. A., male, æt. $1\frac{1}{2}$ years, a recent arrival from Denmark, was brought to the out-door department, March 12th, 1874, presenting a mere fulness in the right lumbar space. The thigh was flexed on pelvis at an angle a little less than 90° , and extension was painful. There was a marked elevation of temperature, a hectic flush, and other signs of some acute disease. The liver was of normal size, abdomen large, spinal column presented no angular prominence. The excessive irritability of the child made the testimony derived from any localized tenderness valueless, and prevented a satisfactory examination. Aided by an interpreter, I learned from the mother that four weeks prior to this visit the patient enjoyed perfect health; that without any apparent cause he became very cross, and had daily

febrile exacerbations, refused food, and had been losing flesh. She had first noticed the lumbar enlargement three weeks after the first signs of indisposition. From the history, then, and the signs already observed, I diagnosticated a deep-seated lumbar abscess, unconnected with carious vertebræ. The urine was not examined. Dr. Knight confirmed my diagnosis, and suggested measures to promote resolution. Within a week the little patient returned, and the tumor filled the whole costo-iliac space, pointing on a line with axilla and crest of ilium. In my haste to make an incision I neglected to notice whether the flexion of the thigh had increased or diminished. A small incision gave exit to about one pint of pus not particularly offensive. Stimulants and tonics were ordered. At the next visit it was evident that great relief had been afforded, and that improvement had energetically commenced. In a month's time all discharge had ceased, and the patient was discharged cured.

CASE III.—M. T., female, æt. 6 years, admitted to the hospital July 21st, 1874. The history, as obtained from an intelligent mother, represents the child as in excellent health two weeks before date of application. She complained first of pain in the right lumbar region, and very soon afterwards it was referred to the neighborhood of the hip. A marked limp was observed, and the thigh was flexed. These symptoms were progressive. In looking over the history as recorded, I find no mention made of any cause, or of the occurrence of any fever, during the two weeks just mentioned. On entry, her general appearance was good, the tongue pale and flabby, the vital signs about normal. While standing, the body was inclined forward and to the right; while walking, the limp was marked. Fulness could be detected in lumbar region, not especially confined to one or the other side; there was, however, great tenderness on pressure in the right loin; while the thigh was semi-flexed, there was no perceptible rotation outward, complete extension impossible, no pain on flexion beyond an angle of 90° ; absence of tenderness in groin and over trochanter. The diagnosis was reserved, and the treatment was expectant. September 8th: contraction of psoas less, while there is a suspicious fulness above the crest of right ilium, palpation over which gives no satisfactory fluctuation. Tr. iodine externally,

and the roller applied with cotton batting as compress. By October 9th, the fulness had entirely disappeared; not a shade of difference could be detected in the appearance of the loins, hips, or thighs; the child stood erect, and walked without a limp. Precautionally, she was retained until December 19th, when an examination, pursuant to discharge, failed to detect any disease or deformity.

CASE IV.—M. H., female, æt. 9 years, was seen by Dr. Knight, June 11, 1875. The general appearance indicated prostration from some acute disease. When placed on the sofa the thighs were flexed on abdomen, and an attempt to remove the clothing failed, so acute the pain. The mother endeavored to hold the little sufferer as nearly erect as possible, in order that a view of the spinal column and nates might be obtained, and for a few moments this was tolerated. In this position no deviation of the spinal column in any direction was observed; the natis of right side was flattened, while the thigh was strongly flexed. Caries of the spine and hip-joint disease were excluded. Further examination was postponed. Tonics and extra diet were ordered, and Dr. Josselyn, one of my associates in the hospital, was requested to visit the patient at the house, her condition making attendance as an outdoor patient impracticable. This was during my absence from the city, but Dr. J. has kindly furnished me with full notes of the case. Her general health was never extra, though comparatively good until three months ago, when she began to walk lame, failing to bring the heel of right foot to the floor. Coincident with this there was pain in the right inguinal region. No exciting cause could be obtained. Two physicians were called in, morbus coxarius was diagnosticated, and a Buck's extension applied. No improvement followed, but the high fever, emaciation, and acute pain became alarming. On the third day following her visit to the hospital, Dr. J. discovered a circumscribed swelling just to the right of the lumbar vertebræ, but could not detect any fluctuation. Within a few days a valvular incision was made, through which a large quantity of pure fetorless pus was evacuated. The hectic subsided almost immediately. By the next day the wound had healed by first intention. Improvement continued until the middle of July, when the hectic returned. On the 21st a free incision was made by Dr. Har-

lan, to whose care the case had been finally committed. In less than three weeks she was cured. I saw the child quite recently, February 12, 1876, and found her in perfect health. There had been no relapse.

CASE V.—B. J., male, æt. $3\frac{1}{2}$ years, was brought for treatment, June 21, 1875. The father bore him in his arms with the utmost care, a momentary neglect of which would cause the little fellow to shriek piteously. On removing all the clothing, the body was found emaciated, hot to the touch, the spinal column preternaturally immobile, though no angular prominence or incurvation could be detected. No tenderness over the spinous processes. He was induced to stand a few moments, and as he stood the right thigh was advanced, the body bent forward, and the hands rested on the knees. Examination of the thigh revealed nothing save contraction of the psoas and pain on extension. There was apparent hyperæsthesia over the right side of the body, and I could not use sufficient pressure to detect any deep-seated tumefaction; none existed superficially. History briefly: good health prior to April 1st; shortly thereafter an acute bronchitis, convalescence from which began in the middle of May; an apparent relapse toward the latter part of same month, the prominent symptoms being excessive peevishness, progressive loss of flesh, moderate jaundice, fever remittent in character, pain referred to chest and back, a disposition when standing to stoop forward and rest hands on knees, at times complaining of sudden sharp pain, which induces him to run to the nearest chair or sofa, preferably the mother's lap; when in bed, to assume the prone position; bowels constipated; nothing abnormal perceived in the gross appearances of the urine. In the early part of June there was semiflexion of the thigh and a marked limp. Such was his history, and I confess my inability to reach an absolute diagnosis. Morbus coxarius I excluded, and while I gravely doubted the existence of caries of the vertebræ, as a precaution I ordered a spinal brace, and directed the mother to report within a few days should any new symptoms develop. Accordingly, on June 28th she returned, reporting a deposit of sediment in the urine after it had stood awhile, an increase of the acuteness of the pain, rendering him delirious at night, an exaggeration of the febrile disturbance. By some oversight I failed to exam-

ine the urine, regarding the sediment as most likely urates. July 1st the mother observed a swelling above the crest of right ilium, and at her visit two days later I found a well-marked tumor three inches to the right of the lumbar spine, fluctuating and pointing midway between crest of ilium and last rib; the thigh could be easily extended. The brace had not been completed, and as there existed no longer doubt as to the diagnosis, I referred, by courtesy, the case back to the family physician, who made an incision giving exit to a large quantity of yellowish offensive pus. He did not explore the cavity. The patient began to improve forthwith, and on July 24th returned to me for examination. He stood erect and walked with ease; general condition had greatly improved; the wound had nearly closed, only a few drops of sero-pus transuded. A few days later he returned, and the wound had closed. Discharged cured. February 5, 1876, at my request, he was brought to the hospital, and the only mark of recognition was a small cicatrix in the right costo-iliac depression. I examined him thoroughly, and found no lesion whatever.

CASE VI. was kindly communicated by Dr. W. G. Russell, of Brooklyn, the physician in conjunction with whom I treated the preceding case.

"July 29th, 1875, saw Mary Peach, æt. two and a half years, whose parents reported that for two or three weeks she had been less playful than usual, had lost her appetite, had lost flesh, walked lame, and for some days had been unable to walk at all. I found her complexion very sallow, the right thigh flexed. A few days subsequently a swelling developed above the crest of the right ilium. As soon as I could get fluctuation I made the incision, and was amazed at the quantity of pus evacuated. The child regained its appetite at once, and made a good recovery. I learned from the parents that the child almost from birth had been in the habit of drinking lager beer daily."

CASE VII.—E. J., female, æt. six years, presented at the outdoor department, Aug. 12th, 1875. Personal and family history good. On July 4th she gave the first symptom of any indisposition whatever. A loss of appetite was first noticed; her rest that night, and subsequent nights, was disturbed, great pain in loin. No history of any fall, strain, or injury, that

could be construed into an exciting cause. Within a few days high fever came on, thirst was great, constipation obstinate. These symptoms increased in severity. No position could be endured for a short time even with comfort. The pain became intense, and to secure relief she stood with the right lumbar region arched outward. During the second week a tumefaction appeared in this region, slowly enlarging, and deceiving the attending physician, who imagined that he had a case of caries of the spine, and sent it to the hospital for treatment. The fluctuating tumor, extreme suffering, absence of spinal deformity, general appearance of the patient, and duration of illness, enabled me to arrive at a correct diagnosis. Within a few days I called at the house, and, having ascertained the size of the tumor to be five and one-half inches vertically, and eight and one half inches transversely, made an incision, giving vent to a quart of thick, yellowish pus. The sac was not explored. I left in charge my friend Dr. Harlan, who had occasion, three days subsequently, to enlarge the opening. The internal medication consisted of iron, quinine, and cod-liver oil. Sept. 1st, all discharge had ceased, and the opening had closed. Within three weeks the cure was complete. Feb. 25th, 1876, one of my associates in the hospital, Dr. Crenshaw, visited the child, and learned that no relapse had occurred, the general health had been excellent, and he could find no trace of disease present.

CASE VIII.—M. E. O'B., female, æt. ten years, was referred to the hospital by Dr. Partridge, Aug. 30th, 1875. Five weeks prior to this date her health had been considered perfect. Began shortly afterwards to complain of pains indefinitely located, and to manifest general malaise. The suffering became distressing; and, at the end of the first week, high fever, location of pain in abdomen, flexion of left thigh and progressive emaciation were prominent symptoms. Hip-joint disease and caries of the spine were diagnosticated by two or three clinical teachers. Finally, on her appearance at the out-door department, her condition was as follows: Totally unable to stand or even assume a tolerable sitting posture; body poorly nourished and covered with sudamina, a uniform posterior curvature of the spinal column from the tenth dorsal to the third lumbar vertebræ when attempting to stand, at the same time flexion of

thigh at an angle of 135° . Along the left vertebral groove, a ridge-like prominence, tender to pressure, non-fluctuating and shading off into the abdominal walls; pressure over psoas, and extension gave tenderness. My diagnosis was deep-seated, acute lumbar abscess, and a nourishing diet was enjoined, Wyeth's preparation of beef wine and iron ordered; while, as an external application, a frequent change of cloths saturated with cold water advised, Sept. 11th. Since last visit the pain has been excruciating, causing delirium by night and great nervous prostration by day. There has also been considerable alopecia. The ridge-like prominence has become more circumscribed, and now we have a distinct tumor with doubtful fluctuation; spasm of psoas not so great. My first impulse was to make a free incision; but Dr. Knight, the surgeon in charge, favored a still further effort to promote resolution, especially as the existence of fluctuation was uncertain, and an evaporating lotion was ordered. The tumor two weeks later was very large, filling the whole lumbar space, and at one point the integument was thin, almost to transparency. Why not incise immediately? One of the hospital internes had differed with me as to the diagnosis, and at the time of this visit he was in the country. I was anxious to convict him of his error by having him see the incision; and as the suffering had greatly diminished, I directed the mother to bring the patient again in two days. She called at the end of a week and my tumor was on the decrease! while the patient was convalescing. I could get no history of diarrhœa, or of presence of purulent material in the stools; a cough had been present, and the mother on cross-examination admitted that the child "spat up something." This latter evidence was obtained, however, at a later date, after the disappearance of the tumor; and on auscultation I detected coarse mucous râles at the base of the left lung.

Well, our patient made a good recovery and no pus was evacuated, unless, perchance, it had forced its way through the diaphragm, the pleura, and the lung, per os. That the fluctuating tumor existed, others will gladly testify. Nov. 2d, I saw her, and no disease or deformity could be discerned. I had the child call on me again Feb. 2d, of present year, and the health was still excellent.

CASE IX.—H. M., æt. two years, a fairly nourished boy, was

seen first at the hospital Dec. 16th, 1875. His condition, so far as influenced by suffering, was very like that of many of the cases already described. It required the whole family and an aunt to bring him, and the combined force to undress him, so illy did he bear any rough handling. On examination, I found a partially-filled abscess over the left vertebral groove near the sacro-iliac junction, extending toward the left about four or five inches; two small punctured wounds from the needle of an aspirator were present, and I learned that the attending physician had removed on two occasions a thick greenish fluid. There existed moderate spasms of the left psoas and efforts to overcome this were attended with pain. Over the scalp was a fading eczema impetiginodes.

The history gives no account of a fall or injury of any kind, simply the eruption of the scalp, which was of several weeks duration. When this began to improve four weeks ago the child became feverish, fretful, and averse to being handled; one week later the left thigh was flexed on abdomen, and continued so for two weeks, when the muscle began to relax, coincident with which a swelling appeared in left lumbar region. My diagnosis was given unhesitatingly as peri-nephritic abscess, and the prognosis I gave with the same degree of freedom. As it was a case for consultation, the physician in charge proceeded with his treatment.

Imagine my surprise as well as chagrin to find at the next visit, January 6th, a slight irregularity in the appearance of the spinous processes of the lower three lumbar vertebræ, and immense tumefaction, with heat and tenderness of right thigh. The lumbar tumor had disappeared entirely, giving me a better view of the spinal column, and the spasm of the left psoas no longer existed. I changed my diagnosis, and applied a spinal brace immediately, telling the father to see his physician, and ask for me exclusive charge of the case. While I could not get satisfactory fluctuation at any point, and while the circumference was two and a half inches in excess of that of fellow thigh, I felt confident that I had here a residual abscess from carious vertebræ. Cold applications were ordered to the limb. January 10th, no perceptible change in condition of thigh, but the spinous processes seemed more nearly normal. January 15th, thigh hot, painful, and at a point over the in-

ternal lateral ligament of the knee fluctuation was distinct. An evaporating lotion, containing muriate of ammonia, acetate of lead and tincture of opium was ordered. January 21st, plentiful pustular eruption over limb like that following the use of croton oil. Lotion discontinued. January 28th, eruption disappearing. Circumference of thigh half an inch less, but at the inner aspect of knee the swelling has become more circumscribed, though there is no tendency to a spontaneous opening. The patient has been taking cod-liver oil with phosphate of lime, and an additional tonic from the beginning of our treatment. The local cellulitis, for such it undoubtedly was, terminated without suppuration, and on February 19th, both thighs and both knees were of one size respectively; in fact, no difference in any respect could be detected by a very close examination. No prominence, or irregularity even, of the spinous processes existed; the boy had been gaining health and strength daily. February 20th, Since last visit he has been running and jumping without the least inconvenience. Discharged cured. My final conclusions then are, that this was a case of peri-nephritic abscess, followed by cellulitis of the right thigh, and that no caries of the spine ever existed.

These cases are reported at length; and my only apology is, that from an analysis I wished to draw clinical conclusions, wished to be practical rather than theoretical. The three cases already on record, together with my own, should afford a complete history of peri-nephritis, whether it terminate in suppuration or resolution.

Etiology.—While studying the subject in text-books and current literature, I was led to the conclusion that this disease had as its cause, contusion, strain, fatigue, cold, or the debilitating influence of certain low fevers. In one of Dr. Bowditch's cases, no cause either predisposing or exciting could be found; in the other, the boy was probably of a strumous diathesis, and the exciting cause was most likely exposure. In three of the cases under discussion this evening, one was undoubtedly strumous, while two were probably strumous. Six of the nine gave no evidence of such a diathesis.

For exciting causes I have sought most diligently, have asked leading questions, and have conducted rigid cross-examinations. My result: One case following in the wake of a bronchitis;

one following an eczema impetiginodes. These two affections have not, as a rule, a very depressing influence on the system, and I fail to see any direct etiological influence. Among other causes, Tronseau¹ in his lecture already referred to, mentions acute pain in neighboring parts as a cause of peri-nephritic abscesses. The diseases above mentioned are not attended with very acute pain. In seven cases I could find no exciting cause whatever.

If sex predisposes, then, from my analysis, the female sex must act as a cause; for six were female and three male. The three cases reported elsewhere occurred in male children. Taking these into account enables us to eliminate sex as a predisposing cause. The age of the youngest child was one and a half years, of the eldest, ten; mean about four and a half. In seven instances the disease was on the right side, in five it was on the left.

Pathology.—Our unvarying success, at the hospital, in treatment, has prevented me from making any practical observations bearing upon this phase of the subject, other than to record as a rule that inflammation of the peri-nephritic cellulo-adipose tissue results in suppuration. To this rule there have been one or two exceptions. Niemeyer² says, in speaking of those cases in which suppuration does not occur, “the loose cellular tissue becomes condensed and indurated, and is converted into a thick fibrous pulp.”

Symptomatology.—In typical cases the disease generally begins with a rigor or two, febrile exacerbations more or less severe according to the acuteness of the attack, lancinating pains in lumbar region, loss of appetite and general indisposition. In fact, the invasion does not differ materially from that of other acute inflammatory lesions, unless perhaps the pain be more localized, and if the child be very young, the locality of the pain is not discovered. During the first week the symptoms change very little, except in degree. Constipation, I believe, is always present. Very soon we have preternatural immobility of the spine, a stooping forward with elevation of the shoulders. After a week or ten days, spasm of psoas muscle occurs, the gait becomes characteristic of that so commonly

¹ Loc. cit., p. 900.

² *Practical Medicine*, vol. ii., p. 40.

regarded as the second stage of hip-joint disease. The urine is of high specific gravity and is loaded with urates. The tumefaction appears, and the pain becomes excruciating. If an exit be given to the pus, a speedy recovery follows; if this be delayed and the contents of the sac be really pus, it burrows along the cellular tissue, producing an immense abscess, a spontaneous opening is effected and the convalescence is protracted. If, on the other hand, the inflammatory process has not resulted in suppuration, the contents are most likely serum, and resolution is effected.

A brief resumé of the symptoms observed in the preceding cases may render the subject still clearer. In the first, the pain invaded the knee, a day or two subsequently the hip-joint, and almost *ab initio* was very acute. An early incision was not made, and an enormous abscess extending from the costal region to the middle third of the thigh was the consequence. A remarkable feature was the quantity of pus evacuated daily; yet, when we remember the extent of pyogenic membrane and the excessive emaciation to which the child had been reduced, this becomes easy of explanation. The severity of the disease in the eighth case, in which delirium and alopecia occurred, surpassed that of any on record. No incision was made and there was no spontaneous opening externally; yet the tumor disappeared and a perfect recovery was attained. These two cases afford an interesting contrast and furnish a striking example of the abundant resources at Nature's command. In the third case the history of the invasion fails to bring out any marked febrile disturbances, and from the condition of the patient two weeks from the inception I would infer an unusual mildness of attack. The pain, however, the contraction of the psoas, and relaxation thereof when the tumefaction appeared, give a train of symptoms, the obscurity of which was dissipated only by the termination in resolution.

The seventh case furnishes the exception to the rule that spasm of the psoas always occurs. The length of time that elapsed in the fourth case between the first sign, lameness, and the lumbar tumor, gives a degree of chronicity unobserved in any except in that of the boy reported by Dr. Bowditch. In that case the abscess seems to have formed in two months' time, and partially emptied itself spontaneously through the bronchi.

Thoracentesis was performed two months later. In my case full three months elapsed before a swelling even could be detected. The early jaundice in the fifth case could have resulted either from a temporary functional disarrangement of the liver or from pressure of the kidneys against the gall bladder. The second, sixth and ninth are interesting on account of the age and the correspondingly rapid recovery. To the ninth is attached the additional interest of a cellulitis as a sequel. If I had not dwelt so much on the condition of the psoas I would again revert to it by way of emphasis. Dr. Duffin¹ lays great stress on this point, but he does not speak of the relief afforded when the abscess presents externally.

Concerning complications, there is no need for any extended remarks. The cases presented, with a single exception, have been uncomplicated. The exceptional case was the one with cellulitis as a sequel. One of the lads in Dr. Bowditch's report lost his life by an intercurrent pulmonary lesion. It would seem that nephritis would frequently result, but in no single instance has such occurred. In a collection of 26 cases in adult life I find eight complicated with renal and two with pulmonary disease. From Dr. Duffin's paper just alluded to, I find that of 26 cases collected by himself, hæmaturia was prominent in 2, pyuria in 5, calculous grit passed per urethram in 1, signs of vesical lesion in two, renal disease without bladder signs in 5, and no urinary disease in 12.

Diagnosis.—This is by far the most important branch of the subject, and a reference merely to the obscurity of the early, and the deception caused by the later symptoms, will be sufficient proof for the statement. All authors recognize the difficulty in diagnosing the affection at an early stage in the adult, and here we get aid from subjective symptoms. The objective symptoms likewise can be better studied in that the spaces in which they are manifest are larger, and the patients are more tractable.

When one comes to examine a child, frequently by nature and domestic surroundings, cross, with the additional peevishness induced by a fortnight's illness,—then long suffering and forbearance are indispensable qualifications to the physician. The

¹ Med. Times and Gaz., vol. ii. 1870, p. 362.

examination may be conducted at a dispensary, where a score of thoughtless babes in the waiting-room are raising their voices in inharmonious concert; it may be in a crowded room of a tenement house, where loose windows allow chilling draughts to transpire. Those physicians who devote much time to charity practice will understand the difficulties to which I refer. To diagnosticate an early peri-nephritis in a young child, I believe, is impossible.

A contusion over the renal region might be followed within twenty-four or forty-eight hours by the ordinary symptoms incident to acute inflammation; and there I see no reason why a diagnosis could not be made. The case reported by Löb¹ bears directly on this point. A boy six years of age was sleigh-riding January 1, 1868; fell striking his left lumbar region against the side of the sleigh. The superficial parts gave evidence of contusion, and on the same day he complained of pain in his loin. On the following day the pain had not abated, he walked lame, and febrile reaction was manifest. January 3d, he took to bed, the left side was arched, there was loss of sleep and appetite. January 17th, Dr. Löb first saw the case; and, though no tumor was present, yet from the clear history, the excruciating pain, the characteristic decubitus, and the spasm of the psoas, he had no difficulty in making a diagnosis. Whether the physician who saw the boy during the seventeen days' suffering made a diagnosis, the learned professor does not state. It may be interesting to mention that, from the language of the report, the diagnosis was made before the urine was examined, this being done at a later date.

The points on which I have placed the most reliance are, the acute nature of the attack, pain in the region of the kidney, flexion of the thigh—which in only two of my cases appeared sooner than the second week, the prone position, tenderness on pressure when the child is old enough to give any reliable testimony, the tumefaction, and, finally, the fluctuation. To analyze these symptoms and differentiate from those diseases which simulate very closely the one in question is not always free from difficulty.

In peri-typhlitis the pain and tenderness is chiefly confined to

¹ *Jahrb. f. Kinderheilkunde. Neue Folge*, viii. S. 197.

the iliac region ; there is pain more or less of a colicky nature, and the affection is generally associated with typhlitis. Should the tumor in peri-typhlitis appear in the lumbar region, then a differential diagnosis could not well be made, and practically it would be unimportant, for in that case we would most likely have a peri-nephritic abscess complicating the peri-typhlitic. In hepatic abscess the function of the liver is deranged, the tumor is located within that region and is movable with respiration. The symptoms of acute nephritis are like many of those now under discussion. From the history and the examination of the urine the question can be definitely settled. Renal calculi might mislead one, but the absence of fever, the paroxysmal nature of the affection, and the urinary examination should guard one against error. Idiopathic psoriasis in children and aneurism of the abdominal aorta are so rare that I shall devote no time to their consideration. Intra-thoracic lesions can be excluded by a physical examination.

Several of my cases have been diagnosticated as morbus coxae, and this mistake might seem unpardonable in an orthopædist. For the benefit of those who are not experts in this branch of the profession, let me say that however easy the differential diagnosis may seem theoretically, it is not always easy practically. I have already alluded to the difficulties attending a thorough examination, and to the unreliability of histories. The points on which I place most dependence are : the position of the limb, approximating toward that which many consider, without due reflection, the exact position for the second stage of hip-joint disease, the duration of this position, the involvement of the psoas alone in the contraction, and the absence of tumefaction or induration about the trochanter, or in the groin. It must be remembered that, as a rule, hip disease is chronic, that when no abscess appears a long time must elapse before the limb assumes the position characteristic of the second stage. A child whose hip is the seat of disease does not walk and run without the sign of a limp, and two weeks subsequently walk with the hand resting on a thigh flexed and rotated outward. Besides, my observation in peri-nephritis is, that little or no rotation outward is ever present. The pains along the thigh in and around the hip and knee-joints may mislead as they did in Case I. Still, I think if one makes a more thorough examina-

tion than I did in that instance, and obtains the history free from bias, a correct diagnosis can be made. We are very apt to construct our arguments by building from the conclusion upward; the premises are an afterthought, and it is amazing how easily one can get premises by asking leading questions.

I now come to speak of the difficulty of excluding caries of the vertebræ. In my experience, this is the *pons asinorum*, and after reviewing my cases I am almost prepared to admit the impossibility. You have heard me while reading the reports speak of the irregularity of the spinous processes, the immobility of the column, the sudden paroxysms of pain, causing the child to rush for the mother's lap, or a chair, or a bed. Well, the text-books give these as pathognomonic of caries of the spine. When a mother tells me that very recently the child was in perfect health, walked and ran like other children, I fancy I am getting at the secret finely. I refrain from interrupting, and she talks along, telling me that she has for a long time thought the child was "troubled with worms," that the stomach has been hard and swollen at times, that she has observed moaning and restlessness through the night, that in the morning the little fellow is not as supple as he is later in the day. She has finally given me, though unintentionally, a very good history of caries of the spine in its incipency. If I can feel assured that the present disease began with rigors and fever, nausea or vomiting even, constipation, loss of appetite, and pain in the side, I can, where the other symptoms point to caries, very easily exclude spinal lesion. The various tests for ascertaining the existence of inflammation in or between the bodies of the vertebræ will sometimes render valuable assistance. I have not found them, however, infallible.

When suppuration follows the peri-nephritis, and the abscess points in the lumbar region, the absence of angular curvature will enable one to exclude caries. To this rule even there is an exception. The case may not be seen until the abscess has appeared, and this may extend across the spinal column, hiding from view the spinous processes. In Case IX. this was the condition of affairs. While the sac was partially filled, and while I had evidence that pus had been removed the preceding day, and that the child had been running around actively four weeks before, I felt sure that no angular deformity existed; but

when, at the next visit, I found tumefaction of the right thigh, I examined the spine more carefully, and the thickened tissues of the collapsed walls of the old abscess deceived me, and I felt sure that caries, with deformity, existed.

After the incision, a manual exploration of the cavity will verify the diagnosis. In Löb's case this was done.

Prognosis.—Not one of our cases has terminated fatally. The duration of the disease, from the inception to the perfect recovery, is as follows: two months in 3 cases; three months in 3; five months in 2; six months in 1. One case, reported by Bowditch, recovered in nine weeks; the other died from a pulmonary complication. Löb's case made a complete recovery in seven weeks. The prognosis, then, in primary uncomplicated peri-nephritis, is very favorable.

Treatment.—During the early stages the treatment should be expectant. Counter-irritation, evaporating lotions, and anodynes I believe, are beneficial. Above all, look after the general health. After the abscess has appeared, that tonic which has been most serviceable in my hands has been a preparation of beef wine and iron, put up by Wyeth, of Philadelphia, and Caswell and Hazard, of this city. When fluctuation is distinct, the indication is to give exit to the pus. For this purpose an aspirator or a bistoury can be used; the former is probably the safer. As regards injections into the cavity, I see no special advantage in this procedure; a little well-guided patience will ensure about as speedy a cure.

ON THE MECHANISM OF PERINEAL INJURIES.

BY

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(With 6 Woodcuts.)

DURING the past two and a half years, whilst in charge of the class of diseases of women, at the North-Western Dispensary, I had occasion to examine over one thousand patients. A large number of the cases were simple endo-cervicitis (uterine catarrhs), accompanied by the thousand and one functional disturbances, incident to diseases of the sexual apparatus in the female. Whilst not having occasion to see many of the peculiar moral effects, so often the result of uterine disease amongst the higher classes, and attributable to civilization and luxury ; the influence of poverty, and debasement, and in some instances actual starvation, evidenced itself in striking pictures of moral depression and disease.

The number and character of uterine displacements were exaggerated in the extreme, and as the consequence of hard work, too early after confinement, might in a great measure be accounted for. A large proportion of the cases were retroversions, usually accompanied by prolapse in the first stage. Anterior displacements were much rarer. Flexions, conical cervixes, undeveloped uteri, and positive organic deformities, were usually found to be congenital, and were attended by barrenness. Lacerated cervix, as an existing cause of intra-uterine disease, was frequently met with, and vaginal deformity, procidentia of the anterior or the posterior wall, with and without rectal, vesical, or uterine complication, was oftener the rule than the exception.

The frequency of vaginal deformities drew my mind to the investigation of the cause predisposing to them, and in by far the majority of cases, injury to the perineum was found to be the inception of the difficulty. I have employed the term in-

jury, in preference to lacerated, or ruptured, as it covers more ground; and injuries *other* than those last-mentioned lesions, often are responsible for much that we see, when the perineum to our casual examination may appear intact. The anatomy of the perineum of the female, whilst being analogous, is essentially different to that of the male, distinct in situation, and in function; and, this part of anatomy being generally gleaned from texts which take the male as a type, mentioning supplementarily analogies in the female, leaves the student with a vague idea of this part of woman, the actual foundation and support of the sexual apparatus.

Dr. Thomas has endeavored to give us, in his work, a true idea of the female perineum; and in doing this and to impress the fact upon the minds of his readers, has avoided the distracting minute anatomy, and named the perineum, the "perineal body." This body is described as a cone, the apex of which is inserted as a wedge between the rectum and vagina. The picture is a good one, but is apt to be taken too literally, and many are prone to look upon the "perineal body" as merely a plug to fill in the recto-vaginal interspace, and from this stand-point lose sight of the physiology of the organism.

This crude idea of the perineum is apt to lead us to overlook the true value and importance of this portion of the generative apparatus, and under-estimate the character of the most ordinary accidents, anticipating lesion where they most rarely occur. Thus, finding the external angle of the base of the perineum intact, we falsely assure ourselves of the integrity of the whole organism. Not accurately recognizing the location or the duties of the perineum, we have not learned to protect it in threatened danger, and from this fact has sprung the the conflicting opinions among accoucheurs, as to the necessity or danger of perineal support in labor. Ramsbotham suggesting direct pressure, Churchill another method, Tyler Smith another, Isaac E. Taylor still another, whilst many late authorities, and amongst them the present German School, caution us against any interference at all. In the male, the perineal body may truly be viewed as a symmetrical cone, whose base corresponds with the outlet of the pelvis, and whose outlines present the appearance of nearly equilateral triangles; but the female pelvis being broader, and having straits with materially

differing planes, the female perineum is consequently modified correspondingly.



Fig. 1.
Reduced from Gray.

The straightness of the female rectum in comparison with that of the male, changes in a considerable degree the line of the posterior border of the perineum, and the rudimentary character of some of the analogous muscles, changes very materially its anterior boundary. The portion of the perineum usually involved in lesion, is that which in the male is behind the accelerator urinæ, which corresponds with the sphincter vaginæ in the female, and the true value of the perineum is impaired by any accident, destroying the relations between the sphincter vaginæ, sphincter ani, and the transversus perinei muscles.

It is not necessary, therefore, that the sphincter ani should be involved in the accident, or even that the integumentary covering, or the fasciæ of the perineum should be materially injured, but a disturbance of the relations between the muscles mentioned above, happens much more frequently than we imagine, and the mechanism of these accidents, with a passing

reference as to how to prevent, and repair the damage, is the object of this communication.

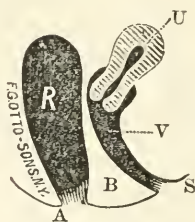


Fig. 2.

Normal Perineum.

A. Anus. B. Base Perineum. S. Vulva. V. Vagina.
R. Rectum. U. Uterus.

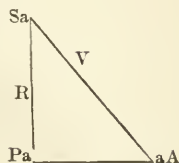


Fig. 3.

S A. Superior Angle. R. Rectal
Border. P A. Post. Angle. a A. Ant.
Angle. V. Vaginal Border.

The destruction or laceration of the posterior vulval fourchette is not to be classed among these accidents, as its preservation in first labors is exceptional, and a simple extension of this laceration, even involving a long rent in the *skin* between the vulva and anus, is frequently a complication which may not in the least impair the value of the perineum. Surgical aid need not be invoked in these cases, the wound almost always uniting to nearly the whole extent of the laceration by first intention. Although in very rapid labors, or in cases where instruments have been resorted to, destruction of the perineum *may* be incepted from the fourchette, it is safe to calculate that this is exceptional, and the radiate appearance of the cicatrices in these cases, points to the mechanism of these accidents. The actual yielding part of the perineum is the centre of the vaginal face of the perineal triangle, the hypotenuse, as it were; and the lesion partakes more of the nature of a perforation than of a tear. For this reason we may almost assure ourselves of a safe delivery in a thoroughly relaxed perineum, whilst a firm, unyielding one bids us anticipate trouble. The experience of accoucheurs that "crowning" assures the safety of the soft parts, even when the external anterior border of the perineum is exposed to its greatest tension, is testimony to sustain the fact, that the fourchette is not the part to be feared, and the patulous and easily distended vulva, even after extension begins, and the "perineal tumor" is forming, teaches us that we

may concern ourselves about the perineum, long before the vulva is implicated in the process.

At the moment that a perineal injury takes place, the location and shape of this part is entirely changed, the vaginal face becoming the base and the previous base becoming the anterior face.

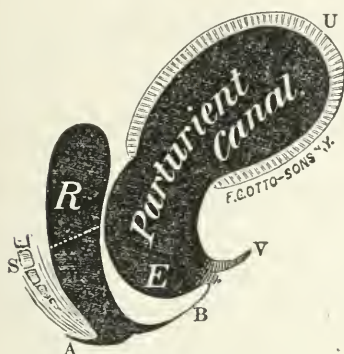


Fig. 4.

Perineum of Labor.

A. Anus. B. Base, now Anterior Wall. V. Vulva. S. Former Superior Angle. R. Rectum. U. Uterus. P. Previous Ant. Wall, now Base.

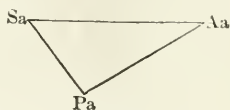
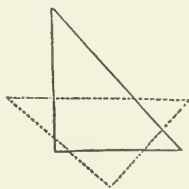


Fig. 5.

S A. Changed Sup. Angle
P a. Changed Post. Angle.
A a. Changed Inf. Ant. Angle.



[Fig. 6.

Comparison of Angles.

The fixed points of the perineum are the ischiæ, one on either side, and the coccyx behind. The sphincter ani muscle closed, being a continuation of the coccyx. The yielding of the anterior fibres of the sphincter ani permits the elongation of the perineum and a moving forward of the inferior posterior angle. The pressure of the advancing head, or head in extension, with a fixed point or fulcrum under the pubes, is consequently directed on the *apex* of the *lower* or *displaced* angle; and, at this point, unless "crowning" is accomplished, which relieves the direct force, and interposes between the pubes and perineum proper, perforation of the tense tissues of the perineum may occur, with *subsequent* lines of cleavage in the direction of the fourchette. The laceration of the anterior border of the sphincter ani permits the retraction of the muscle to its point of origin (the coccyx), tending thus to increase the *extent* of injury. Leaving out, then, those cases (and they are comparatively rare,) in which a large head and a small vulva predispose to laceration of the sphincter vaginæ, and cleavage from this

direction backwards, ruptured perineum may be expected to occur under the following conditions :

1st.—Unyielding perineum, where the angles have been displaced, and pressure is directed on the anterior border of the posterior angle.

2nd.—Too rapid extension of the head and the failure to relieve tension by early crowning.

3d.—Full rectum, mechanically relaxing the sphincter and exaggerating the displacement of the "perineal body."

4th.—Positions varying from first and second.

5th.—Injudicious instrumental and other obstetrical operations.

From what has already been said, the first condition explains the character of the accident.

To the second condition can accidents most frequently be traced; and an explanation of how this comes about refers us to a consideration of the mechanism of that stage of labor where the head has been pressed forward and under the pubes, and extension and formation of the perineal tumor follows.

Recalling this part of labor, we remember that version being almost completed, that portion of the head corresponding to the bi-parietal diameter, lies almost directly under the symphysis pubis, and the antero-posterior diameter corresponds to a line drawn from the symphysis to the coccyx. From this position extension begins, and the pressure of the forehead is directed from above downward directly upon the perineal body. As soon as the vulva has encircled the head, *i. e.*, as soon as the posterior border of the now distended vulva has a point of pressure, with the pubes as a fulcrum, the strain on the perineum above is relieved; but should the chin leave the chest before this relief has been instituted, the salvation of the perineum depends alone upon its power to resist the *vis-à-tergo*, and if labor be very prolonged, or if, on the other hand, too rapid, the overstretched fibres of the perineum yield, and laceration or rupture results.

The treatment of the threatened danger is best considered whilst we yet have the anatomical features of the lesion before us. If "crowning" has not been effected, and if experience proves to us that "crowning" guards the posterior part of the perineum, the pressing back of the sphincter vaginæ muscle

towards the rectum, so as to return the anterior wall to nearly its natural situation, serves to straighten out the parturient canal, and hastens the act of crowning—at the same time pressure gradually tires out the resisting muscles. Whilst pressing back the posterior vaginal wall, the fingers thus employed press also upon the longitudinal fissure of the head, not alone retarding too rapid extension, but substituting themselves as opposing points of pressure to the fulcrum at the pubes. Once fairly engaged in the vulval environment, and resistance of the intervening muscles overcome, the perineum ceases to be an obstacle, by becoming an actual factor of labor, and helps to complete the delivery. The transversus perinei and sphincter, retracting to their fixed points (the ischiæ and coccyx,) draws the released perineum *back*, as the face sweeps *forward*. When the head is well grasped by the vulva, the fingers, relieved of their first duty, are extended across the tense perineum, the thumb near one ischia, the four fingers near the other, permitting the curve between the thumb and first finger to correspond with the thinned edge of the fourchette. The value of this mode of procedure is more easily recognized by personal experiment than by glancing over written explanations; and all patients upon whom I have practised it have told me that the final and agonizing pangs of parturition have been moderated, and have thanked me for “helping” them.

The third condition mentioned as predisposing to these accidents is a full rectum, and the mechanism is as follows: The fecal mass acts as an artificial prolongation of the long or occipito-frontal diameter of the head; and the levator ani muscle, paralyzed by its presence, becomes inert to assist the retraction of the perineum. By this increased diameter the angles are further displaced forward, and from this point every step tends, unless the complication is relieved, to invite danger.

Nature at this point generally comes to our assistance, the fæces are dejected, and the tension thus relieved; but the careful accoucheur should, if called soon enough, see to the condition of both rectum and bladder, and render his aid in prophylaxis, rather than wait till danger threatens.

The fourth condition predisposing to trouble of this kind is where the third or fourth positions are changed to face presentations, or where this latter position (face) was recognized at the

inception of the labor. The fulcrum in these cases is the superior border of the perineum, and the pubes the arc of extension; or in foot or breech presentations, where the extended position of the head throws the chin in the perineum, when the expulsive efforts of the womb or injudicious manipulation pulls the perineum forward, threatening and often accomplishing the damage. Transverse or shoulder presentations, being generally transformed into foot deliveries, may also be herein included.

In speaking of the fifth or last predisposing cause to perineal disasters, instrumental and operative interference, I do not alone refer to the heedless disregard of the natural movements of the head in flexion and extension, but also to the *force* applied, as regards *quality* as well as direction. The misdirection of forces places the perineum directly in front of the line of traction, and *violent* force drags out the muscles and fasciæ till they yield. It is for this reason that we should never, even in the most stoic patients, apply forceps, without previously administering an anæsthetic; as a number of practitioners may recall instances in their own or other's practice, where, no anæsthetic having been used, the patient has assisted her delivery by planting her feet in the chest of her attendant, and suddenly put her perineum, as the wall of resistance, between *her* pelvis and *his* arms.

I at this moment recall an instance of this kind, in 1859, in the practice of one of New York's ablest surgeons. The patient, either without chloroform, or but partially under its influence, had the forceps applied, whilst the head, I think, was engaged. Whilst yet no traction had been made, and without giving the doctor an opportunity of exercising his good judgment and unquestioned skill, she suddenly drew up her knees, and kicked the physician with both feet in the chest, throwing him, still grasping the handles of his instruments, some distance from the bed. The head and perineum came simultaneously in *this* instance. Except in the rarest of cases, rupture of the perineum in instrumental labors may be attributed to needless haste or other carelessness.

The destruction of the whole perineum is hardly ever witnessed; and, when found, these cases necessitate complex opera-

tions, whose results are not always satisfactory. The following division comprises the injuries *usually* found.

1st. *Complete laceration*, in which the symmetry of the perineal body is lost, and where the tendon of the perineum is obliterated by retraction of the torn muscles to their fixed points of origin.

2nd. *Stellate fracture* of the perineum, involving separation from the raphé or central tendon of the transversus perinei—the laceration of the sphincter vaginæ, with or without a *few* fibres of the sphincter ani.

3rd. Laceration of the vagina within the vulva, including the sub-mucous tissues, with *no* injury to the muscles.

4th. A large number of recto-vaginal fistulæ.

The same explanation may be made to apply to each class of accidents, except the third mentioned variety, *i. e.*, where the vaginal tissues are torn. The mechanism is here changed, the accident usually occurring in cases where the head comes down covered by the os uteri, shortening the parturient canal by duplicature of the vagina. The flexion, and not the extension, here does the damage. Instead of the perineal body being pushed *forward*, the vagina is pulled *back*, until, when the head is engaged, and extension about to begin, the vaginal wall, at its junction with the fourchette, is at its *extreme* tension, and gives way by traction *from* this point. As soon as labor is completed in these cases, although the perineum *appears* intact, by careful inspection we will find a tongue-shaped piece of detached vagina lying just within the vulva. This lesion may amount to nothing, the replaced vagina attaching itself to its proper place by first intention; but in some cases this does *not* end so favorably, and we have an actual lengthening of the posterior vaginal wall. This constitutes vaginal prolapse without rectocele; but instituting a deformity which may not alone lead to this complication, but subsequently to uterine displacements. The occasion to demonstrate this character of accident to a professional gentleman happened whilst this paper was being written. The gentleman gave ether for me in a case of forceps delivery, where, in a beaked pelvis, with the head in the second position, I was forced to apply instruments above the brim. After delivery of the head, the upper shoulder being tightly wedged under the pubes, I was obliged

to extract the lower shoulder by lifting it over the perineum, and, as the shoulder slipped over the fourchette, I felt the tissues slightly give. I recognized what had occurred ; and, at my request, my friend examined the perineum, which was found to be perfect ! Not until I had opened the vulva, and put his finger in the little *cul de sac* formed by the retraction of the vagina, and now covered accurately by the detached mucous membrane, did he recognize *any* injury. The adaptation of the vaginal tongue to its bed was accurate, and the healing is complete and perfect as ever. Of these accidents, this much may be said ; although usually not extensive enough to produce displacements of the womb, they are sometimes provocative of vulval irritations, and productive of unpleasant symptoms of tenesmus and discomfort in the vagina and rectum.

I do not intend to enter deeply into the consequences of perineal lesions, reserving what little has been left to be said by our authors on gynecology for some future occasion. The reparative surgery of these injuries depends upon the character of each ; and nearly every surgeon of the present day has some favorite method. All point to a restoration to as nearly a normal condition as possible. There is one *rule* which all operators should observe ; viz., *always do enough*—as an insufficient perineum is almost as bad as no perineum ; and *partial* success often deters both surgeon and patient from further interference, and renders, even when attempted, secondary operations difficult of performance, and doubtful as to result.

A word as to the policy of interference immediately after recent damage to this part. I should certainly advise against it, and for the following reasons :

1st. Deep and extensive injuries are not disposed to heal, owing to the devitalization of the tissues from pressure, and the exposure of the surface of the wound to the irritating influences of the lochia.

2d. The shock which the patient has suffered, both from the labor and the accident, renders her an unfavorable subject for operation.

3d. The surrounding situation ; a lying-in chamber ; a possibly crying child ; an exhausted doctor, and a nervously over-anxious community of friends.

4th. The *fact* that any case which sutures would *possibly* as-

sist, will as surely heal without them, if the knees are bandaged and the vagina kept cleansed from impurities.

The proper repair of accidents of this kind restores the woman to usefulness and health; and a failure to relieve entails a species of chronic invalidity, and an enduring knowledge of having a womb and appendages; a circumstance which a healthy woman should never painfully recognize.

INTRA-PELVIC HEMORRHAGE.

BY

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ON the twenty-third day of December last, Miss X—, aged 32, in robust health, and without premonition, was seized with hypogastric pain, accompanied with nausea and some evidences of prostration, which disappeared in the course of an hour. This seizure was regarded as a simple transitory colic, and elicited no apprehension on the part of the lady or her friends; but, on the succeeding day, whilst ironing some articles of *lingerie* and laces, another attack manifested itself of a most agonizing and alarming nature. Within a few minutes of the onset of the pain she fainted away, and after the disappearance of the syncope, vomiting ensued. She was put to bed, but the horizontal position did not avert the faintings and vomitings. Messengers were dispatched to various medical gentlemen in the neighborhood, some of whom saw her, and, recognizing the gravity of her condition, administered restoratives and stimulants, which were rejected from the stomach almost as soon as swallowed. The hypogastric pains then ceased, or were masked by the appearance of more intense agonies in the hypochondria, which were constant and uninterrupted. In about six hours, convulsions of an epileptiform character were developed, and during the evening and night were very frequent

and violent. A message was sent me some time during the night, but I was unable to see her until about noon on the following day (Dec. 25th), when I learned the above history from the other members of her family. When I saw her she presented the following symptoms. Intense pallor of the skin and lips, dorsal decubitus, jactitations of the arms, general restlessness and uneasiness, whilst she complained of great thirst and unbearable hypochondriac pain. The pulse was almost imperceptible and very rapid, the respiration sighing, feeble, and frequent. The temperature was sub-normal, that of the axilla being 97° Fahr., whilst the cavity of the mouth marked only three-fifths more; the rectum was not tried on account of the restlessness of the patient. She had had no evacuation of blood from the nose, mouth, lungs, stomach, or rectum, but was menstruating, apparently normally, the flow barely staining the bed-linen. Vaginal touch revealed a slight fullness, but no fluctuation in the utero-rectal space; no rectal examination was made, in consequence of urgent objections on the part of the patient. Abdominal palpation provoked no marked uneasiness, and percussion indicated no more dulness than was to be expected in an individual disposed to the deposit of considerable fat, whilst in the gastro-colic region there was some resonance. The physical signs were negative; but from the rapidity and culmination of the symptoms, as manifested upon the second seizure, such as the epileptiform convulsions, the exsanguined and waxy hue of the skin and mucous membranes, the persistence of the syncopical phenomena, the vomitings, the progressive intensity of the anæmia, and the duration of the shock, all concurring with menstruation, led me to diagnosticate intra-pelvic hemorrhage, either from a varix of the venous system of the uterine appendages, most probably the sub-ovarian or utero-ovarian plexus, or in consequence of the rupture of the cyst of the broad ligament. Arterial hemorrhage was excluded, because its results would have been immediately fatal, and there was no clinical history to lead to such belief.

The prognosis was unfavorable. Death was to be apprehended in a few hours, in consequence of the acute anæmia

and prolonged shock ; or, if the hemorrhage should (under a remote contingency) cease, she would be carried off by the peritonitis induced by the extravasation of such a large quantity of blood.

The treatment, of course, was expectant, although one could not anticipate any good result. The damage had been done, and no medicament could possibly repair it. She was sustained by means of hypodermic injections of brandy and quinine, every hour, together with injections in the rectum of beef tea, carbonate of ammonia and brandy.

In a few hours, I saw her again, but the shock had continued and the anæmia was increasing. Her pulse was scarcely manifest, and the hypochondriac agonies were not diminished, notwithstanding the addition of some morphine to the brandy hypodermics. She had never ceased to reject all materials put into the stomach ; and, although the epileptiform convulsions were less frequent and strong, they continued at intervals, as did the vomitings, until death took place about 4 A. M. on the 26th, thirty-six hours from the last seizure, and some sixty hours from the first.

Twelve hours after death an autopsy was held, Drs. Little, Dumond, Wilson and others, besides myself, being present, Dr. Curtis making the section. The body was remarkably round and plump, but excessively pallid and exsanguined. Rigor mortis considerable. The abdomen somewhat tympanitic. The peritoneal cavity having been opened, bloody serum exuded, possibly a quart. When the omentum was lifted up, the intestines were found to be floating in pure blood, which was fluid as high up as the infra-diaphragmatic spaces, but clotted as the inferior pelvic cavities were approached. The entire true pelvis was filled with large masses of coagula. The diaphragm was ecchymotic, which accounted for the hypochondriac pains. The liver, kidneys, stomach, spleen and intestines were healthy but pale. The spleen, however, was shrunken to about half its normal size, and contained very little fluid. After the removal of the intestines, which were carefully examined, and the pelvis well cleansed, the source of the hemorrhage was found in a cyst of the left broad ligament, about the size of a walnut, to

which were matted some of the fimbriae of the oviduct, as well as the ovary. Indications of former pelvic peritonitis were found in numerous intra-alar adhesions of both broad ligaments. On the right side there were three smaller cysts near and under the ovary. The left ovary contained one large recent corpus luteum, and there were also traces of four old ones. The uterus was somewhat enlarged (menstrual hypertrophy), and the catamenial decidua were well developed in its cavity, as well as in the cavities of both oviducts.

The cyst whence issued the blood was eroded on its outer inferior margin, about the size of the little finger nail, and several large tortuous veins were wrapped and glued around the cyst wall.¹ The general venous network presented no abnormal appearances.

Here was a case where a woman, apparently in the bloom of health, without any warning, was stricken with death in the fulness of her usefulness, and who might possibly have been saved. Therefore, to the consideration of the treatment of intra-pelvic hemorrhage of this character I now propose to invite attention.

So much has been written on pelvic hemorrhage, and so many observations recorded during the past forty-five years, that it is quite a task to correctly eliminate the etiology from the mass of theories advanced by the various writers on the subject, from Récamier, in 1831, to Schröder, in 1875. Without analyzing the special views of any one individual or any class of authorities, it may be briefly stated that intra-pelvic hemorrhages are frequent, and many of them comparatively without danger and unrecognized, but that very often most serious and alarming symptoms are unfortunately not utilized with regard to diagnosis and treatment, even when the hemorrhage is recognized.

The sources of the blood are from the ovaries, or the oviducts, or the uterus, or the plexuses within and over the peritoneal folds and connective tissue of the pelvic excavation.

¹ Microscopical examination of the contents of the cyst by Prof. J. W. S. Arnold revealed nothing more than recent blood effusion, which was a mass containing basis tissue of reticulated fibrin with blood corpuscles, certain points holding all the blood-coloring matter.

Aneurismal tumors are purposely excluded, because their consideration more properly belongs to the domain of surgery than that of gynecology. It seems proper to regard intra-pelvic bleedings as free hemorrhages or hæmatomatous effusions, whilst an hæmatocele is a blood encystment. Hæmatocele is always the result of a free effusion, which is either of such insufficient quantity as to coagulate and become encysted in consequence of the peritoneal adhesions developed by the presence of the coagulum, or the blood is poured out into a preformed cavity, which, when filled, produces compression upon the bleeding vessels and excites thrombosis. Schröder insists upon the pre-existence of a cavity; and, notwithstanding Ferber and Barnes regard it as a mere hypothesis, I feel disposed to accept this doctrine, and I have long believed such to be one of the prime factors in the encystment of effused blood. This condition must be frequent, else we cannot explain the cessation of a bleeding which is *subsequently* discovered to be a retro-uterine hæmatocele, because, prior to the solidification of the blood-tumor, either by coagulation or by the filling of a pre-formed cavity, we fail to differentiate the effusion, in consequence of the absence of those diagnostic elements peculiar to hæmatocele, such as displacement of the uterus, compression of the bladder, and obstruction of the rectum, leaving out of the question the phenomena of uterine flexions, perimetritis, perimetric phlegmon, and intra-pelvic tumors, which might be confounded with the lesion under consideration. Unless there is some substance retaining or containing the blood, it cannot be felt through the vagina or rectum, but recedes before the finger; therefore, whenever the hæmatocele is diagnosed as such, the blood has already become partially solidified by coagulation, and fixed in the retro-uterine space in consequence thereof; or, the effusion has filled up a pre-formed cavity, which confines the fluid and prevents its flowing into the abdominal cavity. The non-formation of hæmatocele, after the discovery of intra-pelvic hemorrhage, is a very important element in the diagnosis, as well as the most salient point, the determining factor in the treatment of those sudden and overpowering hemorrhages which invariably end in death if left to the usual course.

Pelvic hemorrhages are classified in accordance with the etiological phenomena of their appearance, and are,

Fluxionary,
Obstructive, and
Traumatic.

Whilst this classification may not be strictly pathologically true; yet, in the main, it is clinically correct. The hemorrhage may be superinduced under any one of these conditions, or it may result from a union of either one. No hemorrhage takes place without a traumatism of the blood vessels, either physiological or pathological; yet a traumatic bleeding may be developed in the absence of the other factors.

Fluxionary Hemorrhage.—As most blood effusions take place coincident with the menses of menstruation, when the entire generative circle is gorged with blood, near the period of the rupture of the Graafian vesicle, when the menstrual decidua are developed in the uterus and oviducts, we readily understand how a thin-walled distended vessel may give way in any one of the various plexuses; and the anatomical locality of this effusion determines its *inter-* or *intra-peritoneal* lodgment. Under the head of fluxionary hemorrhages we might classify:

Failure of the ovipont.

Rupture of an ovarian or utero-ovarian varix.

Softening of ovarian structure, resulting from hyperæmia, or apoplectic effusions; and

Rupture of a cystic formation in the broad ligaments, uterus, or ovary.

Obstructive Hemorrhage.—Blood poured into the pelvic cavity, in consequence of obstructions, rarely takes place suddenly and in large quantities; and phenomena pointing to such issues are sufficiently marked to render the diagnosis and treatment clear. Under this heading are classified:

Imperforate hymen.

Atresia of the vulva or vagina.

Atresia of the cervical canal.

Absence of the vagina.

Bifid uterus and vagina, with imperforation or atresia of one of the canals.

Absence of the uterus, coincident with the presence of the ovaries and dehiscence of ova.

Stenosis of the uterine canal from flexions.

Polypi, polypoid and adventitious growths above the os internum.

Traumatic hemorrhage is the result of certain accidents, in consequence of pathological causes; or it is produced by blows, falls, etc. This classification includes:

Rupture of uterus during labor.

Rupture of an extra-uterine gestation sac, in consequence of the growth of the embryo.

Reflux through the oviducts, in consequence of irregular uterine contractions pending an abortion.

Disruption of adhesions to an ovarian or uterine cyst.

Laceration of the wall of an ovarian or uterine cystoma.

Rupture of a blood-vessel from any ulcerative process.

With a knowledge of the numerous causes of intra-pelvic hemorrhage, the question of treatment is the prime consideration. The array of fatal results is most formidable and appalling. Thus, for instance, Bernutz and Goupil give us the history of fifty cases of menstrual retention; and of that number seventeen died, and post-mortems revealed intra-pelvic hemorrhages. Thirty-four per cent. of deaths wherein no surgical interference was attempted, whereas three cases out of four recovered, in which the vagina was punctured to relieve the menstrual retention; to which may be added another, wherein suppurative pelvic peritonitis was mistaken for a retro-uterine hæmatocele, and a cure supervened upon puncture. Eighty per cent. of recoveries where an effort was made to get rid of the effusion. Again, of these fifty cases, seven recovered after a spontaneous evacuation through the rectum, vagina or bladder, and seventeen others were entirely or partially cured by means of surgical procedures. These figures teach us, that in all of the post-mortem examinations there was a greater or less amount of blood in the peritoneal cavity, and of those patients who did not die of the shock and anæmia, the rest succumbed from peritonitis. Furthermore, in the seven who recovered after spontaneous evacuation of the retained menses,

no peritonitis of an overwhelming character took place, nor did any intra-pelvic hemorrhage ensue.

In the face of these facts, Bernutz and Goupil regard the puncture of a hæmatocele as a terrible operation, because the admission of air, even in subcutaneous blood cavities is fraught with extreme peril, yet their successes were eighty per cent. after surgical treatment, and probably air must have penetrated the cavities of the seven who recovered after spontaneous evacuation. The deductions, then, are certainly not favorable to the expectant plan of treatment of intra-pelvic hemorrhages resulting from the obstructive etiological factors.

Now let us analyze the thirty cases of intra-pelvic hemorrhage and peri-uterine hæmatocele, also mentioned by Bernutz and Goupil. Of that number only six recovered, and they were diagnosticated simply as peri-uterine hæmatocele; and twenty-four died of peritonitis in consequence of the effusion, or of shock and anæmia prior to the development of the peritoneal inflammation. Of the twenty-four deaths, five were in consequence of the rupture of the foetal cyst of an extra-uterine gestation, and three from hemorrhage within the foetal cyst.

However interesting the monograph of Bernutz and Goupil may be, in regard to the summing up and clinical histories of the cases detailed, we glean very meagre information in relation to treatment, and we close this book with a feeling akin to despair after the perusal of such graphic mortuary statistics. Every case of intra-pelvic hemorrhage pure and defined, died; and died, too, because of the expectant or do-nothing plan, and this number constituted seventy-five per cent. of the total number. Twenty-five per cent. recovered because of the fixation of the blood tumor in the retro-uterine space, but the diagnosis was made only after such fixation.

I have dwelt thus particularly upon the cases of Bernutz and Goupil, because to them we are indebted for a most classical description of the subject under consideration; but an analysis of the general literature of the question gives us pretty much the same results, the same high rate of mortality, the same indecision and negative results of treatment. Of course I refer to those cases of sudden overpowering bleedings

which Barnes calls "cataclysmic," and which the French authors call "foudroyante." When the effusion becomes fixed and encysted in the true pelvis, when the hæmatocele is accomplished, the diagnosis and treatment are clear. In such cases the hemorrhage has been temporarily suspended, sometimes permanently arrested, and the symptoms of shock and anaemia replaced by those of peritonitis and pelvic obstruction; but in the primary stages of the trouble the points at issue are of supreme importance.

There are cases on record where very profuse hemorrhages cease and become encysted, even from ruptured extra-uterine gestation sacs, as reported by Aran, Voisin, Barnes and others; and the knowledge of these facts very much complicates the prognosis.

Can we have any assurance that the hemorrhage will cease, that the shock will subside, that the subsequent peritonitis will not be fatal, that the fluid particles of the blood will be absorbed, that protective encystment will ensue?

In reviewing the very meagre and vague suggestions of the treatment of hemorrhage within the pelvic cavity, we find nothing which offers the woman a probability of recovery. Most authorities are agreed upon the superior advantages of rest and absolute quiet, the administration of opium, and the application of ice to the abdomen during the period of the bleeding, as recognized by the pallor of the skin, the shock, the feeble sighing respiration, the rapid attenuated pulse, and the extreme restlessness.

But of what avail has been such medication co-added to stimulation? Absolutely nothing in at least seventy-five per cent. of all the recorded cases; and, could we obtain the histories of all such hæmatomatous effusions, the statistics would be even more harrowing. Shock and collapse contra-indicate medication, because of the irritable state of the medulla, as manifested by nausea, vomiting, feeble respiration, and debilitated heart action. It is a question by no means settled, that gastric absorption takes place during the stage of collapse; and if it should, is not reaction overtaken by stimulation; and is not that flood-tide so earnestly waited for, apt to become a

tidal wave rudely thrusting aside all of the barriers against death which the "vis medicatrix naturæ" interposes?

To shock and collapse, let the additional trouble of increasing anæmia be added, and we struggle in vain. From facts demonstrated by the histories of all these cases, can we anticipate much or any good to result from stimulation as long as the hemorrhage persists? If it be checked, and shock persists, can we anticipate a regular progressive reaction, which permits our patient to survive sufficiently long to encounter the almost equally deadly perils of peritonitis, induced by the presence of fluid and coagulated blood? These questions are perplexing in the extreme; therefore, we must look about us for something more hopeful than opium, rest, and abdominal ice applications.

Is opium an anti-hemorrhagic *per se*, or rather does it not soothe the irritated medulla, and deaden its sensitiveness for the time being, thereby quieting the sympathetic system? Certainly its exhibition is most beneficial—not as a stimulant, or a styptic, but as the sedative which gives nature time to ward off over-reaction. Notwithstanding this most potent reason, opium is powerless to stay a bleeding such as described in the case at present under consideration.

The same is true with regard to ice, the application of which is presumed to excite contraction of the vessels and thrombotic action, in consequence of its stimulation of the peripheral cutaneous nerve filaments. Nothing short of absolute freezing could check the effusion from a cyst such as was found in the specimen, where a reticulated mass of veins were welling forth from an erosion as large as the little finger-nail. Could ergot (supposing it absorbed through the stomach, or given hypodermically) produce such contractility of the vascular muscular system as to close the gaping orifices of a ruptured varix, a lacerated cyst wall, or a bursted gestation sac? Yet ergot is gravely recommended by many of the best authorities. We might as well attempt to obtain thrombotic action in any of the large veins of the mesentery, as to hope for such a result in any one of the tortuous plexuses of the utero-ovarian system. From these facts there remains but one course of action, a procedure in itself almost as dangerous as the lesion itself.

The hemorrhage must be stayed; and the only alternative left is to make gastrotomy, seek out the leakage, and ligate. From

the numerous cases of recovery after gastrotomy for ovarian cystomata and other pelvic tumors, we need not be very apprehensive of the extreme dangers of peritoneal section, its exposure to the atmosphere, and the cleansing of its surfaces of the effused and coagulated blood. Even should peritonitis have supervened, septicæmia may be moderated or checked by the washings now sanctioned by ovariologists; and the escape of red serum may be facilitated by proper drainage of the cavity. If the anæmia be so excessive and continuous as to threaten life, transfusion may be performed, and with almost as much hope for its success as we would anticipate for it in post-partum hemorrhage.

As to the indications for such treatment, no precise law can be made, nor any absolute directions laid down; yet we may approximatively arrive at a conclusion, whereby we would be warranted to proceed to such practice in a case presenting the symptoms of the one under consideration, and they may be summed up as follows:

The subjective or rational phenomena are: prostration and shock continued for more than a very few hours, progressive anæmia, as indicated by increasing enfeebled and sighing respiration, jactitation of the arms, debilitated heart action, syncopical phenomena, sleeplessness, (sometimes) epileptiform convulsions, and a tendency to, or actual, collapse.

The physical signs are the extreme and increasing pallor of the skin and mucous membranes, the cold sweats, the sub-normal temperature, and, above all, the failure to discover the presence of an hæmatocele or fixed blood-tumor in the retro-uterine space. The non-fixation of this blood effusion, co-added to the subjective phenomena, indicates that the quantity of fluid is great, that it has been and is being rapidly poured out in such large quantities as to overflow the pelvic brim, and to run into the abdominal cavity, thereby preventing a stationary coagulum which will of itself compress the bleeding vessels. In fact, when we can diagnosticate the presence of a well-marked and decided tumor behind the uterus, the probabilities are that the blood is retained by some substance above the superior pelvic strait, or contained within some pre-formed cavity, and

is prone to coagulate and of itself check the farther effusion, thereby acting as a mechanical hæmostatic, and preventing the issue of so much fluid as will immediately endanger life, or become subsequently fatal by the development of general diffuse pelvic and abdominal peritonitis. Hæmatocele, marked and defined, is a symptom which contra-indicates gastrotomy, but one which points to its future performance, if re-action do not supervene; but if the fixed blood-tumor do not exist, it is more than hazardous to wait. Experience has taught us that more than seventy-five per cent. of all recorded cases were fatal, either from the direct hemorrhage and shock, or from the subsequent peritonitis. Therefore, if we know, or at least if our fears are for the worst, we must not let our patient slip away without offering her a chance for life, and that chance consists in ligation of the bleeding vessel, cleansing the peritoneal cavity of blood and coagula, transfusion, if necessary, and the subsequent cares given all cases where such a serious procedure has been made, as involves the opening of the peritoneal sac by gastrotomy.

TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

Stated Meeting, Dec. 21st, 1875.

The President, DR. T. G. THOMAS, in the Chair.

FIBRO-CYST OF THE LABIUM MINUS.

DR. C. C. LEE presented an encysted tumor of the right labium minus, which he had removed from a patient 45 years of age, that afternoon. The tumor was of the size of a pigeon's egg, tolerably hard, and was supposed to have been caused by a strain during a slip several months previously. Pain and swelling of the right nymphæ were first noticed after this accident. The tumor had a distinct pedicle extending to the right ramus of the pubis. He expected to find a simple cyst, but found it to be a fibro-cyst, containing thick, grumous, chocolate-colored

fluid, similar to that occurring in ovarian tumors. The cyst had no connection whatever with the Bartholinian gland.

Dr. MUNDÉ asked whether cysts of the Bartholinian gland should ever be removed, or should merely be incised and cauterized to excite adhesive inflammation of their walls. He had seen two cases, in which he incised the cyst and filled its cavity with lint soaked in an undiluted solution of persulphate of iron, and both of which did not return, one patient being seen after two years, and the other not returning for treatment, which she had promised to do in case the cyst returned.

Dr. LEE said that his impression was that these cysts always refill unless extirpated.

Dr. GILLETTE said, that he had incised several, all of which had returned, one as many as three times in two years, although he packed its cavity with lint soaked in persulphate of iron.

Dr. CHAMBERLAIN said that he had incised one case and believed that it never returned; in another case he performed enucleation, and experienced quite profuse hemorrhage from a branch of the internal pudic artery.

Dr. BYRNE asked whether a microscopic examination of the cyst-contents had been made in Dr. Lee's case. He had seen one case in which a similar fluid was discharged on incision, which the microscope showed to be blood, thus proving the tumor to be a thrombus or hæmatoma. Might not this cyst of Dr. Lee's be merely a blood-cyst?

Dr. LEE said that he had not yet had time to examine the contents, but would do so; he did not think, however, that it was blood. [A subsequent examination showed the grumous contents to consist of fat, lymph, and some pus corpuscles, but of no blood whatever.]

Dr. THOMAS said that the question of treatment of these retention-cysts of the Bartholinian gland was a very interesting one. Guérin has given the most explicit account of them, and has referred to the danger of wounding the internal pudic artery during their extirpation. He himself had operated on probably a couple of dozen cases, and had never failed to effect a cure. His practice is to slit up the whole wall of the cyst, and cauterize its cavity thoroughly with the stick of nitrate of silver.

CASE OF FIBRINOUS POLYPUS.

Dr. T. G. THOMAS showed a tumor of the size of a small hen's egg, the history of which is as follows: A woman from Greenpoint came to him four days previously for violent metrorrhagia, from which she had been suffering for five months. She had not been pregnant for several years according to her

statement. No surgical treatment of any kind had been employed by her family physician. Dr. Thomas found the uterus enlarged, sharply retroflexed; the sound passed easily, and the uterus was readily replaced, which having been done, a wire loop was passed up, and a portion of the tumor, which could be felt with the loop and sound, removed. The patient returned home, and was seized with violent uterine contractions during the following night, which expelled the tumor presented, which appears to be what is known as a "placental polypus."

A subsequent microscopic examination of the tumor by Dr. M. D. MANN showed it to consist entirely of coagulated blood, with no trace whatever of placental tissue. Dr. Mann thought that nothing indicated that the effusion of blood had any connection whatever with a retained portion of the placenta.

CASE OF DIFFICULT LABOR WITH A BICEPHALOUS FŒTUS.

Dr. THOMAS exhibited a papier-maché cast of a bicephalous monster, which had been delivered by him several years previously. The case had never been reported, because the family were very much averse to such a publication; therefore, also, no dissection of the fœtus could be made. He was called to assist Drs. Thebaud and Reynolds, and found the fœtus, which had been in a breech presentation, born as far as the shoulders, and resisting all efforts at extraction. The mother was apparently moribund. He introduced his hand, and found both arms in the pelvis and a head above the superior strait, on the left side. He inserted his fingers into the mouth of this head, and attempted to draw it into the pelvis, but without success. Becoming fatigued, he withdrew his hand, and, re-introducing it, found that the head and mouth were now on the right side, thus showing the presence of two heads. Finding all efforts at traction unavailing, he cut through the median line of the child's body, from anus to dorsal vertebræ, with a pocket-knife, passed a strong fishing-line on a sound over the neck between the heads, and out on the lower surface of the child, and quickly sawed through the neck. Each half of the child, with whole head, was then easily removed. The mother rallied slightly, but died in a few hours. The body of the monster is single, there are two legs, two normal arms, two well-formed heads; and between the two heads, at the back of the neck, a thick arm projecting at a right angle to the body, the hand and fingers of which last arm are partly double, as though two arms were welded together.

Dr. JACOBI thought that the third arm was really single, and that its deformity had resulted merely from a super-abundance

of nutrient material during foetal life. The double fingers, also, were probably only remnants of the supernumerary primordial fingers growing physiologically in every foetus during the earlier months, which supernumerary digits generally atrophy to the regular number of five on each hand.

DR. JAMES B. HUNTER showed the photographs, and read the history of another

CASE OF DIFFICULT LABOR WITH A BICEPHALOUS FŒTUS,

which had occurred in the practice of Dr. S. McFarlane, of Toronto, Canada. The woman, a German, was a primipara, aged fifteen years and seven months. There was nothing unusual in her pregnancy, except that it was not even suspected by her father, who lived in the same house with her, up to the time of her confinement.

Labor pains began at noon July 30th, 1875. At eight p. m. the doctor was summoned, and saw the patient for the first time. He found one head, the right, already born, and in attempting to aid the delivery of the body, was surprised to find the vagina blocked by a mass, which, after examination, proved to be a second head. With a little assistance, and without the use of forceps, the second head was delivered and the body easily came down. There was but one cord and placenta. The perineum was not ruptured. The child seemed to Dr. McFarlane to have died in the act of delivery. The mother made a good recovery.

The monster, a male, was well developed in every respect, and rather above the average size. The specimen, when examined, had been two months in alcohol, and measured 21 3-8 inches in length and 16 inches round the breast. The occipito-frontal diameter of the right head was 13 7-8 inches, that of the left, 14 inches. There were two normal arms and legs, one body, and two distinct spinal columns, which seemed to unite at the sacrum; also two distinct ani. No dissection of the specimen was made.

The photographs of the monster were furnished through the courtesy of Professor H. H. Wright, of the University of Toronto, who also afforded every facility for its examination. It is preserved in the museum of that University.

DR. W. T. LUSK exhibited the specimen of the

GENITAL ORGANS OF A WOMAN, UPON WHOM SIMON'S OPERATION
OF POSTERIOR COLPORRHAPHY FOR PROLAPUS UTERI HAD
BEEN SUCCESSFULLY PERFORMED

by him, and read the history of the case and the cause of the sudden accidental death of the patient.

Mary G., æt. 50, entered Bellevue Hospital, Sept. 15th, for prolapsus uteri—the result of a laceration of the perineum, following forceps delivery, seven years previous to admission. On examination, an external tumor was found, about the size of a child's head, which contained not only bladder, uterus, and rectum, but intestines, which could only be replaced with difficulty. The cervical canal was completely everted, and formed part of the walls of the sac. On the 14th of October, the operation of colporrhaphia posterior (Simon's method) was performed for her relief. The night following the operation, in the absence of the night nurse, the patient got up out of bed and ran across the ward to chase a cat out of the window. Fever set in the next day. The perineum was found covered with blood. In spite, however, of the woman's imprudence, the vaginal sutures all united, and she recovered, so far as the mere retention of the uterus was concerned. As, however, the perineal sutures had in part given away, it was thought safer to perform perineorrhaphy before discharging her. This was done on the 18th of November. On the evening of the operation, the patient being at the time comfortable, two teaspoonsful of Magendie's solution were administered to her by mistake, and she died of opium poisoning.

The specimen presented shows very nicely the effect of the replacement of the uterus upon the cervix uteri.

Previous to replacement the cervix was completely everted, and was scarcely distinguishable from the vaginal tissues. The sound passed into uterus to the extent of two inches. Since replacement the cervix has been completely restored, and the distance from the os externum to the fundus uteri is nearly four-and-a-half inches. By pressing downward, however, upon the fundus of the uterus, it is easy, even now, to restore the everted condition.

Dr. Wm. M. CHAMBERLAIN related the following

CASE OF PROFUSE VAGINAL HYDRORRHOEA AFTER PARTURITION.

On the 22d of last October he delivered a lady, whom he had expected to attend as early as Oct. 5th. The pains being inefficient, ergot was given, and the child and appendages were readily expelled. While applying the binder he noticed a full, rigid condition of the abdomen, but could detect nothing on pressure. This condition still continued after several days, and on the seventh day a careful examination of the abdomen was made, without finding any solid or fluid-containing body. Hardly had the doctor left the room, when he was called back and found the patient drenched in a clear inodorous fluid, which

had suddenly escaped from the vagina. He would estimate the amount at at least a quart. The next day a discharge of meliceritous fluid remained, which gradually ceased. The clear fluid first discharged stiffened the linen soaked in it, on drying. He had been able to find no better explanation than that of dropsy of the Fallopian tube, which had developed during pregnancy. Still the possibility of the sudden rupture of a cyst of the broad ligament must be borne in mind.

Dr. PEASLEE said that if the fluid had been in the Fallopian tube, it would easily have been recognized by a physical examination after delivery. He thought the possibility of its being an ovarian tumor should not be excluded, especially as the fluid stiffened linen, which the fluid of a cyst of the broad ligament would not be likely to do. He had had a case in which, during a tedious labor, something was suddenly felt to have come down into the vagina. On examination, this something was found to be smegma, of which about one pint came away. The remainder of the contents of the dermoid ovarian cyst (for that it was) gradually escaped, and the patient recovered. Still, a fetid discharge continued, which was found on examination, to be caused by a bunch of hair in the cyst. On removing this, the discharge ceased.

Dr. THOMAS mentioned hearing of a similar case reported in Troy by a physician from Cohoes, in which a large discharge of clear fluid took place immediately after delivery, and repeated itself at the next confinement, some two years later.

Dr. DAWSON read the history of a case of

DEATH FROM TETANUS AFTER SUCCESSFUL PERINEORRHAPHY.

On Monday, Nov. 15th, at the invitation of Drs. T. B. Stirling and T. C. Finnell of this city, I operated on Mrs. M—, aged 35, for complete rupture of the perineum. There were present at the operation, besides the above, Drs. H. D. Nicoll, Paul F. Mundé and Chauveau. The rupture, which had occurred eighteen months previous, during delivery with forceps, extended a little obliquely to the left of the median line, an inch and a half up the recto-vaginal septum. The operation performed was the one so well described by Dr. Thomas in his work. The first two sutures were passed through the ruptured ends of the sphincter ani muscle, and within the recto-vaginal septum around the rupture through the latter. Four others were passed in the usual manner, and, on all being tightened, the denuded surfaces were perfectly coapted. I purposed moving the sutures on the afternoon of the ninth day, but early on that day her husband called upon me, and stated that his

wife could not open her mouth. I immediately visited her, in company with Drs. Nicoll and C. S. Ward, and at once removed all the sutures. The wound was perfectly closed throughout, no sign of suppuration or erysipelas being apparent. The patient was in good spirits, but with well marked lock-jaw, the masseter and temporal muscles being involved; the muscles of chest and back were apparently unaffected. Temperature in the vagina was 100 2-5, pulse 88. Ordered the rectum and vagina to be repeatedly washed out, and put the patient upon gr. iii. of Calabar bean every two hours. Saw her again at 5.30 p. m. Temp. 100, pulse 85; lock-jaw no better and complained of pain in the muscles of the back of the neck. Swallowed milk without difficulty. Being very restless gave her 15 drops of Magendie's solution hypodermically. Continued the Calabar bean. The next day, Nov. 25, she was no better, her temp. was 99 2-5, pulse 80, and there was also slight but well marked opisthotonus, with occasional twitchings of the pectoral muscles. Was then able to swallow milk, and was quite cheerful. The next day, Nov. 26, there were well-marked but slight tetanic convulsions, at intervals of five minutes, temp. 100, pulse 120, unable to swallow, pain in back, marked opisthotonus. Ordered ice to the spine and 20 grs. of chloral hypodermically every hour. This treatment being without apparent influence over the disease, she was put under the influence of chloroform, which controlled the convulsions somewhat, but they recurred during the night at intervals of five minutes, until the morning of the 13th day after the operation, when she died, apparently from paralysis of the heart, as Dr. Finnell informed me.

In addition to the above I will mention that the patient was a strong healthy woman; the house she lived in was clean and well-ventilated, and the behavior and appearance of the woman was all that could be desired. The Calabar bean, morphine and chloral, apparently had but little, if any, influence over the disease.

Stated Meeting, Jan 4th, 1876.

The President, DR. T. G. THOMAS, in the Chair.

DR. ROBERT WATTS read the following history of a case of

INVERSION OF THE UTERUS.

Catherine C., aged 21 years, married, seamstress, was admitted to the Roosevelt Hospital on Sept. 3, 1875, and gave the following history:

She began to menstruate at the age of 15, and was always regular, up to the time of her marriage, three years ago. Two

years ago she was confined at full term. Her labor, she says, was a severe one, but unattended by any accident; but she lost a great deal of blood and was obliged to keep her bed for several weeks. Hemorrhage continued for three months after confinement, and then ceased, but recurred again at intervals of two weeks, and lasted about eight days each time. This metrorrhagia has continued ever since. In April last, being very much exhausted by the frequently recurring hemorrhages, she consulted a physician who, after making a vaginal examination, told her she had "falling of the womb," and inserted a ring pessary. Since then she thinks the hemorrhages have not been quite so severe, but she has suffered with a constant, profuse leucorrhœa, tinged with blood. On admission to the hospital she was very weak, the slightest exertion causing violent palpitation of the heart; the skin was blanched, and the lips colorless; appetite poor; pulse 100 and small.

The urine was alkaline, sp. gr. 1008. It contained no albumen.

Vaginal examination, after removing the pessary which had been worn for five months, revealed a pear-shaped tumor high up in the vagina, the largest part being the most dependent. Around the neck of the tumor could be felt a ring separated from it by a shallow groove. Careful exploration with a probe failed to detect any opening at the bottom of this sulcus. By conjoined manipulation through the abdominal walls, and with a sound in the bladder and finger in the rectum, no uterus could be felt in the normal situation. Manipulation caused the tumor to bleed freely, and on introducing a speculum, it was seen to be of a bright, red color, and evidently covered by mucous membrane, from the surface of which the blood could be seen oozing.

The diagnosis was made of complete inversion of the uterus. Chalybeates and generous diet were ordered.

Sept. 10. As hemorrhage continued, a solution of iron-alum, gr. v ad $\frac{3}{4}$ i; was ordered as an injection every night and morning, to be followed by the insertion of a pledget of cotton soaked in a solution of tannin in glycerine.

Sept. 17. Tumor slightly diminished in size and paler in color. The constricting ring a little relaxed. The hemorrhage had ceased and the injections were discontinued. Inserted an air ball pessary which is to be removed every night and morning, and again replaced after syringing the vagina with hot carbolized water.

Sept. 24. Patient's general condition much improved. The air pessary has been used until to-day.

This afternoon, the patient having been etherized, an attempt was made to reduce the inversion by grasping the uterus

firmly with one hand, while counter-pressure was made with the other hand through the abdominal walls. The pressure was directed mainly against one horn of the uterus, as suggested by Dr. Noeggerath; but, after $1\frac{1}{2}$ hours, no progress had been made, further than a slight dilatation of the constricting ring, and the attempt was abandoned.

The patient was ordered morphine, gr. $\frac{1}{4}$ every four hours, and put upon milk diet.

No unpleasant symptoms followed this operation, but nothing further was attempted, in consequence of the occurrence of a small phlegmon over the pubis, until Oct. 22, when a second attempt at forcible reduction was made. Pressure was continued for $1\frac{1}{2}$ hours, at the end of which time it was discontinued.

Oct. 28. On my invitation, Dr. T. G. Thomas was kind enough to see the patient with me, and tried a plan of treatment proposed by himself, which consisted in applying a tightly fitting india-rubber cup over the inverted uterus. This was accomplished with some difficulty, the uterus being drawn well down, and the patient being etherized.

The patient had a slight attack of pain two hours afterwards, but it soon ceased and did not recur.

The following morning the rubber cup was found lying loose in the vagina, having slipped off the uterus, which remained as before.

Oct. 31. The patient was etherized; the uterus was drawn down outside the vagina and held firmly, while Dr. Thomas wound around it a bandage made of thin sheet india-rubber. This was secured by india-rubber rings slipped over it. Magendie's solution morphiæ \mathfrak{m} xv were then administered hypodermically and the patient put to bed. This was at 11.30 A. M.

At 2 P. M. she complained of severe pain in the back and running through to the hypogastric region, and the morphine was repeated.

At 6 P. M. she still had a little pain. The bandage was in place. Morphine repeated.

Nov. 1. The rubber bandage, having been applied 22 hours, was removed by Dr. Thomas. The uterus remained unreduced. The pulse was then 132, temp. $99\frac{1}{2}$. Two hours later the patient had a violent chill, followed by high fever (temp. $104\frac{3}{4}$), and during the night had some vomiting. The abdomen was tender on pressure and a little tympanitic. Magendie \mathfrak{m} xv was given every four hours.

Nov. 2, 9 A. M. Pulse 136, resp. 20, temp. $105\frac{1}{4}$. Patient vomiting constantly. Abdomen tender and tympanitic. The vagina was found filled with very fetid clots. The uterus re-

mained inverted. Ordered vaginal injection of carbolized water every six hours, and morphine to be given to control pain.

8 P. M. Abdomen swollen and tympanitic; knees drawn up; face pinched and a little cyanotic; pulse 140 thready; resp. 20, temp. 104½.

The patient passed through a sharp attack of peritonitis, which was treated with morphine. The symptoms gradually subsided, and on Nov. 10 the abdominal tenderness was gone; the face had assumed a natural expression, and the temperature was nearly normal, 99¼.

All vaginal discharge had pretty much ceased; but on Nov. 10 there was passed, per vaginam, a mass which, on examination, appeared to be a slough of the mucous membrane of the uterus. It was not a perfect mould of the uterus, one side being partially wanting, but the mouth of the mould was a complete ring. Digital examination revealed the absence of the inverted fundus from the vagina. The cervix was in its proper position, but feeling as if lacerated bi-laterally, and the tip of the forefinger could be inserted into the os. No further examination was deemed prudent at that time. The patient was kept in bed for another week, at the end of which she was feeling quite well and able to be up.

On Nov. 30, a careful examination was made. No tumor could be felt in the vagina. The cervix occupied its proper position, but still felt as if lacerated. The uterine sound could be passed only to the depth of two inches. With a sound in the bladder and a finger in the rectum, the body of the uterus could be felt in position, but apparently smaller than natural. The patient absolutely refused to take ether, so that conjoined manipulation through the abdominal walls could not be satisfactorily practised, owing to the resistance of the muscles.

Dec. 31st. The patient is now working about the house, and seems quite well, except that she complains of a heavy feeling, such as she says always used to precede menstruation. No menstrual flow has, however, made its appearance as yet, though these feelings have continued for a fortnight.

In reviewing this case, the points of interest which attract attention are:

1st. The time when inversion occurred.

It seems probable that it did not take place suddenly at or just after delivery, as the patient gives no history of such an accident; but that a partial inversion having taken place then, it increased gradually until it became total. The completion of the process must have taken place a long time since, as the uterus had become reduced to nearly its normal state.

2nd. The treatment.

The plan carried out by Dr. Thomas was based upon the theory that to reduce an inverted uterus it was necessary to make only circumferential, and not upward, pressure. This theory was founded upon the fact that, in the several cases seen by Dr. Thomas, the re-inversion began at one horn of the uterus (to which attention has been directed by Dr. Noeggerath) while the organ was being firmly grasped by the hand, but no upward pressure was being made. The result of this single case would seem to prove this theory incorrect.

The lighting up of an acute peritonitis, after the removal of the india-rubber bandage, shows the danger from prolonged constricting pressure; but it may be reasonably questioned whether the use of such a bandage for a short time, say an hour, by emptying the inverted uterus of blood and other fluid, would not diminish its bulk sufficiently to insure its successful reduction by forcible pressures, applied immediately after the removal of the bandage.

3d. The spontaneous reduction of the inversion immediately upon the diminution in the size of the tumor caused by the exfoliation of the mucous membrane.

DR. JAMES S. GREEN presented the specimen, and read the history of a

CASE OF EXTRA-UTERINE PREGNANCY—RUPTURE OF THE SAC—
DEATH FROM HEMORRHAGE.

Eliza K., aged 28 years, colored, resided in Rahway, New Jersey, and was the mother of two living children. A short time before her death, she supposed herself to be in the third month of pregnancy. She had suffered very much for two weeks previous to her demise, with frequently-recurring pains in the lower part of the abdomen. On the evening of December 29, 1874, after she had gone to bed, she was startled by the feeling of "something warm pouring into her stomach" (as she expressed herself), and became suddenly faint. Before medical aid could be summoned, she died in collapse.

Autopsy. Upon opening the cavity of the peritoneum, it was found to be filled with fluid and clotted blood, to such an extent that the intestines floated in it. Upon removing the uterus and appendages, the specimen presented to the Society was found.

This proved to be a case of extra-uterine pregnancy of the "*tubal*" variety, and was developed in the *left* Fallopian tube, at the point where it joins the fundus uteri.

The remains of the ruptured sac measured three inches by two inches, and contained the placenta, with a portion of the

umbilical cord attached. The remains of the free walls of the sac are still attached to the edges of the cavity, and show that the anterior wall of the sac, opposite the attachment of the placenta, was the point at which the rupture took place.

The uterus measured four and a half inches, from the os externum to the fundus; and two inches and a quarter in the breadth of its fundus. The walls of the uterus were increased at their thickest part to three-quarters of an inch in thickness. The uterine cavity measured three and three-quarter inches in depth.

The fœtus appeared to be, in size, like one of two and a half to three months' growth, and measured four inches and a half in length.

DR. HORACE T. HANKS reported a

CASE OF CARCINOMA OF THE VAGINA,

Feb. 23d, 1875, Mrs. L. S. M. was sent to me by Dr. Darkin for diagnosis and treatment. She was a native of U. S., aged 25, married four years, living single one year. Had never had children, but had aborted at the third month twenty months previous. The cause of abortion was unknown. Her health, prior to this accident, had been excellent. She gave no history of syphilis. Her husband, however, who was four years her junior, had been in poor health, and was growing idiotic at the time of her conception. (He has since been sent to an asylum.) She thought he had never shown symptoms of syphilis. Since the abortion she has never felt well. There has been a constant dragging sensation, with often a positive burning pain in lower pelvic region. The pain lately had begun to extend back towards the sacrum. It has not been constant, but frequently very acute. She has suffered some rectal tenesmus while at stool. Her menses had been irregular for the past four months, being profuse and alarming. She had also had two severe hemorrhages between the menstrual epochs. Her appetite had been capricious and her bowels constipated. Had frequent headaches. Her husband had caused her much anxiety for the last year, and she had become fretful and impatient. Had taken much medicine, but never had had a physical examination per vaginam.

The patient is a small, fair, delicate woman, apparently less than 25 years of age. She is exceedingly pale from loss of blood. The complexion has a slightly straw-colored tint. The features have that peculiar pinched and anxious expression which so surely indicate a painful constitutional disease. The

pulse is feeble, slightly accelerated; respiration and temperature normal.

On making a physical examination a fetid, watery discharge is found oozing from between the labia. About one inch from fourchette on the posterior wall of vagina, is a nodulated growth one inch by one and a half inches, of elliptical form, rising one-sixteenth to one-tenth of an inch above the floor of the vagina. The growth extends backward to within one-half inch of the juncture of the vagina with the cervix uteri. The vaginal wall surrounding the tumor is but slightly indurated and fixed, but is very sensitive. This outgrowth is readily broken down, bleeding profusely. It has a slightly gritty feeling, like the pulp of a half-ripe water-melon. Ocular inspection shows the tumor to be bright red, rough and uneven, like a large flattened raspberry, magnified to twice its diameter. The uterus is in position, of normal size and healthy.

The symptoms, subjective and objective, all point to a serious constitutional disease. Physical examination reveals the disease to be a malignant outgrowth of the vagina.

Feb. 26. The patient was taken to the clinic for diseases of women, at the College of Physicians and Surgeons. Prof. Thomas and the clinical assistants, Drs. Walker and Vernilye, confirmed my diagnosis, and pronounced the type of the disease to be carcinoma vaginæ of the encephaloid variety. Prognosis unfavorable.

Feb. 27. The diseased parts were carefully brought into view with a Cusco's improved bivalve speculum, and freely painted with the strong solution of the sesquichloride of iron. The patient was ordered quinine and iron; opium as needed, and daily injections of warm water, with chlorate of potash and alum in solution. To be nourished with milk and beef tea, and such solid food as she can take.

March 2d. Has passed three comfortable days, without hemorrhage. The outgrowth has entirely disappeared, leaving a clean, bright red, granulating ulcer. This was painted with a dilute solution of the sesquichloride of iron. Continuation of the same medicines internally and locally.

March 9th. Has had less pain, and no hemorrhage; rests better; appetite improved; granulating surface of ulcer red and exuberant; no spreading of the disease. Painted with a strong solution of the sesquichloride of iron.

The patient was seen every week until April 7th. The same treatment was continued. There was no hemorrhage except at her menstrual epoch, and this not abundant. At this time she became aware of the character of the disease, and an advertising "cancer doctor" was called, who attended her for

a short time without local treatment. Afterwards, a homœopathic physician was called, who visited her daily for two months, without making a physical examination or using local treatment.

July 30. I was again summoned to check an alarming hemorrhage—the third since my last visit. An examination showed that the disease had extended backwards and into the rectum, and upwards to the cervico-vaginal junction. The hemorrhage was checked as before.

Aug. 4. Has lost no blood, but suffers excessively from pain. Is unwilling to have any local application made. From this date she grew worse rapidly.

Sept. 25, a profuse hemorrhage occurred, from which she never rallied, but remained in a semi-unconscious state until Oct. 10, when she died.

On the evening of the same day, assisted by Dr. Bullard, I was permitted to remove the diseased parts. The ulceration had extended upwards and to the left, destroying nearly all the posterior and left lateral portion of the vagina, and invaded a part of the cervical portion of the uterus, on the same side. Inferiorly it had eaten through into the rectum, and destroyed the anterior portion of the rectum for an oval space of about 1 by $\frac{1}{2}$ inches. The tissue to the left of the vagina, and posterior to the rectum, is much thickened, and the seat of nodules. These have been examined by Dr. T. E. Satterthwaite, with the following results: He found that "some of them had undergone degeneration, and had become cysts, with dirty-brown or yellowish-white contents, consisting of broken-down epithelial corpuscles, granular matter, and fatty crystals. The firmer nodules were found to consist of epithelial corpuscles, of large size, with large nuclei packed away in between bundles of connective tissue."

Remarks. In conclusion, I wish to call attention to the advantages of the strong solution of the sesquichloride of iron in the treatment of epithelioma, medullary carcinoma, and sarcoma. In the early stage of all these diseases there is a peculiar outgrowth, consisting of numerous small, thin blood-vessels, the walls of which are easily broken down. Sesquichloride of iron seems to have a decided action upon—almost an affinity for—these congeries of blood-vessels, and quickly destroys them, at the same time stimulating the surface underneath to a more healthy granulation.

During the last year I have had six cases in which the sesquichloride of iron was used exclusively, with the result of preventing all hemorrhage, and evidently staying the progress of the disease. In one case of incipient cauliflower excrescence,

it completely destroyed the growth, leaving a healthy surface, which has remained so for six months, and there is no appearance of a recurrence of the disease at the present time.

The case of carcinoma here reported is of interest :

(1) On account of the *age* of the patient, she being only 25 years old,—few having been recorded under that age.

(2) On account of the *locality* of the disease—encephaloid carcinoma of the vagina being unusual.

(3) The rapidity of the progress of the disease.

(4) The effects of the treatment in staying the progress of the disease, and in preventing and checking the hemorrhage.

CROTON OIL PAINT IN PELVIC CELLULITIS.

DR. CHAS. A. BUDD called attention to the unsatisfactory treatment of pelvic cellulitis by blisters, iodine, and oleate of mercury, and said that he had lately been using Dr. Corson's croton oil paint (℞ ol. crotonis, ʒ ii, ether. sulph. fort. ʒ iv, tinct. iodini, ʒ ii, potass. iodid., ℥ i, iodini, gr. x, ʒ i.), with the happiest results. In four cases, where a pelvic abscess seemed unavoidable, under the use of this paint the exudation fairly melted away.

He also exhibited an instrument for washing out the bladder in cystitis; with a little practice the patient could be taught to do it herself.

DR. WM. T. LUSK showed the specimen from and read the history of a

CASE OF PREGNANCY WITH FIBRO-CYSTIC TUMOR OF THE UTERUS ; OCCLUSION OF THE CERVIX, ECLAMPSIA, VERSION, POST- PARTUM HEMORRHAGE—DEATH FROM PERITONITIS AND COL- LAPSE—RESUSCITATION OF STILL-BORN CHILD.

Mrs. X., primipara, æt. 38, came to my office, in the latter part of November, upon the recommendation of Dr. Marion Sims, to ask for my professional services in her approaching confinement. She stated that her case had excited the interest of a number of physicians, and at once called my attention to a large tumor apparently solid in character, occupying the left half of the abdomen; while upon the right side the fœtus could be distinctly mapped out through the abdominal and uterine walls, thus leaving no question as to the existence of pregnancy. A hasty vaginal examination satisfied me that I had to deal with a head presentation. The tumor was situated high up, and did not encroach upon the pelvic cavity. According to Mrs. X.'s calculation, her confinement was to be looked for about the 15th of January.

On the 20th of December I was summoned to see Mrs. X. by a message apprising me that she was seriously ill. Apprehensive of imperfect uterine contraction after delivery, supposing labor to have begun, I put in my bag the various requisites which might be needed in case of post-partum hemorrhage, and hastened to the house.

On arrival, the husband informed me that on his return from breakfast, he had found his wife lying on the floor insensible, by the side of her bed, from which she had apparently fallen.

He at once raised her up, and replaced her in bed. Soon after, she had a "fit," whereupon he sent for me. I found the patient conscious, but blind, and with nearly complete abolition of memory. However, as I knew the lady came of hysterical stock, and had experienced some annoyances for a few days past, I hesitated a short time, before deciding as to the character of the convulsions. A third attack, occurring in my presence, left me in no doubt, however, that I had to deal with true eclampsia. I had examined the urine three weeks previously, and found it free from albumen.

Chloroform was at once administered, and its action supported by hypodermic injections of morphia (two injections of Magendie's solution, mv . each.) After complete anæsthesia had been secured, I proceeded to make a vaginal examination, and was not a little surprised to find the vagina terminate seemingly in a cul-de-sac. Upon careful examination, however, I detected a small cicatricial seam, which I rightly assumed to be the orifice of the occluded cervix. With my finger-nail, I gradually succeeded in making a passage upward from this point to the membranes. Dr. Sims informs me that the patient had menstruated previous to pregnancy, but through an opening so small as barely to admit the finest uterine probe. I then endeavored, by means of Barnes' dilators, to expand the cervix so as to enable me to complete delivery. Upon the removal of the smaller-sized dilator, I recognized the cord within the membranes. As the dilatation of the cervix was chiefly mechanical, and did not provoke labor pains, it was upwards of three hours before the cervix became sufficiently dilated to allow version to be performed. During this time no convulsions occurred. Finally, however, the feet were brought down by means of Braxton Hicks's method. As the membranes ruptured, the cord was washed outside the vulva. No pulsations could be detected. Assuming the death of the child, I performed the act of extraction slowly, while Dr. Van Vorst, from Bellevue Hospital, who had kindly come to my assistance, maintained steady pressure over the uterus. Notwithstanding these precautions, the

expected hemorrhage followed the delivery of the child. Without stopping to tie the cord, I at once introduced my hand into the uterus, and brought away the placenta. After the removal of clots, pieces of ice were rapidly passed into the uterine cavity, and in the course of a few minutes firm contractions were attained; not, however, until the patient had lost a serious amount of blood. Dr. Van Vorst, at this juncture, informed me that the child's heart was beating. Asking him to take the uterus in charge, I took the child, and by means of Schultze's method for artificial respiration, succeeded very soon in getting it to breathe spontaneously. After rather vigorous flagellation, the child began to cry, was then wrapped in warm blankets, and laid upon the bed between bottles of hot water. Since that time the child has continued to thrive (written Jan. 29th). It weighed, at the time of birth, $4\frac{1}{2}$ pounds.

TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

Reported by W. H. H. GITHENS, M.D., Secretary.

Stated Meeting, Thursday, September 2, 1875.

DR. A. H. SMITH, President, in the Chair.

OVARIAN TUMOR.

DR. J. V. INGHAM exhibited to the Society a specimen of a solid ovarian tumor recently removed by Dr. Walter F. Atlee, of this city, who had presented him with the following notes of the case.

Miss H——, of Mount Holly, asked my advice about a tumor of the belly, in the middle of May. She was 33 years of age, rather pale, but in other respects of healthy appearance. She had been annoyed by nasal catarrh for many years; some dyspeptic symptoms had begun about a year before, and for the last three months her menses had not appeared. The tumor was nowhere adherent to the skin, movable, not fluctuating, only to the touch it felt like a multilocular cyst of the ovary.

On the 14th of June, I removed the tumor here presented, with the assistance of my father, Dr. Atlee, of Lancaster, and Drs. Pleasants, Hunt, and Hickman. Having cut through

the tissues covering the tumor, a large trocar was pushed into it, but nothing flowed through the canula. By some tearing of the tumor, and by extending the incision above the navel, it was extracted. The pedicle was very short and thick, and the clamp was applied very close to the left horn of the womb.

On the 14th day the clamp was removed; on the 16th day she came down stairs, and a few days afterwards she returned home to Germantown, from which place she came to be operated upon, perfectly well.

The tumor was referred, for microscopic examination, to a committee consisting of Drs. Tyson, Parry, and Ingham. At a subsequent meeting they reported as follows:

REPORT.

"The committee to which was referred the ovarian tumor presented by Dr. Ingham, would report that they have carefully examined its structure, and find it made up of loculi of different sizes from that of almost microscopic minuteness to that of an inch or more in diameter. The walls of these loculi were composed of fibrous tissue and lined with a single layer of a most perfect and regular columnar epithelium. From the walls of these loculi often projected villous extensions, capped by this same layer of columnar cells.

"The cavities of the cysts were filled with cholesterin plates and epithelial débris.

"From these appearances your committee concluded that the tumor is an ordinary cystic colloid of the ovary (cystoma ovarii), and not a sarcoma or carcinoma."

DR. ELLWOOD WILSON read the following reply to Dr. Goodell's paper of June 3d last, on

VERSION IN CONTRACTED PELVES.

PAPER III.

MR. PRESIDENT AND GENTLEMEN:

To the general tone of Dr. Goodell's reply¹ to my criticism on his "Clinical Memoir on Turning in Pelves narrowed in the Conjugate Diameter," I shall make no answer. It was written, as was stated, in haste, and abounded in expressions which its author, on leisurely consideration of the character of this discussion, would have doubtless expunged. But I desire to reply to some few points contained in it, that I may clear myself from the aspersions which Dr. Goodell, in the excite-

¹ Read before the Obstetrical Society, in June. See Feb. Number of this JOURNAL.

ment of his haste, and his impatience of criticism, sought to put upon me, and especially those in reference to the citation of authors.

In my paper I established the following objections to version as a substitute for the forceps:

1. The great mortality to the child in pelvic labors, arising from interruption of the foetal circulation by compression of the cord and placenta, and showing in pelvis contracted at the brim to three inches, or less than three inches, an almost universally fatal result.

2. That version is a painful and serious operation to the mother, proving fatal to her in one in ten and a fraction cases, according to Cazeaux, and one in fourteen according to Churchill. Even allowing for the alleviating influence of anæsthesia, the operation is a grave one, and not to be resorted to, save for well-understood and sufficient reasons.

3. That in pelvis of plus three inches and of three and a half inches, there exists no sufficient reason for subjecting the woman to this serious operation, since she may sometimes even be delivered spontaneously; and again, her delivery may be materially aided, and even accomplished, by increased and prolonged voluntary effort on her part at the moment when the head impinges upon the narrowed marginal brim; or, these efforts failing, the case is readily within the control of a master with forceps, with safety to the mother, and a general result favorable to the child.

4. That the forceps can be applied to the sides of the child's head at the brim, and when thus applied, by cautious interrupted compression and traction, together with the expulsive efforts of the mother, and the resistance of her tissues, the head can be so moulded as to diminish its transverse or biparietal diameter from four to ten lines without injury to the child.

5. That an enormous strain, certainly a tractile force of one hundred and thirty pounds, cannot with safety be thrown upon the neck of an unborn child.

To no one of these propositions has Dr. Goodell offered any rebutting evidence, save the last; and that I am wholly right on this point I shall be able to show by abundant testimony.

Dr. Goodell, in speaking of the mortality of head-last labors, says, "Hodge rates the average still-births at thirty-three per cent. According to Capuron and Cazeaux, in the more difficult cases from sixty-six to seventy-five per cent. perish. Said the late George T. Elliot, 'I always regret to meet a pelvic presentation in my practice, for fear that the child may not be born alive.'" Further, "Since now these statistics represent the experience of the most skilled specialists, . . . it stands to

reason that, in the practice of the profession at large, the average number of head-last still-births must be very much higher. For this mortality, fifty per cent. is, I think, a very low estimate."¹ He thus claims for pelvic natural labors a greater mortality for the child than I give in any quotation in my previous paper.

He further says, "My reviewer begins by showing the foetal mortality of head-last labors, and of those attended by contraction of the brim, but he overlooks the fact that I have offered a new mode of delivery, by which I claim that this mortality can be reduced."²

Now, to what does the doctor allude as this "new mode of delivery" in pelvic cases? Is it the supra-pubic pressure? Barnes writes, "Great assistance in extraction may be gained, and traction force economized, by getting an assistant to press firmly upon the vault of the head through the abdominal wall, thus helping to push the head through the strait. This proceeding was advised by Pugh and Wiegand, and quite recently Dr. Strassmann has insisted upon its utility. The possibility of deriving advantage from it should be borne in mind in all cases of head-last labor."³ Yet Dr. Goodell ventures to say, "So far as I can judge from the history of these cases" (referring to pelvic cases reported by Simpson, Schroeder, Blot, and Taylor), "none of the operators, *excepting myself*, invoked the very substantial help of a propelling force; hence, the inference is logical that the conjunction of traction and propulsion offers better results."⁴ The supra-pubic pressure clearly is not the "new mode of delivery," and Dr. Goodell is not warranted in judging that the writers he has named did not invoke an aid so simple, so well known, and so substantial. Or, is it his mode of making traction? We find this mode thus described: "The physician should make his first movement of traction in the axis of the outlet; for then the pubic side of the head will be tilted away from the inlet, while the sacral side will proportionately descend over the promontory, and affront the brim." Afterwards "the direction is reversed, and the child's body swept backwards upon the coccyx, the neck also being forced downwards and backwards into the hollow of the sacrum with all one's power."⁵ It appears to me that if the head, yet above the brim of a contracted pelvis, could be changed in its position by pulling upon the child's body, the movements described would necessarily carry that pole of the biparietal diameter

¹ Philadelphia Medical Times, March 20, 1875, p. 385.

² MS. of Dr. Goodell, read in June, which, for convenience of reference, I shall designate his Reply.

³ Obstetric Operations, 2d Amer. ed., p. 253.

⁴ Clinical Memoir, p. 21.

⁵ Clinical Memoir, p. 19.

looking towards the front of the pelvis against or upon the pubic bone; and with the reverse movement recommended, the smooth convex surface of the head, looking towards the sacrum, would, being moist and glib with the fluids, rise up and pass over the convexity of the jutting promontory. Both the first and the second movement recommended could of necessity result only from a force acting very obliquely upon the head, the tendency of which would be to produce a rocking or see-saw movement; but this rocking would be above the brim, and not tend to engage the head, unless great force be used,—such a force as could be greatly economized if exerted by direct traction in the axis of the inlet. According to Barnes, “it is, above all things, necessary to draw at first as much backwards as possible, so as to make the head revolve around the jutting promontory until it clears the strait, when the head can enter its natural orbit, the curve of Carns.”¹ He also says, speaking of the delivery of the head, “If you carry the body forward too soon, you simply convert the child’s head and neck into a hook, or crowbar, which, holding on the anterior pelvic wall, will effectually resist all efforts at extraction.”² Now, Dr. Barnes claims the experience of the management of two hundred cases of version,—Dr. Goodell, in his reply, says three hundred. But, supposing that Barnes was correct in his estimate, two hundred cases of version are certainly sufficient to give him great familiarity with the management of pelvic labors.

I believe, therefore, the best way to make the head engage would be to make traction coincident with the axis of the inlet. Statistics are, however, wanting to show that by either of these methods of delivery the fœtal mortality can be reduced materially below the estimates given in my previous paper.

In the Reply we find such statements as these: “I give reasons why an average head is more readily drawn through a narrow pelvis than a large head through an average pelvis.”

This is purely hypothetical. It is a sort of sliding scale, that may be moved up or down to please the fancy of the narrator.

Dr. Simpson advocates “turning.”

So he does, “as an alternative for craniotomy and the long forceps.”³ And he contrasts “the transient and not necessarily fatal depression of the flexible skull of the fœtus with the destructive and necessarily deadly perforation of it” (p. 452). Again, in speaking of the advantages of turning, Simpson says,

¹ Op. cit., p. 252.

² Ibid., p. 208.

³ Obstetric Works, first series, p. 449

"It is more safe to the mother, because it can be performed earlier in the labor, and more speedily than craniotomy or the application of the long forceps" (p. 465). Again, "When the long forceps are used in instances of arrestment of the head at the brim, they are in general never applied till the cranium is thrust and wedged down into the upper pelvic aperture to as great an extent as can possibly be effected by the unassisted uterine efforts" (p. 476). And yet again, "But it is principally as a substitute for craniotomy that I venture to suggest the operation of turning, etc." (p. 474). His book contains many like passages.

I fully agree with Radford, quoted approvingly by Dr. Simpson, that "whatever practice can safely supersede the murderous craniotomy should be adopted;" but while this is to be accepted as a most humane doctrine, it cannot be regarded as offering any justification for the indiscriminate resort to version in contracted pelves.

W. Tyler Smith¹ recommends version "in some cases of moderately contracted pelves," but says "it is a serious operation as regards both mother and child. In the performance of the operation uterine inflammation may be excited, the uterus may be ruptured or perforated, and the shock of the operation is fatal in some cases" (p. 669). He says the head may be compressed one inch, and quotes Denman for a greater amount (p. 680). In contracted pelves he says the head of the child is in a state of flexion, and applies the forceps to the sides of the pelvis when the head is at the brim (p. 696).

Meadows² is also quoted by Dr. Goodell as recommending version as a means of delivery in contracted pelves. Speaking of the advantages of version, he says (p. 237), "It is excluded only by the existence of a pelvic diameter of less than three inches." He quotes Dr. Churchill as giving a mortality to the mother of one in fifteen,—it should be one in fourteen,—and recommends version "in certain cases of pelvic deformity where craniotomy would otherwise be required;" adding, that "of course the pelvis must be sufficiently roomy to admit of delivery without injury to the child by laceration" (p. 240).

In speaking of version, he says, "And after bringing down the arms" (he, unlike Dr. Goodell, gives no direction for traction upon the child's body until the arms are down), "with some traction the head will probably soon descend; but should the passage of it be delayed, and supposing the child to be living,

¹ "The Modern Practice of Midwifery." A Course of Lectures on Obstetrics, 2d ed.

² A Manual of Midwifery, 1871.

we ought certainly to apply the forceps and terminate delivery" (p. 251).

Thus we see this distinguished author does not recommend a traction of "one hundred and thirty pounds" to be suddenly thrown upon the child's body by either a steady or a "pump-handle" movement. In referring to the use of the forceps he gives "the result to the mother, as far as can be gathered from the same source (Churchill), as with British practitioners one death in twenty-nine, with the Germans and French one death in thirty-four" (p. 211). As to the reduction of the child's head in its transverse diameter, he says, "as much as half an inch, and perhaps even three-quarters of an inch, can be gained without injury" (p. 215).

Milne,¹ also quoted by Dr. Goodell, says, "From statistics given by Dr. Churchill in his admirable book, it would seem that this operation (version) was fatal to the mother in the proportion of one in fourteen;" and "it is a strongly-contested point whether or not in cases of pelvic deformity, especially contracted brim, version should not be tried instead of the long forceps or even craniotomy" (p. 238). Further, in speaking of version in narrow pelvis he says, "In vertex presentations the chin lies to the sternum, and therefore the vertex and the biparietal part of the head enter the brim first; that is, with the larger diameter (the biparietal) opposed to the antero-posterior; in turning, on the contrary, the chin is extended, and the biparietal diameter enters the widest part of the pelvis. Version may be attempted at an earlier period than craniotomy, as a rule" (p. 239).

He also says, "The cases suitable for this line of treatment are those where the conjugate diameter is about three inches."

And in speaking of the dangers of version, he says,—

"1st. We are apt to lose children in turning, from compression of the cord or even extension of the neck; but the former, at least, as we shall notice in describing the method of operating, may be in part avoided.

"2d. The traction required may be so great as to contuse the maternal parts seriously.

"3d. We may miscalculate the diameters of the pelvis; and, after using strong traction, fail, and have to perforate after all."

He says further (p. 251), "The dangers to the mother are,—

"1st. Risk of lacerating the parts or rupturing the uterus.

"2d. Inflammation from the irritation, etc., of the operation.

"3d. Puerperal fever or phlebitis, from the same cause.

¹ The Principles and Practice of Midwifery, 1871.

“4th. Serious if not fatal nervous shock.”

Thus we find authors quoted by Dr. Goodell, in his Reply, stating that the vertex does enter in narrow pelves, flexed, and also acknowledging that there are great dangers in the operation of version, both to mother and to child. Both these statements were denied by Dr. Goodell in his reply.

But Milne says of the forceps, “Now we arrive at an instrument which it is impossible to land too highly, or adequately to estimate the good which it has achieved.”

From forceps delivery the “mortality to the mother was one in twenty-one and one-third” (p. 254).

“According to the French and German authors it was one in thirty-four.”

Leishman, another authority quoted in the Reply, advises version in “cases of moderate pelvic distortion” (p. 484). Now, any one carefully studying his directions for making version, and observing the dangers and difficulties which he mentions as liable to arise during the operation, could scarcely advise the young and inexperienced physician to undertake so important an operation without a consultation.

In treating of the management of pelvic cases, he says, “When the breech is born and the legs lie between the thighs of the mother, and in footling cases, even before the passage of the breech, an almost irrepressible desire may possess the accoucheur to grasp the limbs and bring the labor to a rapid termination” (p. 328). He then describes the disastrous results that are likely to result from such a course, and condemns such procedures in the following language: “This, then, is an obvious error in practice, which the young practitioner should carefully avoid.” Further, in speaking of the arms riding up upon the sides of the head, he remarks, “Under all circumstances it is an unfortunate complication, and requires, when recognized, immediate attention.” Thus we see that Dr. Leishman does not recommend forcible traction upon the child’s neck, nor even moderate traction, until the arms are first disengaged. Again, in speaking of traction for the delivery of the head, he says (p. 329), “Forcible traction of the neck is by no means free from the danger of causing instant death by injury to the spinal marrow.” And he adds, “If, however, the resistance is unusually great, we must, in preference to dragging upon the neck, apply the forceps without delay to the sides of the child’s head, and thus complete the delivery” (p. 330).

In Chapter XXVIII. (p. 448), in speaking of the forceps, he says, “It is scarcely possible to exaggerate the importance of this instrument, which is simple in construction, easy of application, and marvellous in power.” He says, “If possible, but

with exceptions to be afterwards noticed, the blades should be applied to the sides of the child's head" (p. 455). With the exceptions here alluded to, which refer to the applications of the instrument at the brim of the pelvis, the author just cited prefers to apply the blades to the sides of the pelvis; here I differ with him, and upon the evidence of much experience I declare that the forceps should always be applied to the sides of the child's head.

Also, "In so far as compression is concerned, a certain degree of this is essential in order to grasp the head with the blades, which otherwise would slip off; but by compression something more is implied than mere grasping, for by it, as is obvious from the yielding nature of the sutures and fontanelles, the actual diameter of the cranium may be materially diminished" (p. 459).

And "In cases in which the head is arrested by the pelvic contraction at the brim, we have three possible modes of action, among which we must select—turning, forceps, or craniotomy" (p. 492). Further, he says, "The dangers and difficulties of the long forceps are well known." Here we see the strong bent of the British obstetric mind as regards the dangers of what writers designate "the long forceps." It is an instrument that I very seldom use; but I have in many instances applied Davis's forceps with a pivot lock, as modified by Dr. A. H. Smith, and successfully delivered living children through pelves of three, and but slightly more than three inches. Yet, notwithstanding this adverse impression in the mind of Dr. Leishman in regard to the forceps, he states, referring to their use, "The first point of importance then is to determine whether or not the child is alive; and if, this being established, we fail to deliver by the long forceps, or that instrument is contra-indicated, the question before us simply is, Shall we turn or perforate?" (p. 492). The most earnest advocate for the forceps could ask no more than is here admitted.

He also cites Churchill, "who fixes the limits (for version) at two inches and six-eighths; and Barnes, as we believe with more justice, at from three and a quarter inches to three and three-quarter inches, so that we may say in round numbers that when the conjugate is less than three inches, to attempt to turn would be to subject the woman to needless risk, while we may be confident that nothing but failure could attend our efforts."

But the evidence given in my former paper clearly demonstrates that the child's head can be safely diminished in its conjugate diameter from six to eight lines, and even to a greater extent, by the judicious use of the forceps conjointly with the mother's efforts. Thus we may conclude, *à fortiori*, that in

most instances, as statistics show, the woman, with the aid of the forceps, may accomplish her delivery without accident or injury, so far as she is concerned, and generally with safety to the child, when the pelvis has a capacity of three inches or upwards.

Finally, Leishman, in speaking of craniotomy, says, "The conditions, then, which may be held as warranting the operation of craniotomy are those in which the forceps and turning are of no avail," here again giving the preference to the forceps (p. 500).

Cazeaux is also referred to by Dr. Goodell as an advocate for version. Let us follow him for a few minutes, and see what he says upon this important subject. "A patient presented herself at the clinique in 1858, whose pelvis was only two and three-quarter inches in its antero-posterior diameter. She was delivered in eighteen hours of a living infant at term, the dimensions of which were nearly normal, and whose head was scarcely deformed." Further, "Baudelocque relates (p. 558) having seen at the amphitheatre of Solayrès the head of a foetus, which was elongated to such an extent that its greatest diameter measured nearly eight and a half inches, whilst the biparietal one was reduced to two and three-eighths or two and three-quarter inches; and he speaks of another very similar instance; but in neither of these cases was the child's life compromised for a single instant." Again, "M. Martin, of Lyons, has known a rachitic woman to be delivered of a healthy infant at term by the efforts of nature alone, where the autopsical examination showed that the antero-posterior diameter was only two and a half inches." You will remember that Dr. Goodell denied this reducibility of the child's head, or the possibility of delivery at term, with a pelvis of two and a half inches, and, greatly to my astonishment, he questioned ("Clinical Memoir," p. 20) the truthfulness of Madame Lachapelle's statistics.

In my paper I called attention to the fact that, in pelves contracted at the brim, the head is generally transverse at the brim, and that the forces which direct the head in this wise will, so long as the membranes are intact and the child free to move in its long or spinal axis, necessarily tend to drive the vertex to the larger side of the pelvis whenever (and this is almost universal) it is unsymmetrical. And I pointed out the great advantage of seizing the head in this relation, when assistance in labor was required. Now, at p. 559, Cazeaux says, "In most cases of deformity, the child's position is far from being an indifferent matter, for when the sacrum, in being carried forward, is, at the same time, turned to one side, whereby one of the lateral portions of the pelvis is more contracted

than the other, who does not foresee that the labor may then be accomplished spontaneously, if the head presents in such a way as to offer its great occipital extremity to the well-formed side, and that, on the contrary, it would become impossible if the occiput should correspond to the contracted one?" Cazeaux here points out a great objection to version in pelves contracted at the brim; because, if the child thus situated is turned, and the version is made in the circumference of the foetal ellipse (as universally advised), the occiput must necessarily be brought to the smaller side of the pelvic opening, unless the child should be twisted upon its spinal axis in the act of version, which would be a very dangerous procedure.

Cazeaux also says, at p. 560, "If the pelvis have at least three and three-quarter inches in its contracted part, here the labor, although in general longer, more difficult, and therefore more dangerous, both for the mother and the child, than in ordinary cases, may, however, be accomplished spontaneously, and, indeed, we might hope for such an expulsion in most cases." And "a spontaneous expulsion of the foetus is still barely possible where there is from three and one-eighth to three and three-quarter inches in the contracted part" (p. 561).

He further quotes from Homberger a case in which "the sacro-pubic diameter was scarcely two inches in length; nevertheless, after having ascertained the flexibility of the bones, caused by the malacosteon, he declared that the delivery might be effected by the powers of nature. He ruptured the membranes at the end of twenty-four hours; then, after waiting as much longer, the engagement was sufficiently advanced to enable him to apply the forceps, when, by the aid of powerful traction, he succeeded in bringing away a girl, who lived four weeks." Also "in another woman, whose sacro-pubic diameter was two and a quarter inches, French measurement, at the most, Hasslocker, a physician of Landau, was enabled by the aid of external pressure [Dr. Goodell's new method] to make the child's head engage in the cavity of the pelvis; he then applied the forceps, and found that only a moderate effort was required to deliver a dead child weighing six pounds and a half." I mention this case as showing again that supra-pubic pressure is no novelty in obstetric practice, and in further illustration of the statement that in many instances of coarctated pelves the contraction is *merely marginal*, and that with such pelves women are frequently enabled by their unaided efforts to drive the head beyond the point of resistance.

I also give the account of five labors in the case of Mrs. —, stout or fleshy:

First labor, October 10, 1868. Vertex to left acetabulum;

twenty-five hours; male child, living. When the patient was lying upon her left side, slightly inclined towards her face, the index-finger readily touched the promontory, with slight pressure upon the perineum. I therefore regarded the diameter of the pelvis as about three and one-fourth or three and one-third inches.

Second labor, June 1, 1870. Vertex to left acetabulum; twelve hours; male, living.

Third labor, December 10, 1871. Breech; first position; twelve hours; male, living.

Fourth labor, October 29, 1873. Vertex to left acetabulum; sixteen hours; female, living.

Fifth labor, June 21, 1875. Vertex to right acetabulum; six hours; male, living; weighed nine pounds. In this labor the forceps were applied at the brim to the sides of the head; the child was born twenty minutes after the patient was placed in position for their use. The forceps were adjusted, the patient etherized, and the child delivered in twenty minutes. The child was not marked by the instruments, and I am quite certain I did not use a traction force of twenty pounds.

In all of her labors the pains were frequent and powerfully expulsive. By using the forceps as soon as the tissues were fully dilated, her labor was lessened fully one-half in duration.

Dr. Goodell has been at some pains to show that when women have been left in labor for thirty, forty, or sixty hours, and the "formidable long forceps" have then been applied over the face and the occiput of the child's head, or made to grasp the head in its fronto-mastoid relation, the women not only sometimes die, but also that such reprehensible procedures are frequently fatal to the child. Is this an argument in favor of version, or against the proper use of the forceps? Or, indeed, have such facts any value whatever in the present state of obstetric art?

Cazeaux remarks, "As a general rule, we may wait six, seven, or even eight hours after the membranes give way and after the os is fully dilated. It is unnecessary to add that if any accident whatsoever, grave enough to endanger the health of the mother or the life of the child, should occur during the labor, it would demand a more prompt interference of art" (*op. cit.*, p. 583). My own views are, that the time for using the forceps is as soon as the os uteri is fully dilated or sufficiently dilatable, the soft tissues relaxed, and it has been ascertained that the woman is not likely to accomplish the labor without assistance. I have on several occasions even adjusted the instruments to the sides of the child's head at the brim without rupturing the membranes. Certainly both the health of the

mother and the life of the child would be endangered before the woman had been in hard labor for sixty hours.

He remarks, "With a pelvis of three and three-quarter inches, when the vertex presents, and the amniotic waters are partially discharged without the head making any progress, an application of the forceps is the only remedy to which we can resort" (p. 583).

In referring to pelvic presentations, he says (p. 584), "We expressly recommended that no traction should be made on the pelvic extremity in breech presentations with a view of avoiding the straightening out of the arms and an extension of the head, and we still insist on the same precept here." Thus Cazeaux differs essentially from Dr. Goodell, and gives special directions for the avoidance of two complications liable to occur in pelvic labors, namely, the rising up of the arms and the extension of the head,—complications which Dr. Goodell seems to disregard, because he courts the extension of the head and disregards the rising up of the arms, even directing us to proceed to deliver, when the pelvis is ample, without restoring the arms. "In cases of pelves known to be ample, I can conceive of its being perfectly justifiable to follow Giffard and Frouiep's plan of dragging the head through with the impacted arm extended, rather than that of losing golden minutes in liberating it."¹ In my opinion, no more dangerous rule for obstetric guidance than this has been announced since the publications cited, and Dr. Goodell may share jointly with these gentlemen the honors derived from it.

Again, in speaking of labors with a pelvis of three and three-quarter inches, Cazeaux says, "If the uterine contractions are exerted on the child's body alone, or repeated attempts at extraction have been made without success, . . . if, in a word, the child's life has been compromised, either by the length of the labor or the useless intervention of art,—in all such cases it may be regarded, though still living, as non-viable, and craniotomy is considered by most modern accoucheurs to be the only possible measure. We ourselves held this opinion for a long time; but, being rather less fearful of the probable consequences of pelvic version in contraction of the pelvis, we now think that, so long as any chance remains in favor of the child, the latter operation (version) should first be attempted" (p. 586). We most cordially accept this lesson. Cazeaux merely says that when we have exhausted all other means and there yet remains a chance for the child, version should be attempted before resorting to craniotomy. We have in the

¹ Philadelphia Medical Times, March 20, 1875, p. 386.

above quotation nothing adverse to the skilful use of the forceps in such labors.

He further states, in speaking of pelves of three and three-quarter inches, "After having waited for all that can reasonably be expected from uterine contraction, the forceps are to be applied when the vertex presents favorably, and if with moderate traction they are found to be insufficient, the instrument should be withdrawn and pelvic version attempted, in the hope of extracting a living child." Here again we have a preference for the forceps. "Should this attempt prove fruitless, the contractions may be allowed to go on for an hour or two longer, and if these are ineffectual the instruments are again to be applied." This is the course recommended before resorting to craniotomy. Further, Cazeaux continues, "When the pelvic diameters afford but two and three-quarter inches to three and three-quarter inches, the indications to be fulfilled remain the same."

We see by the above directions that the preference of Cazeaux is strongly in favor of the forceps; and that these failing and the child being yet alive, he humanely recommends that version should "be attempted," hoping thereby to save the life of the child. Again, in speaking of the forceps, "The blades should be applied as nearly as possible on the sides of the head in such a way that the concavity of their margins shall be directed towards that part of the head which is to be brought under the symphysis pubis" (p. 801).

Again, referring to the application of the forceps when the head is movable above the superior strait, he says, "It has elsewhere been shown that whenever the smallest diameter of the pelvis amounts to three inches, there is reason to expect that delivery can be effected by means of the forceps (p. 813).

When the head is movable above the brim, the author prefers the application of the instrument to the oblique diameters of the head. He states in another place, "In fact, the contractions usually exist at the superior strait, where it is particularly apt to affect the sacro-pubic diameter; and as the head always has a tendency to present its long diameter to those of the pelvis, when retained above, it is generally found in a transverse or an oblique position (more frequently the former). Its biparietal diameter will therefore correspond to the smallest one of the strait, and of course the blades of the forceps should be applied in the direction of this diameter; but we have shown that such an application is not possible in any case, and this impossibility is still more evident when contractions exist" (p. 821).

He therefore admits that if the forceps could be applied to the sides of the head, they would act by compressing the diameters of the head at fault. We have seen that they *can* be thus applied. He says the dangers to the child are from too rapid compression. I recommend that they should be used with caution and delay. I may thus claim to have answered or removed the principal objection which Cazeaux urges against their use in contracted pelves. He even makes the following admission in favor of forceps: "When the smallest diameter of the contracted pelvis is less than three inches, we are still almost obliged to try the forceps before having recourse to craniotomy or symphyseotomy" (p. 822).

"And it has several times been the means of extracting a living child through a diameter of but two and three-quarter inches."

Moreover, I quote Dr. Goodell's Reply: "We of this city are flourishing our cephalic applications of the forceps in the face of the English pelvic applications;" and again, "But while numerous facts seem to prove that in very narrow pelves the forceps cannot compete with version, I am sure that its range of usefulness can be very materially widened by that cephalic application of the blades, viz., to the sides of the head, which is practised by the best obstetricians of this city."¹

The reason why he (Cazeaux) changed his views of practice, he acknowledges, was owing to views brought forward by Simpson and Radford, namely, that when the head engaged in the pelvis by its base, it could be more readily drawn through the narrowed pelvis than when it presented by the vertex. This view of Dr. Simpson's is not accepted by Hodge, Leishman, and McClintock; and certainly I cannot accept it.

Cazeaux also says, "If on the contrary we suppose the cone represented by the head to engage by its point, that is to say, by its bimastoid diameter, the traction upon the body of the child might have the following effect, namely: if the shortened pelvic diameter presents at least from two and three-quarter inches to three and a quarter inches, it will present no serious obstacle to the engagement of the bimastoid diameter, and from that time the compression upon the sides of the parietal protuberances produced by the resistance of the symphysis pubis and sacro-vertebral angle tends to force them nearer together, that is to say, to shorten the biparietal diameter, and the head drawn down by the accoucheur will engage in the contracted part of the pelvis like a wedge, the base of which is compressible; in short, the resistance of the bones of the pelvis in the presentation of the top of the head, tends to lessen the occipito-frontal or occipito-

¹ Clinical Memoir, p. 22. "

mental diameter, whilst in foot presentations it tends to diminish the transverse diameter, that is to say the only one which it is important should be reduced" (p. 823, Simpson). Here then is the great argument in support of version: namely, that it affords a means of making traction upon the body of the child in such a manner as may enable the resistance of the sides of the pelvis to act upon and compress the lateral surfaces of the child's head so as to lessen the conjugate or biparietal diameter; "that is to say, the only one which it is important should be reduced." Had these gentlemen conceived the possibility and tested the feasibility of applying the forceps to the sides of the child's head when at the brim, they would then have accomplished just what every experienced obstetrician acknowledges, with Cazeaux, to be the great consideration in the management of labor with coarctated pelves. With the forceps (properly applied), this lessening of the conjugate diameter is effected without danger to the mother, merely aiding her in her natural efforts, while at the same the child is subjected to far less risk.

In speaking of the biparietal and bimastoid diameters of the head, Cazeaux says this latter diameter is irreducible, whilst the former is susceptible, under the influence of pressure applied for a longer or shorter time, of being shortened to the extent of three-eighths, or even five-eighths, of an inch, and, quoting Bandelocque, "half an inch" (pp. 821-823).

Cazeaux then quotes Madame Lachapelle and others, as showing that version "affords us at least the hope of sometimes saving the child," and remarks that "notwithstanding the facts collected by that illustrious midwife, and whilst admitting with the English accoucheurs that the compression is less dangerous to the fœtus when exerted on the sides of the head than when its tendency is to shorten the occipito-frontal diameter, we confess that we cannot share their preference when the top of the head presents in a favorable position. The arrest of the base of the cranium above the contraction, the possible extension of the head, the stretching of the cervical region to which the tractions made on the body necessarily expose it, the possible compression of the umbilical cord during the time occupied in the extraction of the child, are, indeed, very unfavorable circumstances for the latter, and, unfortunately, greatly to be feared during version. But when, with a shortened diameter of three and a quarter inches, there coincides an unfavorable presentation, as those of the face or the trunk; and when, before the application of the forceps, it is first necessary to perform the cephalic version; or when, the top of the head presenting, it is so situated that its longitudinal diameter corresponds to the contracted one

we are of their opinion, and prefer version to the use of the instruments" (pp. 824, 825). In his allusions to the reference to these statements of Cazeaux in my review of the "Clinical Memoir," Dr. Goodell, in his Reply, takes an unwarrantable license in language.

Cazeaux remarks, "If, after several fruitless attempts made with the forceps upon a favorably-situated head, the heart is heard to beat distinctly and regularly, we should, if the pelvis has at least two and three-quarter inches, attempt the pelvic version before resorting to craniotomy" (p. 825).

He sums up thus: "To recapitulate, when the pelvis has at least two and three-quarter inches in its sacro-pubic diameter, the forceps should be used if the top of the head presents in a transverse position. The pelvic version should be preferred: 1st, in direct antero-posterior positions; 2d, in inclined or irregular positions of the top of the head; 3d, in face and trunk presentations." Pardon these lengthy quotations; they are produced to sustain the brief allusion of my former paper.

Dr. Goodell quotes Barnes as recommending version in pelvis narrowed at the brim. Let us follow for a few minutes the lessons of this teacher, and see to what extent and under what circumstances he advises version. In speaking of obstetric instruments, he makes use of such expressions as these: "For example, the noblest of all, the forceps." It is not saying too much to pronounce the invention of the forceps one of the greatest improvements in the history of obstetric science; and although at times dangerous to the mother or child, yet its life-saving power is so great as to cancel the evil results which have often followed its use. Thus we bring within the saving help of the forceps a further number of children that must otherwise be given up to the perforator or run the risk of turning" (pp. 28, 29). "It appears to me quite certain that in this country [England] we are yet far from having utilized the powers of the forceps to the highest legitimate extent" (p. 37). By a careful study of their instruments and their mode of applying them, the correctness of this observation becomes very apparent; and to this cause, I presume, must in a large measure be attributed Dr. Simpson's great fear of what he designates as the "formidable steel clamps."

Professor Barnes emphasizes the following rule: "*Whenever the long forceps will lock without force, it may be reasonably concluded that the case is a fit one for the trial of this instrument, and a reasonable attempt should be made to deliver by its aid before passing on to turning or perforation.*"

This embodies the whole subject now under discussion. And

¹ Barnes's "Obstetric Operations," 1874.

as an advocate for the masterly use of the forceps, I find no objection thereto. Certainly this proposition of Dr. Barnes cannot be regarded as an endorsement of the indiscriminate resort to version.

Further, he says, "As head presentation is the type of natural labors, it follows that to obtain a head presentation is the great end to be contemplated by art" (p. 164).

Referring to anæsthetics as a means of diminishing the suffering, etc., of version, he says, "These advantages are not, indeed, always obtained without drawbacks. A perfectly flaccid uterus indicates considerable general prostration, and predisposes to flooding" (p. 186).

In reference to the riding up of the arms upon the sides of the child's head in pelvic labors, he says, "Hence often arises a serious delay in the descent of the head; for this, the most bulky and least compressible part of the fœtus, increased by the thickness of the arms, forms a wedge which is very apt to stick in the brim. This is one great reason for not putting on extractive force if it can be avoided. If, however, we find the arms in this unfortunate position, we must be prepared to liberate them promptly, and at the same time without injury" (p. 202). After discussing the mode of bringing down the arms, he emphatically says, "That is what has to be done, but it is indifferent which arm you shall bring down first" (p. 203).

In speaking of version as a means of sometimes saving children, he says (p. 237), "If children were sometimes saved, many mothers were injured or lost by attempt to turn under circumstances which are now encountered successfully by the forceps or craniotomy."

Dr. Goodell, in his Reply, quotes from Dr. Barnes a statement of Pugh's, an advocate of the long forceps, to the effect "that by version and the curved forceps he had never opened one head for upwards of fourteen years. Dr. A. Nebinger, of this city, writes me that he has never performed craniotomy. I suppose he can claim an experience of thirty years' active engagement in obstetric practice. Dr. J. Eshleman also informs me that he has never performed craniotomy. Dr. Jos. Warrington had but one case in an experience of thirty years.

Barnes also refers to Prof. G. T. Elliot, who "expresses it as his opinion that in a conjugate diameter of three inches and upwards, with a living child and a head presentation, first choice should be made of the forceps, but when the diameter is between two and a half and three inches with a living child version is preferable." In the same excellent work are some valuable statistics of "German experience in version as an

elective operation in contracted pelves," which show that, as regards the safety of the child, the best results are obtained where moulding of the head is gradually accomplished by the uterine forces, but that where there is insufficiency of the latter the forceps is superior to version."

Dr. McClintock, quoted by Dr. Barnes, says, "I do not believe that the diameters of the head are more advantageously placed with regard to those of the pelvis, nor can I believe that the head is more compressible when entering the strait with its base than when it does so with its vertex, till this is demonstrated by direct experiment."¹ I fully coincide with Dr. McClintock. Dr. Barnes also quotes Churchill, who says, "A living child can pass through a pelvis of three inches and two-eighths antero-posterior diameter with or without the forceps. With a pelvis of this size, then, the operation is unnecessary; and if the antero-posterior diameter be less than two inches and six-eighths, the operation would be impracticable."² I saw, at 6½ p.m., August 5, 1875, in consultation, a rickety patient at full term of pregnancy, and in labor since 10 a.m.; her pains had been regular. At 5 p.m., she had been etherized, and Barnes's dilator used. The os was dilated to about two inches, soft and yielding. We directed grs. xxx crystallized chloral, to be given during the evening. At 1 a.m. the vertex, which presented to the left side, dipped into the excavation and turned posteriorly in the space behind the projecting promontory. The forceps were applied to the sides of the head, and a living child was delivered without much effort. It weighed six pounds and seven ounces. The occipito-mental diameter was five and a half inches; the biparietal, three and a half inches. The woman was aged thirty-two. This was her second pregnancy; with her first she miscarried at six months. She had been seen by several physicians. Her pelvis was regarded as generally contracted. In touching, the index-finger was readily brought in contact with the promontory; with two fingers firmly pressed into the vagina, the top of the pelvic basin on either side could be touched. The diameters of her pelvis were estimated to be as follows: antero-posterior, +3, probably 3½; transverse, 4, possibly 4½; bis-iliac, not exceeding 3.

Barnes, commenting upon the statements of Churchill (p. 244), says, "I claim to speak with confidence, drawn from large experience, when I say that a head of standard proportions and firmness will hardly ever pass a conjugate of three and a quarter inches without the forceps, and very rarely indeed with the

¹ *Obstetric Trans.*, vol. iv., 1863.

² *Pract. Mid.*, 1866.

forceps,—that is, alive. I might even extend the conjugate to three inches and a half, and affirm the same thing.”

Now, from my own knowledge and experience I can say positively that a living child can as a rule be born through a pelvis of + 3 or $3\frac{1}{2}$ inches, as the case I have just narrated in part shows.

Barnes further says, “I believe no one advocates resort to version when the conjugate measures less than three inches” (p. 244).

Again, “It is enough to state that the operation is not recommended by any one when the pelvis is contracted more than one-fourth, that is, below three inches” (p. 248).

He further declares his inability “to estimate the proportion of children saved or lost under the operation,” asserting that it is enough to justify the operation if we save a child now and then. And, “assuming a standard head whose base (unyielding) measures three inches, this is obviously the limit beyond which the operation would be useless.”

Further, “If the head should fortunately be under size or unusually plastic, there is a fair prospect of the child being drawn alive through a conjugate diameter measuring three inches [*i.e.*, by version], but generally from three and a quarter inches to three and a half inches, or even a little more, is the working range for a child at term.”

On p. 251 Dr. Barnes says, “The operation [version] may also be performed, as the complement to premature induction of labor, where the conjugate measures two inches and three-quarters to three inches and a half. Indeed, this I believe to be one of its most valuable applications.”

In alluding to the difficulties in version when the head cannot be brought through the contracted brim, he says, “Of course the mother may suffer if we persevere in dragging the child too long and too forcibly” (p. 245). Now, what shall be the limit of this dragging? Dr. Goodell says he has repeatedly thrown a force of one hundred and thirty pounds upon the child’s neck, with a pump-handle movement.

J. Braxton Hicks has been quoted as an advocate of version. After a careful perusal of his book,¹ I cannot find that he recommends anything like an indiscriminate resort to version in contracted pelves. To use his own words, he recommends version “where there is not too great a discordance between the foetal head and the maternal passages” (p. 1). He informs us that the practice of version was much interfered with when the forceps were introduced, and calls attention to “the great facil-

¹ Combined Version, 1864.

ity thus given the practitioner in delivering the head, and the marvellous advantages apparent to every one over the perforator and the hook, especially in cases where turning was impossible" (p. 2). "Happily, however, we can offer the fœtus another chance for its life, and can place turning before the perforator. By some practitioners turning is placed before the forceps; for *myself*, I prefer, as a rule, the forceps first" (p. 2). He is well aware of the dangers of version, as is evident from these extracts: "I again wish to urge the importance of not delivering the child with rapidity after the accomplishment of version" (p. 28). And, referring to the advantages to the child, "they are greater, provided we do not use force to extract it."

Look at these two cases from his book :

Case 13.—"Coarctation of the brim. Version. The head gave much difficulty while passing the brim, but eventually it was delivered without perforation." The child was still-born.

Case 14. Coarctation of the brim.—First, the woman had borne a living child at full term, therefore the obstacle at that time was not extreme; secondly, it was possible that the head had never been fairly applied at the brim; thirdly, if there was a greater facility for the passage of the head by breech delivery, it might possibly in this instance be successful in saving the child. "After version it was found that the head, however, on reaching the brim would not pass through until after much time had elapsed, and then only by perforation through the occiput and mouth" (p. 56). In truth, his first choice in labors with coarctated pelves is the forceps; he resorts to version very cautiously. There is clearly a doubt in his mind about the possible advantages of the head passing the brim with greater facility in breech cases than it does in cephalic cases; and finally, we may reflect upon the results of his cases, which do not offer much for version.

Dr. J. Eshleman writes me, "I have delivered by the forceps several children alive in diameters of three inches and three and a half inches." Dr. A. Nebinger also writes, that in his practice he has delivered living children at full term through pelves of three and a half inches, and that he has never delivered a woman of either a living or a dead child which passed through a pelvis of only three inches at the conjugate diameter of the superior strait, because he never had such a case occur in his practice. And in confirmation of this I may add as the result of my own observation that in this community we do not meet with many cases of contraction at the brim to a degree of less than three inches. And Schroeder says, "It has already been said that the simple flat pelvis is the most frequent of all

narrowed pelves, and doubtless more frequent than all the other varieties taken together, and is therefore a very frequent cause of dystocia; however, it does not show the highest degree of contraction, for its conjugate rarely measures less than eight centimetres, or 3.14 inches" (p. 238). In my previous paper I referred to Schroeder's statistics of version. "We [Schroeder] therefore recommend version in *all* cases of pelvic contraction where this is not absolute. Our reasons for doing so are: first, in the interest of the mother; second, in very great pelvic contractions, with a conjugate of $+7\frac{1}{2}$ centimetres ($2\frac{8}{12}$ inches), we have extracted live children; third, because perforation of the after-coming head is neither more difficult nor more dangerous to the mother than that of the presenting head" (p. 262). These "live children" were evidently very small, because immediately following this statement he says, "Occasionally we may have the pleasure of extracting a live child of moderate size through a conjugate which measures less than eight centimetres."

Dr. Schroeder, after stating that he always recommends version in contracted pelves, says, "When the conjugate measures not less than nine and a half centimetres, the head presents at the brim in the way before described (vertex). Whilst the os is very slightly dilated the cranial bones are made to overlap each other, and a caput succedaneum is formed; thus the head enters the brim, and the child is pushed through the pelvis, the os gradually dilating. In such cases, the mother, as a rule, does not suffer at all, and the child also is usually born in perfect health" (p. 258).

Now, Schroeder is certainly one of the strongest advocates of version. Yet he informs us that with a pelvis of nine and a half centimetres ($+3.7$ inches) the woman can accomplish her delivery without "*suffering at all*" and the "*child be born in perfect health*;" yet, as you may observe, it is about what Dr. Barnes represents as the "working range for version at term." This degree of contraction comes fairly within that class of cases which I have said are within the control of the natural efforts of the mother, or easily so with the aid of forceps.

Let me quote Schroeder further: "The reasons why we do not recommend the forceps in these cases" (contracted pelves) "are the following: the head enters with the sagittal suture in the transverse diameter of the contracted pelvis; but when the head is high up, the forceps can only be applied in the transverse diameter of the pelvis, and consequently the blades must grasp the forehead and the occiput" (p. 259). We cannot step back two centuries in order that this assertion of Schroeder's may be accepted as an unquestioned canon in obstetric practice.

Moreover, Dr. Goodell has himself positively asserted that the forceps should be applied to the sides of the child's head,—that is, at the brim. This has been for many years my own practice, and I have never known a patient to die from the use of forceps in my hands. If Schroeder understood the application of the forceps at the brim, as it is practised in this city, he might change his views as to the propriety of version in *all* cases of contraction not absolute.

K. Braun, of Vienna, is also said to be an advocate for version. There is reported in the *Philadelphia Medical Times*, February, 1875, a case from his clinic. The woman was a primipara, aged twenty-three years. Strong pains for one day. An examination with the finger gave an estimate of the pelvis at two and three-quarter inches or, at most, three inches. Version was made. There was great difficulty in disengaging the arms; but it was impossible to extract the head, even after the child was ascertained to be dead, and delivery was effected by the trepan and cranioclast. I am not able to say whether, in contracted pelves, version is always the first choice of Braun or not; but the result of this case is a practical confirmation of the teachings of Hodge, Leishman, Cazeaux, Churchill, Elliot, Barnes, and J. B. Hicks. Dr. Goodell quotes Hodge, alluding to cases where women are delivered with pelves of three inches, as saying, "Such cases are of very rare occurrence. Few accoucheurs have been permitted to see them." Let me add what Dr. Hodge says in immediate connection with this statement: "Statistics, we believe, have thrown no special light upon this point, and we have a right to conclude that such deliveries are exceptional, and should not be the foundation of a practical rule. In other words, practitioners ought not to commit such cases of labor, where there is but three inches in the short diameter of the pelvis, to the unaided efforts of nature."

It is, however, generally admitted that a child may be born alive by the natural efforts through a pelvis of three inches. Dr. Hodge, referring to the dangers to both mother and child in labors with contracted pelves, very emphatically advises against prolonged delay. "In view, therefore, of these imminent dangers to the child, and even to the parent, the suggestion of waiting any length of time when there is but three inches in the short diameter of the pelvis cannot, we believe, be justified" (p. 400). And "in cases where there is a disproportion between the head of the fœtus and the passage of the pelvis, demanding more power than can be exerted by the mother, the forceps has been instrumental in saving a large number of chil-

dren which otherwise would have inevitably perished," etc. (p. 401).

We have seen that Hodge, in his "Obstetrics," positively asserts that children can be born at term through pelves of three inches, and that the delivery in such cases may be greatly facilitated by the forceps. A later paper¹ contains the following more exact statement: "The occipito-mental diameter may be elongated from five to six or six and a half inches. As to the lateral diminution of the head, it is usually stated at from three to six lines; but as well-developed fœtuses have been born alive where the conjugate of the brim has not measured more than three inches, we have a right to infer that the biparietal diameter would be lessened to six or eight lines" (p. 4).

Further he says, "In addition, experience proves that no matter how much pressure is made upon the cerebral mass during labor, even for a long time, the child may survive, provided there is no solution of continuity of the tissues and no internal extravasation" (p. 8). And after a full examination of the causes of death to the fœtus in pelvic labors, he refers to forceps defectively constructed, and to the accidents likely to result from their use, saying, "No wonder then we read of such horrible lacerations of the uterus and vagina—accidents which ought never to occur with a well-constructed instrument" (p. 9).

As to the successful use of the forceps as a compressor, we find him advising that "no unnecessary delay should be permitted. As soon as it is ascertained that the natural powers are inadequate, artificial means should be employed" (p. 12), and "all action with the forceps should be intermittent and in unison with the uterine efforts. Constant traction would keep up constant uterine action, which, interfering with the respiratory powers of the placenta, would be dangerous." Further, "Another important rule is, that the blades should be as nearly coincident as possible with the occipito-mental diameter of the head, as compression will then be made in the most favorable direction" (p. 13). Indeed, Dr. Hodge prefers that the forceps should be always applied to the sides of the head, except when it presents obliquely or transversely at the brim; and this is his only exception to the rule. Thus, "in no case, therefore, in our opinion, should the blades of the forceps be ever applied directly to the pubis and sacrum" (p. 259). Dr. Goodell, in his Reply, is mistaken when he says that I quoted Hodge as advising the application of the forceps to the sides of the head *when at the brim*.

¹ On Compression of the Fœtal Head by the Forceps and Cephalotribe.—*Am. Jour. Obst.*, May 1875.

With moderately contracted pelves, Dr. Hodge says, "It is very universally true, when the process of labor is not interfered with, that either the chin or vertex presents; but, as already remarked, it is not the top of the head, but the occiput, that presents; for the top of the head to present is *contra naturam*."

"The presentation of the vertex, therefore, is more favorable for delivery than the presentation of the base of the cranium, the length and compressibility of the occipital extremity being greater than that of the sides. No argument, therefore, for version by the feet can be drawn from the supposed advantages of the wedge form of the sides of the head" (p. 16).

Dr. Lusk, in a *resumé* of German practice published in Elliot's "Obstetric Clinic," covering many thousand cases, states that after making every allowance for doubtful cases the mortality of children in deformed pelves is thirty per cent. in vertex cases, and thirty-six per cent. in version cases. Dr. Lusk concludes that the experience in Germany is decidedly in opposition to Simpson's views.¹

Dr. McClintock, of Dublin, and Dr. Martin, of Berlin, both favor occipital rather than pelvic deliveries in contracted pelves (p. 18). I cannot avoid the conclusion in view of all these facts and opinions, that in cases of contracted pelvis where there are at least three inches in the conjugate diameter, the delivery by suitable forceps is far more safe for the mother, and that the mortality to the child is at the same time less.

Hodge then contrasts delivery by the pelvic operation: "No time is allowed for the gradual compression and moulding of the cranium. Powerful traction must be made by the muscles, superadded, according to some, by the weight of the accoucheur, all of which power is directed through the medium of the child's neck, injuriously to its tissues, and not infrequently there is a complete laceration of it, so that the practitioner sometimes finds himself prostrate, holding the decapitated trunk in his hands; certainly this is not in accordance with science based on natural laws" (p. 19).

Dr. Goodell has questioned my statement that Dr. Meigs applied the forceps to the sides of the child's head. I refer him to Meigs's *Obstetrics*, p. 547: "The blades are to be applied to the sides of the head, the extremities of them passing up nearly as far as the chin."

And p. 554, "The instrument being now adjusted over the sides of the child's head."

¹ Hodge's paper, p. 17.

Or p. 553, "It should not be forgotten that the forceps embraces the head in a direction from the vertex to the chin."

In referring to the application of the forceps before rotation is completed, he says, "The first and one of the most important steps here is to ascertain accurately, I say with absolute accuracy, the situation of the foetal head" (p. 557). He then explains the importance of this to be, that the operator may *adjust the blades to the sides of the child's head*. Thus we see that he not only recommends the application of the forceps to the sides of the head, but also, in common with all other operators with the instrument, to make compression. And we find on p. 557, "Inasmuch as we cannot exert any very considerable tractile force without compressing the head with a severity proportionate to it," and so on.

Let me add once for all that I have repeatedly assisted the late Professor Meigs in cases in which he applied the forceps at the brim to the sides of the child's head.

Dr. Goodell attempts to show that Dr. Warrington does not teach the application of the forceps to the sides of the child's head at the brim, because he makes it a condition that the physician should be skilled in their use, and if not skilled in their use he had better make version.

This reasoning is on a par with Dr. Goodell's allusion to Baudelocque's celebrated remark, "that it (the forceps) has been more injurious than useful to society." We all know that this remark of Baudelocque's refers only to the ignorant and unskilful use of the instrument. Now, Dr. Goodell may not be aware that for ten years I assisted Dr. Warrington in his lecture-room, and that I am altogether familiar with his teachings and his bed-side practice. I must simply reaffirm that he taught and practised the application of the forceps to the sides of the child's head at the brim of the pelvis;¹ and elaborate directions for this mode of application, when necessary, are to be found in his writings.

Dr. Goodell refers to a case occurring in the practice of Dr. Fox, and reported by Meigs: "Dr. Fox, the attending physician, called in Drs. James, Meigs, and Lukens. Dr. Physick was called on, but was sick. Dr. Dewees was called, but was absent from the city. Dr. Hewson was added to the consultation, making five prominent physicians who saw the case." I shall not detain you with a lengthy narrative, because the case is familiar to every novice in the profession. 1st. Dr. Fox found there was a "sensible deformity." 2d. Drs. Fox and James found "from as accurate measurements as we were

¹ See Warrington's Obstetric Catechism, pp. 253, 259-60.

capable of making, we came to the conclusion that there was not at most three inches in the antero-posterior diameter. Drs. Meigs and Lukens saw her, as per appointment, by this time. After repeated and the most accurate examination that the case admitted of, we were unanimous in the opinion that there was not more than two inches in the antero-posterior diameter; most probably only one inch and three-quarters."

Dr. Goodell says, "It does not appear that any of them tried to apply the forceps." No, they did not, it is true, nor did they, so far as we can find, attempt version. Dr. Meigs delivered the woman on this and a subsequent occasion by craniotomy, and informs us that "this patient again became pregnant; that the child presented by the breech, which would make delivery *per vias naturales* absolutely impossible," and because of this complication she was delivered by the "Cæsarean operation;" and again in a fourth pregnancy, with success. I do not see in the report of this case anything adverse to the use of the forceps in pelves of three inches and upwards, and there is no allusion to version; on the contrary, when it was found that the child presented by the breech, these eminent physicians abandoned the case to the "Cæsarean section." But it does show that five distinguished physicians, after the most careful examination of the pelvis repeatedly made, were greatly at a loss in their estimate of its capacity, which they variously found to be from three to one and three-quarter inches in diameter; yet when I allude to the possibility of Dr. Goodell's being mistaken in the nice fractional estimate (to 2.82 inches) of his ninth case, he considers it a discourtesy.

Again I quote from the Reply: "But granting that Siebold, Velpeau, and others could reduce the biparietal diameter when the blades were accurately adjusted to the sides of the child's head, what advantage did this give to those of them who in narrow pelves applied the blades to the occipito-frontal diameter?" By the first proposition stated we gain all that art can give for the relief of the difficulty under discussion,—namely, the greatest possible facility of compressing the head in the direction in which it is most yielding, and in those relations in which compression according to all experience is found to be least dangerous to the child. And this in co-operation with the natural efforts of the mother. Baudelocque says, "I have taken children whose heads seemed to have lost nine or ten lines of their natural thickness in passing the superior strait; also the heads of several of these children were above six and a half inches, and even seven inches long, while the thickness from one parietal protuberance to the other was but two and a half to two and three-quarters, and in some, three inches" (p. 398).

And in a foot note he says, "Solayres informed us one day in his lectures, that he had taken a child the evening before whose head, at the moment of its birth, was eight inches all but two lines, measured from occiput to chin, while it had preserved but two inches and five or six lines in thickness."

Simpson remarks that "Of all directions the lateral is the one in which the child's head can endure the greatest degree of compression with the least degree of danger." He also quotes Ramsbotham for the same opinion: "A full-grown foetal head may be lessened from side to side, without endangering the child's life, one-seventh of its own extent, or from three inches and a half to three inches," and adds, "We have just now seen that Dr. Denman imagines that the amount of compression may be carried much further,—namely to one-third—without the life of the child being necessarily injured and destroyed."

We find Hodge saying, "In one case of Dr. Denman the depression of the parietal bones measured one inch."

I have seen it reduced ten lines, and he himself states that the head may be safely compressed in its conjugate diameter from six to eight lines, and gives a case in which it was reduced ten lines. Most authors admit that from six to eight lines is a safe range.

Dr. A. Nebinger writes me that in his practice he has observed that the head may be reduced in its biparietal diameter by the efforts of the mother, aided by compression and traction with the forceps, without injury to the child, from half an inch to one inch.

We have here abundant evidence that the child's head can with safety be greatly reduced when the compressing force is applied in its lateral direction. And this is a convincing argument in favor of the application of the forceps to the sides of the head. It may be claimed that this is no less an argument in favor of version than of the forceps; that the reduction is the result of the efforts of parturition rather than of the forceps. Not at all. With the head presenting, the forces of the mother are immensely more efficient than they can be after version has been made; while, with the head presenting and the forceps properly adjusted, we add to the maternal forces a new compression and a tractile force, not inferior in degree, yet vastly safer than that which after version can be brought into play,—a compression and traction interrupted, graduated, made at the moments of uterine contraction, desisted from in the intervals of rest. I have used the forceps in this way eight hours, with, as the result of my patience, a living child and an uninjured mother,—a result which the brilliant operation of

turning would not have reasonably allowed me to expect in the case.

To the second question, namely, "What advantage did this give to those of them who in narrow pelves apply the blades to the occipito-frontal diameter?" I reply that this is not the way to adjust the instrument. Dr. Goodell himself cannot indorse it. It increases the danger to the child, and aggravates the difficulty that the forceps is used to overcome.

Dr. Churchill¹ gives the mortality to mothers resulting from version, as follows (p. 312): "In 2939 cases where the result to the mothers is specially mentioned, 212 mothers died, or nearly 1 in 14."² He limits the use of the forceps to three inches as a minimum diameter.

Dr. Goodell alludes to the liability of the cord to become prolapsed while applying the forceps at the brim, and makes this an objection to the operation. We admit that it is possible for this accident to occur as a complication; but such an occurrence is purely accidental. He says, "I have met with it in the ratio of twice in three cases." This is a remarkable frequency. I have seen but two cases of prolapse of the cord occurring in forceps cases in thirty years, and neither of these cases occurred in contracted pelves.

Dr. Goodell is mistaken when he says that Mrs. McN., whose case I gave, was a patient of the Philadelphia Lying-in Charity.

As for the maternal death-rate in cases under the care of this Charity, every care is taken to have all cases fully reported. It is remarkable that, upon the representation merely of ignorant women applying at the Preston Retreat, of which they supposed him to be chief almoner, Dr. Goodell should discredit the records of any institution.

Dr. Goodell has quoted Schroeder as saying that "all the various steps of version are devoid of danger to the mother," and adds, "So they are when the operation is one of election, as in turning in narrow pelves." This statement is given to demonstrate that version is unattended with dangers to the mother. Further on in his Reply, when he wishes to show that to turn and deliver a woman immediately upon her entering the Preston Retreat (Case viii. of the Memoir) is not a rapid labor, he says, "Now, Cazeaux, and with him all obstetric authors, define a rapid labor to be one in which the woman is delivered with only a few pains. But why is version resorted to in narrow pelves? Because the woman has been long in labor, with perhaps violent expulsive pains, and cannot deliver herself; because the forceps have been used in vain. Does this comport

¹ System of Midwifery. Condie, 1866.

² In my previous paper I said 1 in 14.

with the definition of a rapid delivery?" Let me ask, Was this the course pursued in Cases iv., v., and viii. of the Memoir? By no means. These cases are in strict accordance with Cazeaux's definition of rapid labor,—are indeed typical of rapid deliveries, and, as such, are liable to *post-partum* hemorrhages, swoonings, and shock, which may be fatal.

Dr. Goodell also speaks of squeezing the head with the forceps "viciously." I am not able to find such directions for using the forceps anywhere in Meigs's writings.

Simpson informs us (p. 451) that he turned and delivered in two or three minutes,—Dr. Goodell accomplished the same thing in three minutes. That such rapid labors predisposed to *post-partum* hemorrhage has been shown. All writers on obstetrics teach that *post-partum* hemorrhage is a serious and often fatal complication to the parturient woman, and also that she may die from nerve shock. Many illustrative cases are to be found in the journals of the past few years, as, for example, the case reported by Dr. George Mayles (in a recent number of the *Obstetrical Journal of Great Britain and Ireland*, p. 405), in which the woman died two hours after delivery; and every student of midwifery is familiar with the famous case of the Princess Charlotte. Indeed, the list might be increased indefinitely.

Are we to believe that the rupture of the perineum is a trifling accident? Are we to accept, on the evidence of a single case, the bold statement "that in nine out of ten cases of rupture into the rectum, the forceps will be found to have been the cause"? The evidence is altogether insufficient. Are we then to infer that this is the result of Dr. Goodell's personal experience? If it is, he has indeed been unfortunate with the forceps. Perhaps it is the result of observation at the bedside of those cases to which he has been called in consultation. Then the physicians who had managed the labors did not always use their forceps skilfully. Surely, if nine out of ten cases of rupture into the rectum have been caused by the forceps, some one has blundered. After all, it is merely an opinion that Dr. Goodell has ventured. In science, facts are better, or opinions derived from the observation of facts. In the absence of facts, I protest against such a sweeping condemnation of the profession or of the instrument, which is much more judiciously and dexterously used by the profession than Dr. Goodell seems to suppose.¹

[¹ Dr. Wilson's paper, as well as the whole discussion on "Version in Contracted Pelves," comprising a final paper by Dr. Wm. Goodell, and one by Dr. R. Stewart, will be concluded in the next number.—ED.]

CORRESPONDENCE.

THE REAL AUTHOR OF THE OPERATION OF GASTRO- ELYTROTOMY.

TO THE EDITOR OF THE AMERICAN JOURNAL OF OBSTETRICS, ETC.

DEAR SIR: In the report of his exceedingly interesting case of artificial delivery in a deformed pelvis, published by Dr. Alex. J. C. Skene, in the February number of your journal, the doctor is in error regarding the author of the method so successfully resorted to by him. Near the bottom of page 637, he says: "At nine o'clock I performed gastro-elytrotomy, according to the method of Prof. Thomas—the best, indeed, the only authority on this operation;" and in concluding his report, he says further: "I trust that the history of this case, (the first successful one on record,) will aid in placing the operation among the chief triumphs of obstetrical surgery, to the honor of Prof. Thomas, whom I believe to be its author."

Now that Dr. Skene's success and the publication of his paper are likely to attract attention again to the method, it is but fair that Drs. Ritgen, Physick and Baudelocque should receive the credit which is due them for having first suggested it; and, in the case of the latter, planned and attempted its execution. In the paper by Prof. Thomas on this operation, contributed to the third volume, (May, 1870,) of your journal, he refers fully to the literature of the subject, and his remarks are included by Dr. Dawson in the American edition of Barnes's work on "Obstetrical Operations" (p. 302); so that Dr. Skene has no good reason for giving to Professor Thomas the credit of originating an operation, which the latter is careful to lay no claim to himself.

Very truly yours,

FREDERICK A. CASTLE, M.D.

New York, February, 1876.

REPORT ON THE PROGRESS OF GYNECOLOGY DURING THE YEAR 1875.

By

PAUL F. MUNDÉ, M.D.

No one subject in the domain of Gynecology has received such universal and deserving attention during the past year, both from special investigators and the profession generally, as *THE PHYSIOLOGY OF MENSTRUATION AND ITS RELATION TO OVULATION*.

The investigations of Pouchet, Coste, Bischoff, Brierre de Boismont, Meckel, Pflüger, Raciborski, Tyler Smith, Ritchie, and others, although they developed many important points, still left the nature of the physiological changes taking place in the uterine mucous membrane at the time of menstruation, and the essential character of that supposed physiological function enveloped in almost as great an obscurity as ever. For the last twenty years the generally accepted theory was that, at the time of the catamenia, all the pelvic organs, and especially the mucous membrane of the uterus, became intensely congested; that at the height of this hyperæmia a Graafian follicle was ruptured, and an ovum discharged simultaneously with the desquamation of the intra-uterine epithelium and a flow of blood from the uterus; that this ovum passed through the Fallopian tubes into the uterine cavity, where, if not impregnated, it was lost, unless its fecundation caused it to adhere to and engraft itself on the denuded surface of the uterine mucous membrane. The uterine hyperæmia and hemorrhage, and the discharge of an ovum from a Graafian follicle were supposed to be contemporaneous and indissoluble processes; in fact, menstruation was assumed to be due to the successive evolution and periodic discharge of ovules from the Graafian follicles, without which it could not recur. The cause for the alleged periodicity of ovulation, for the supposed monthly elaboration of ovules, was classed by most writers among the "impenetrable mysteries of nature"; a few endeavored to explain the phenomena by comparing it with the other regular events occurring at stated periods in the human life; such as the occurrence of first and second dentition, the growth or fall of the hair, etc. (Meigs.) The ovum was thus held to have been discharged during the preceding menstrual flow, and belonging to it; therefore, of course, conception was thought to be most probable immediately after the cessation of the flow, while the ovum belonging to the menstrual epoch just completed was still on its way to the uterus, and had not yet had time to become lost in the meshes of its newly-developing mucous membrane. This period of special susceptibility was supposed to last from ten to twelve days after the cessation of the catamenia. (Bischoff, Coste.) This theory was certainly very plausible; so much so, indeed, that a very great number of excellent observers at the present day are loth to depart from it—as, for instance, one of its originators, Prof. Bischoff, of Munich, himself. But, unfortunately, in the course of years, a number of incontrovertible facts have appeared, which cannot be made to correspond with it, and which prevent the indissoluble connection of menstruation and ovulation from being received as a law. A few of such facts are the following:

1. If a Graafian follicle ruptures during every menstruation, we should find evidences of that rupture at the post-mortem of every woman dying during or soon after the flow. But that is not universally the case; accurate observers, such as Coste, Ritchie, Ashwell, Paget, Bischoff, Williams and others, have failed to find such evidence.

2. Ovulation frequently takes place without menstruation, as when conception takes place during lactation, or in women who have never yet menstruated; indeed, some authors (Scanzoni) assert that ovulation continues during pregnancy. Doubtless ova are also discharged at indefinite periods between the catamenial epochs.

3. Of late years the cases have been increasing in number, in which, after the operation of double ovariectomy, menstruation continued regularly for years, often up to the ordinary climacteric age. Goodman, in an excellent paper, from which I have largely drawn in preparing this sketch, (*Richmond and Louisville Medical Journal*, December, 1875) has collected twenty-seven cases of this kind, in ten of which menstruation was in no wise affected by the operation; in one the flow was increased, in one diminished, in two it occurred at irregular intervals. To attribute the continuance of the flow in these cases to the "force of habit," as has repeatedly been done, is but giving a very unsatisfactory and unscientific explanation of a mysterious phenomenon; every "habit of recurrence" (Goodman) such as that most familiar one of regular defecation, has an antecedent cause, which can be logically analyzed and explained. Where is the cause for menstruation, when the ovaries, in which the supposed impetus of that function, ovulation, has its seat, have been removed? Surely it must be sought for somewhere else; and as menstruation can still occur after the removal of both ovaries, what is more natural than to assume that it never depended on ovulation?

In view of these serious and insurmountable objections to this old theory, and the great scientific and practical importance of the subject, it is not strange that numerous attempts have of late been made to unravel the mystery. Following in the lead of Power, Gusserow, Sigismund, Löwenhardt and others, original investigations on the normal structure of the uterine mucous membrane, and the changes it undergoes during the menstrual act, have been reported during the past year by Engelmann, of St. Louis, Williams, of London, and Barnsfather, of Cincinnati; and papers have been written on the subject of menstruation generally by Goodman, of Louisville, Schroeder, of Erlangen, Bischoff, of Munich, Mayrhofer, of Vienna, Lombe Atthill, of Dublin, Simpson, of Edinburgh, King, of Washington, Studley and John C. Peters, of New York, and Parvin, of Louisville.

The importance of an accurate knowledge of THE NORMAL STRUCTURE OF THE UTERINE MUCOSA, AND ITS PERIODICAL CHANGES to a rational study of the nature of menstruation is obvious. DR. JOHN WILLIAMS' paper with the above title (*Obstetrical Journal, Great Britain and Ireland*, February and March, 1875) is based on the examination of twelve uteri, taken from women who had died of typhoid fever, pneumonia, or pleurisy (9), and by accident (3) at different stages of the intermenstrual period, and forming, as he says, a complete series. Williams believes that "the menstrual flow is not a process complete in itself, but the terminal change only of a cycle of changes, which begins at the cessation of one menstrual flow, passes through the developmental changes of the mucous membrane of the uterus, and ends with the cessation of the flow next following." He revives the old theory of Pouchet (1842), that the mucous membrane of the uterus is entirely cast off during the catamenial flow, and is re-developed in the intermenstrual period; and describes the changes going on in the mucosa, as seen by him in his twelve specimens, as follows: While the fatty degeneration and complete disintegration and removal of the uterine mucosa is going on during the bleeding period, the subjacent muscular wall, which would otherwise be left bare, develops an active proliferation of its elements, the muscular fibres producing the fusiform cells, the connective tissue the round cells, and the groups of round cells in the meshes formed by the muscular bundles the glandular epithelium of the

new mucous membrane. This development spreads from the internal os upwards towards the fundus; and, at the end of the third day after the cessation of the catamenia, the lower two-thirds of the body; at the end of a week, the whole of the body of the uterus is covered with a thin mucous membrane and lined by columnar epithelium. An abrupt distinction between the mucous membrane and the muscular wall is observed, first near the cervix, at about the tenth day of the intermenstrual period; it gradually spreads over the whole corpus uteri, reaching the fundus a short time before the expected return of the menstrual flow, at which time the mucous membrane is at its highest degree of development, and ready to receive the impregnated ovum. (Aveling's *Nidation*.) Now, if impregnation does not take place, fatty degeneration of the mucosa commences, and rapidly spreads over the whole membrane down to the muscular wall. Uterine contractions now occur, and drive the blood forcibly into the weakened vessels of the mucosa, which rupture, and the sanguineous flow, known as menstruation, makes its appearance. The membrane now undergoes rapid disintegration, and is removed cell by cell (not in pieces), the removal lasting from three to eight days in different individuals. This disintegration affects the vessels as well as the surrounding tissue; they rupture, and hemorrhage takes place from them, until the whole membrane is discharged. (Aveling's *Denudation*.) These changes take place only in the cavity of the corpus uteri. "Menstruation, then, is neither a congestion nor a species of erection, but a molecular disintegration of the mucous membrane of the uterus, followed by hemorrhage."

To controvert the palpable objections to this anomalous theory of the complete development of a tissue from a heterogeneous stratum—of *mucous* membrane from *muscular* tissue—Williams, in a later paper (*Ibid*, November, 1875), states, that subsequent investigations have revealed to him that the mucous membrane of the body of the uterus is possessed of a very highly developed muscularis; that is, that "a considerable portion of the thickness of the uterine wall is formed by *muscularis mucosæ*," a slight modification of the somewhat extravagant opinion formerly expressed by him (*London Obst. Soc. Trans.* Vol. XVI.), that the uterus is not an organ possessing a mucous membrane, but a mucous membrane with highly developed muscular fibre-cells.

In another recent communication (*Proc. Royal Society*, No. 162, 1875), DR. WILLIAMS reports his conclusions on the RELATION OF THE DISCHARGE OF OVA TO THE TIME OF MENSTRUATION, drawn from the examination of 16 cases; namely, that the rupture of the follicle, and discharge of the ovum occurs, as a rule, before the appearance of the monthly flow with which it is connected. In 12 out of these 16 cases, the follicle had ruptured *before* the return of the catamenia; in one it was doubtful whether rupture of the follicle or the flow would have occurred first; in two a menstrual period had passed without maturation of a follicle; and in one the catamenia were imminent, although the ovaries contained no matured Graafian follicle. From the examination of Reichert, in Berlin, of 23 cases, in which the genital organs showed signs of menstruation, it would appear that the rupture of the follicle takes place at an early stage of the menstrual flow; the general impression has been hitherto that it occurs towards the end, or immediately after the cessation, of the hemorrhage. Reichert's observations would seem to confirm those of Dr. Williams, since it is not at all impossible that, in the 18 cases in which rupture of the follicle and menstrual hemorrhage were found coincident, the rupture took place before the hemorrhage. (Of the five other cases, in four the follicle had matured before menstruation began, one follicle being on the point of rupture; and in the fifth case the follicle had actually ruptured.)

According to the *Obstetrical Journal of Great Britain and Ireland* (Editorial, July, 1875), DR. BARNSFATHER, of Cincinnati, confirms Dr. Williams' views regarding the monthly shedding of the uterine mucosa. He says, that for a number of years he has been examining microscopically, from month to month, the menstrual discharges of women; and, in all cases, he has found

exfoliation of the mucous membrane, even from perfectly healthy females. In dysmenorrhœa, the membrane was hypertrophied, and came away in large pieces. Tyler Smith, years ago, came to the same conclusion as Williams and Barnsfather.

The investigations of DR. GEORGE J. ENGELMANN, of St. Louis, were made in conjunction with DR. HANNS KUNDRAT, then assistant to Prof. Rokitsky, in Vienna, in 1871-72, and later by Dr. E. alone, in Berlin, and were published originally in Stricker's *Medizinische Jahrbücher*, in 1873. They attracted much attention at the time, and Dr. Engelmann, finding himself since then unable, for want of leisure, to continue and complete his researches, finally decided to republish the article, in a modified form, in accordance with various new views developed after his departure from Vienna. This was done in the *Am. Jour. of Obstetrics* for May, 1875. Dr. E.'s investigations cover a much wider field than those of Dr. Williams. His conclusions are based on the examination of 17 uteri, containing ova in all stages of pregnancy, numerous healthy ova (29 less than a month old), 200 other uteri, either virgin, menstrual, post-abortionum, or post-partum; and a number of pre-puberic uteri. These specimens were examined with reference to the following points: I. The development of the uterine mucous membrane up to the time of puberty; II. The condition of the uterine mucous membrane during the period of maturity and functional activity, from the time of puberty to the change of life, (a) the fully developed membrane during its period of rest, (b) the uterine mucosa during the menstrual period, (c) the mucous membrane of the uterus during pregnancy, the decidua (development and structure of the placenta, retrograde metamorphosis of the membranes, decidua vera, decidua reflexa, decidua scrotina, expulsion of the decidua, decidua of abortion, decidua in uterus bicornis and extra-uterine pregnancy), (d) the regeneration of the mucous membrane after parturition; III. The mucous membrane after the change of life. Only the first portion of Dr. E.'s paper, as far as (c) and Chap. III., concerns us in this Report; the remainder belongs under the head of Obstetrics.

The mucous membrane of the womb, before it has attained functional maturity, consists merely of round or polygonal cells, with round nuclei, imbedded in a very fine network of connective tissue; an exceedingly delicate, ciliated epithelium lines the surface. No glands occur in it during the first three or four years, when the growth of the surface epithelium, from without inward, in the form of small crypts, shows the first sign of their development. These crypts increase, especially during the seventh and eighth years, forming narrow ducts, and the formation of the uterine glands is completed shortly before puberty. During its period of rest, the fully developed uterine mucosa appears as a delicate, grayish-red layer, about 0.04 inch in thickness, closely attached to the muscularis, in the absence of a sub-mucosa. It is composed almost entirely of perpendicular tubular glands, mere epithelial tubes, with cylindrical, ciliated cells, and no basement membrane; the glands are enclosed in a fine mesh-work of connective tissue, the fibres of which radiate outward, and are lost in the inter-fibrillar septa of the muscularis, thus forming a firm connecting link between the two structures. Muscular fibres Dr. E. was never able to discover, even in the deepest layers of the mucosa. During the *menstrual period*, the uterine mucous membrane presents an entirely different appearance; it is swollen to 0.118—0.236 inch in thickness, almost pulpy in consistence, its surface puffy and wavy. This swelling of the membrane is due to a proliferation of the round cells of the stroma, and an enlargement of the individual cells of all kinds, the tumefaction being limited to the superficial layers of the mucosa. The glands themselves are very much enlarged, and assume a very tortuous course. A new formation of blood vessels could not be observed, but those already existing were enlarged, and gorged with blood. After the menstrual membrane has reached a certain degree of tumefaction, the upper layers undergo a moderate degree of fatty degeneration, which involves, not only the cells of the interglandular tissue, but also the blood vessels and the glandular and surface epi-

thelia; this superficial fatty degeneration, Dr. Engelmann pronounces to be the cause of the hemorrhage, which he has always found confined to the surface of the mucous membrane. He states that the facts gathered by him warrant him in concluding that the uterine mucosa begins to increase in thickness and succulence as the time of menstruation approaches, that this tumefaction is most marked during the period itself, and gradually decreases after the cessation of the catamenial discharge. *In not one of the many uteri examined at such periods, was the mucous membrane, or even its superficial layer, found wanting.* The only evidence of retrograde metamorphosis observable, is the destruction and detachment of a large portion of the surface, and some of the glandular epithelium, (not of the entire surface, however). A completely normal inactive uterine mucosa is seldom met with, an indication that the actual period of rest for that membrane is much shorter than generally assumed. In this particular, he somewhat inclines to Williams' view, that there is no such thing as a period of uterine rest, the menstrual epoch being merely the diminution of a period of extreme uterine activity, during which the uterus was being prepared for the reception of the impregnated ovum; after the catamenia, the uterus at once sets to work again to build up a new membrane, only to discharge it at the next monthly cycle.

As regards the relation of the time of the discharge of ova to menstruation, Dr. Engelmann dissents both from his co-laborer, Kundrat,—who, in their joint article in 1873, had independently declared himself in favor of the views of Löwenhardt (*Arch. für Gynäkol.*, III, 1872) to the effect that the impregnated ovum did not escape at the menstrual period last preceding conception, but shortly previous to the menstrual period following, at which only tumefaction of the mucosa occurred, but no discharge—and from Dr. Williams, who has come to the same conclusion as these two observers. Engelmann says that this theory would make the duration of pregnancy three-fourths of a menstrual period or lunar month shorter than is usually accepted; and, as Löwenhardt claims that conception cannot take place during the four or five days immediately preceding menstruation, the follicle must be assumed to rupture some time before the menstrual flow. He, (E.) therefore, referring to several specimens of his own, and to those cited by Dalton and Michel, pronounces his conviction that menstruation and ovulation are simultaneous, and declines to depart from this old and apparently well-founded theory. He infers from his specimens that the Graafian follicle generally ruptures toward the close of the catamenial period.

To the atrophy of the mucous membrane after the change of life, with its concomitant fibrous and obliterative changes, I need scarcely refer. Suffice it to say, that the glands disappear, the cells contract, and the formerly succulent membrane becomes thin, smooth and hard.

It will thus be observed that Dr. Engelmann virtually supports in every particular the old theory enunciated at the commencement of this *resumé*, and the careful accuracy of his investigations and statements certainly entitles them to the highest consideration. He characterizes Dr. Williams's apparently equally positive and careful observations, as establishing a physiological improbability (the monthly desquamation) supported by a histological incongruity (the monthly reproduction of the mucosa from the muscularis); his chief objection, however, to W.'s conclusions is, that they are all drawn from pathological specimens, all but three of his cases having died of acute inflammatory affections, and one with a fibroid, cause of death not stated; and that W. has therefore described purely pathological changes.

Williams, however, (*l. c.*, Nov. 1875) meets these objections of Engelmann's by asserting that the latter never examined a case in which death took place at or near the menstrual flow; that in none of his cases was the precise date of menstruation known; and that, therefore, not even probable conclusions can be drawn from such investigations.

Judging merely from analogy with other physiological processes, I should be inclined to agree rather with Dr. Engelmann, as supporting the more probable view of the two; but it would be very unjust to condemn Dr. Williams's

elaborate and logical deductions on this ground alone. Only precise future investigations and careful records of the post-mortem appearances in numerous cases of death during or near the menstrual period will decide the question, and give the victory either to the desquamative or the involutive theory. A wide field of research and a glorious chance of achieving a world-wide reputation is here open to any man whose good fortune enables him to utilize intelligently the fruits of a large number of autopsies of women dying at all periods of their reproductive life. For my part, thus far, my studies on this question do not permit me to agree with the statement made in the Editorial in the *British Obstetrical Journal* (*l. c.*), that at "present the preponderance of evidence is with those who believe denudation to be an act of desquamation."

Having disposed of the two most important papers published on this topic during the past year, I will now turn to the other essays referred to, none of which occupies itself with original investigations, all being devoted to a more or less complete and lucid review and discussion of the nature of menstruation and its relation to ovulation.

The physiological character of the menstrual function is unhesitatingly acknowledged by all writers, with one exception—that of Dr. Kiug, to whose paper I shall refer particularly hereafter. About this point there is no dispute; the unsettled question is: Has ovulation anything to do with menstruation; and, if not, what causes menstruation?

Neither Dr. Williams nor Dr. Engelmann endeavor to lift the veil from either of these mysteries; indeed, their papers did not pretend to cover this ground. Dr. JOHN GOODMAN, of Louisville, however (*l. c.*), has attempted, with great ability, to give a tangible solution of the problem. He declares his rejection of the ovular theory of menstruation, on the grounds already quoted above, and numerous others which he does not mention; but expressly states that he does not mean to deny all influence to the ovaries in determining the menstrual function; for observation has taught us that the presence of these organs is absolutely essential to its primary establishment; and where they were either congenitally absent, or had been removed before puberty, no pubertic development and no menstruation appeared. The function once definitely established, however, the determining influence of the ovaries appears to be no longer necessary, and menstruation will often go on regularly after their removal, as after double ovariectomy. (The same opinion is expressed by Dr. J. T. Gilmore, of Mobile, in an article on "Normal Ovariectomy," in the *Atlanta Med. and Surg. Journal*, Sept., 1874. He looks upon the menstrual flux as a secretion, and the uterus as a secreting organ.) As the maturation of ova reacts, in some mysterious manner, on the nervous system, and produces changes in different parts of the body—the pelvis, breasts, and adipose tissue—the same takes place in the male, with the evolution of spermatozoa in the testes, as shown by the growth of hair on the face, etc.), thus constituting apparently an "inherent power in the economy," so we may consider the "law of monthly periodicity dominating the menstrual function, also an inherent power in the animal economy, simply stimulated into activity by the ovular erethism." This law of monthly periodicity is inherent only to the human female, and, possibly, the female of some of the higher mammals. According to Girdwood, each genus of mammalia has its own catamenial cycle—the rabbit, 10 days; the mare, a fortnight; the cow, 3 weeks; the dog, 12 to 16 days. In woman alone does the "dynamic cycle of physiological acts" constituting menstruation, and governed by the above law, revolve every 28 days. The cause and seat of these physiological acts, is to be sought for neither in the ovaries (Tilt), nor in the uterus, nor in the blood (Paracelsus, De Graff), for the absence of both ovaries and uterus does not arrest the action of the law; and the case of the Hungarian sisters, in whom the blood-vessels were united at the loins, thus giving them a common circulation, but whose menstrual periods were different, disproves the last theory. Neither can Tyler Smith's hypothesis, of a reciprocal action between the uterus, ovaries, and mammae, as a cause of the monthly cycle, be maintained,

since two of these organs may be withdrawn with impunity. The undoubted seat of the functional process by which the monthly cycle is accomplished, is the ganglionic nervous system. In the course of the systemic development taking place in the young girl, at the time of puberty, a development dependent on a modification of the nutrition of the parts involved, presided over by the ganglionic nerves, the elements of the law of periodicity are elaborated, and the series of changes constituting the monthly cycle are instituted. The reason why the law declares itself through the circulation, is, because it, like nutrition, is controlled by the ganglionic system. This, Dr. Goodman says, is the limit to which inductive reasoning will carry us at present; the discovery of the nature of the changes he leaves to the anatomists (such as Williams and Engelmann), and of the part of the sympathetic system in which they occur to the experimental physiologists—another wide field for investigation. This theory certainly is a very ingenious one, and ably worked out. Its chief defect is, perhaps, the lack of precision necessarily accompanying it.

PROF. CARL SCHROEDER, now of Berlin, in the article on NORMAL MENSTRUATION, in Vol. X. of *Ziemssen's Encyclopædia*, on *The Diseases of the Female Sexual Organs*, considers the views of Kundrat and Engelmann exceedingly plausible, and appears to incline decidedly to the modern theory of Löwenhardt and others, that the ovum is discharged prior to the occurrence of the menstrual flow to which it belongs, and not subsequent to it. He does not, however, subscribe to the conclusion that ovulation and menstruation are entirely independent of each other, agreeing therein with Engelmann, and disagreeing with Beigel (*Krankh. der Weibl. Sexual Organe*, Vol. I., Erlangen, 1874), who maintains that ovulation may occur at any time, even in children, but that menstruation is nothing but the periodically returning demand of the female genital organs for sexual gratification. Schroeder considers it clear that the two phenomena have no necessary and absolute connection, and that one may very well occur without the other. It is perfectly conceivable that, with some monthly congestions, no follicles may ripen, and consequently no ova are discharged; and that, in other cases, an ovum is elaborated, but no laceration of the vessels of the uterine mucosa takes place. But the theory of their complete independence of each other must be rejected. According to the modern view, menstruation is no longer the indication of the greatest afflux of blood to the genital organs, but must be regarded as a retrogressive stage of the menstrual epoch. The essential event, of the periodical genital congestion is not menstruation, but the discharge of an ovum; the escape of blood is merely an accessory occurrence, perhaps only the sign of retrograde metamorphosis of the mucous membrane. Schroeder does not appear to look upon the calculation of the duration of pregnancy, according to the theory that the impregnated ovum escapes shortly before the expected menstrual period, at which time no hemorrhage takes place, as so impracticable as Engelmann, and seems to think the deductions already drawn by Löwenhardt, from clinical observations, borne out by these recent observations.

PROF. BISCHOFF, of Munich, whose name is associated with the earliest investigations on this subject, in a series of papers contributed to the *Wiener Medizinische Wochenschrift*, Nos. 21–24, 1875, pronounces himself as still unequivocally in favor of the old theory so long advocated by him. The time of menstruation in mammals is also the time of mensturation, and the maturation and discharge of ova. Conception, without menstruation, may occur, but only as a rare exception, which proves the rule; regular metrostaxis, after removal of the ovaries, says B., is a habit, acquired after many years of typical congestion and hemorrhage. Coition at the end of an intermenstrual period may be fruitful, because the spermatozoa may remain alive in the genital tract for eight or nine days, and may, therefore, meet the ovum discharged at the next menstrual epoch; a matured and discharged ovum, from analogy with animals, is fecundable until the middle of the intermenstrual period, and is imprægnatable only in the Fallopian tube, not in the uterus. B. and

Leuckardt have ascertained, by statistics, that there is an intermenstrual period unfavorable to impregnation, dating from 12 to 14 days after the catamenia, till the next are due. The new theory of the pre-menstrual impregnation of the ovum is declared very plausible by B., and appears to explain the well-known fertility of the Jews, notwithstanding the strict observance by the orthodox classes of the Levitical Law, which forbids sexual intercourse during the seven days preceding and following menstruation. Although Kundrat and Engelmann's and Williams' anatomical investigations appear to support this theory, B. does not think that Kundrat's researches conclusively prove that the mucosa, at the time of hemorrhage, is so far destroyed as to prevent the fixation of the impregnated ovum after the cessation of the flow; and, as for Williams' idea, he says it may easily be reconciled with the old theory—thus: While the ovum, which has been discharged during the shedding of the mucosa, passes slowly through the tube, that membrane has become regenerated; and when coition and impregnation take place, the uterus is fully adapted to receive the ovum. If impregnation does not occur, of course the ovum is lost, and the mucosa goes on to its usual destruction at the next period. B. says that it were strange if man should make the sole exception to the universal law of the animal kingdom, that a fruitful coition always relates to an ovum already detached from the ovary, or in process of immediate discharge; and persists in maintaining that the order of the factors of generation—ovulation, formation of menstrual decidua, hemorrhage, coition, and conception—is the true one.

The interest taken in this discussion is further shown by the fact, that the Presidents of two Obstetrical Societies have chosen the subject of "Emmenologia" for their inaugural addresses;—DR. ALEX. R. SIMPSON, at the Edinburgh Obstetrical Society, Dec. 8th, and DR. LOMBE ATTILL, at the Dublin Obstetrical Society, Nov. 20, 1875 (*Obst. Jour., Gr. Br. and Ire.*, Jan., 1876). Dr. Simpson's address, being the most comprehensive, shall be considered first. After describing the influence of climate, race, and social condition, on the advent of menstruation, the nature of the discharge, its quantity, duration, frequency, in all of which particulars there is so much diversity as to render a general average almost impossible, he considers the ancient theories and the source of the discharge. Dr. S. seems to occupy an intermediate ground between Engelmann and Williams; for he says that, during the stage of decline of the menstrual flow, when a mucous or muco-purulent discharge follows the sanguineous exudation, a distinct layer of the uterine mucosa has moulted, or been shed off, leaving a fretted surface, in which the follicles can be detected by close examination with a fine gelatinous layer of connective tissue about them, "through which may sometimes be seen what has been shrewdly described as the muscularis mucosæ; *i. e.*, the innermost layer of the muscular walls of the uterus." On this layer of mucous membrane that is left, a process of repair at once begins, and is completed in a few days.

The key for these changes is to be found in the ovaries; the expansion of the Graafian follicle produces irritation of the ovarian nerves, and then of the central ganglia, whence it is reflected to the sexual apparatus as a gradually increasing hyperæmia, until the follicle ruptures and the ovum is expelled; if not then, and probably also there amalgamated with the spermatozoa, it drifts into the uterus, where the surface has possibly already begun to melt away, and is washed away with the menstrual discharge. "There has been a birth of an unimpregnated ovum." [DR. JEROME COCHRAN, (*Va. Med. Monthly*, Feb. '76) also looks upon menstruation as the monthly abortion of an unimpregnated ovum.] With some such close relationship in time and function, will the ovary discharge its ovisac, and the uterus shed its surface, period after period, as each new follicle rises to maturity. It is easily comprehensible how, besides climatic, racial, and other general conditions, the rate of rapidity with which each follicle ripens and bursts, will depend on individual and special conditions in the ovaries themselves. Dr. Simpson thus believes in the pre-menstrual elaboration of the ova, and explains the mode of impregnation of such an ovum, in face of the acknowledged improb-

ability of fruitful coition within a week of the commencement of menstruation, as follows. During the pre-menstrual week, the uterine mucosa is so swollen as to block up the uterine canal, and prevent the upward migration of the spermatozoa; during the two post-menstrual weeks, however, the whole canal is more easily permeable, especially during the first days of this period, before the uterine mucosa has become regenerated. If coition now takes place, at any time during the first two or three weeks after the cessation of menstruation, the spermatozoa can freely pass up through the uterus and Fallopian tubes to the ovary (we know that in the mucus of the healthy female generative passages, they can retain their vitality for some time), there to meet the ovum at the moment of its escape from its follicle, immediately prior to the menstrual era. If the time is well chosen, and amalgamation of spermatozoa and ovum actually takes place, the menstrual period duly comes, but without hemorrhage, and the fecundated ovum passes on to the uterine cavity, to take up its abode in its well-prepared nest. If the meeting does not occur, both ovum and spermatozoa perish, and the uterine mucosa sheds its surface layer, and bleeds. From this programme, it is evident that the period of agensis (impossible impregnation) is during the pre-menstrual week, a period shorter only by two days than that claimed by Bischoff and Leuckardt (duration of menstrual flow, on an average, 5 days; genetic period, 14 days=19 days; agenetic period, 9 days=28); these two, however, attributed the agensis to another cause; namely, to the death of the ovum discharged at the last menstruation, and the impossibility of the spermatozoa meeting a new ovum until after its discharge during the next menstrual period. Simpson's theory is so clear and plausible, that I feel very much inclined to accept it. The point would be, however, to ascertain whether a coitus during the pre-menstrual week really never proves fruitful.

DR. LOMBE ATHILL's address, although it refers to the investigations of Williams, and points out the present standing of the question (Dr. Athill compares the menstrual flow to the lochia of pregnancy—a discharge mainly composed of the effete materials of the intra-menstrual period), treats chiefly of several points mainly interesting and valuable from a therapeutical point of view. I shall, therefore, discuss them briefly hereafter.

DR. JOHN C. PETERS, in a paper on "THE PATHOLOGY AND TREATMENT OF AMENORRHEA" (*Virginia Med. Monthly*, Nov., 1875), speaks of the monthly moulting of the uterine epithelium (not the whole mucous membrane), sympathetically and exactly coincident with the epithelial development in the ovisacs and ovulation, and says that menstruation may possibly occur without ovulation, provided the necessary changes take place in the uterine epithelium. In order to produce menstruation, ovulation, or, at least, epithelial growth in the ovisacs, or a healthy growth and decay of epithelium in the uterus, or, at least, congestion of the ovaries and uterus, will have to be produced.

An article by PROF. CARL MAYRHOFER, of Vienna, on "THE CORPORA LUTEA AND THE TRANSMIGRATION OF THE OVUM," has been running through a long series of numbers of the *Wiener Medizinische Wochenschrift* during the past year, and, as yet, shows no signs of approaching completion. I am, therefore, compelled to defer its discussion to my next annual report.

A brief review of the foregoing pages shows us that the emmenologists are at present divided into three main parties: I. The desquamationists—those who believe in, or incline towards, the theory of the desquamation of the entire uterine mucous membrane at the menstrual epoch, the pre-menstrual discharge of ova, and the occurrence of impregnation at that time, and not after the discharge, as hitherto maintained; and the entire independence of menstruation and ovulation after the former process has once been inaugurated—(Williams, Aveling, Lombe Athill, Barnsfather); II. The involutionists—those who maintain that only the epithelial, or surface layer, of the mucosa is shed during menstruation, that the ova are discharged during or at the close of the menstrual flow, and that impregnation is then

most likely, and that menstruation is directly dependent on ovulation, exceptions only proving the rule—(Engelmann, Bischoff, Peters); and, III. Those who assume portions of each theory, and reject others: Involutionists, adherents of the theory of pre-menstrual discharge of ova, dependence of menstruation on ovulation—Schroeder, Simpson—believers in total independence of ovulation and menstruation—Goodman, whose article does not refer to the anatomical changes in the uterus, and the temporal relations of ovulation and menstruation. As only the four gentlemen mentioned above have declared their adherence to Dr. Williams' theory, it is evident that thus far, not the modern desquamationists, but the old involutionists, are in the majority; for the greater portion of the profession must be considered to belong to that party. While in this respect no innovation has been made, the new theory of pre-menstrual ovulation and impregnation, and the total independence of menstruation and ovulation, appears, on the other hand, to have been received with general favor, and to be steadily gaining ground.

The startling and adventurous proposition (although not original or novel, because already entertained by Rousset, Emmet, and Auber, years ago), that MENSTRUATION IS AN ABNORMAL PROCESS, was revived by DR. A. F. A. KING, of Washington, D. C., in a paper entitled, "A NEW BASIS FOR UTERINE PATHOLOGY," published in the *American Journal of Obstetrics* for August, 1875. He bases his assertion on twelve skilfully constructed and apparently logically well-founded facts, which, condensed, read as follows: 1. Menstruation is the result of an interference with nature. 2. In the majority of cases, it is accompanied by unpleasant symptoms, as is no other physiological process. 3. To preserve comfort and cleanliness, it requires the application of an artificial appendage to the person, a requirement belonging to no natural emunctory function. 4. Menstruation is a hemorrhage; it is attended with the rupture of blood-vessels; blood-vessels were not made to be ruptured; no hemorrhage is natural. 5. Although menstruation is desirable and necessary in celibate females, to relieve congestion of the uterus, it ought not to be considered physiological, any more than epistaxis, which relieves congestion of the brain, etc. 6. The menstrual periods are analogous with the periods of œstruation in animals, when ova are discharged, and coition is most likely to be fruitful. But the menstrual discharge prevents coition; or, if the latter be indulged in without precautionary measures, it may produce gonorrhœa in the male. The menstrual discharge of blood has no analogue in animals. 7. Menstruation does not occur in women belonging to the savage races, who live in accordance with nature, and untrammelled in their reproductive function by the usages of civilization. The Hindoo women do not menstruate. 8. History does not furnish unequivocal evidence that menstruation was common in ancient times. 9. Can those women who have borne large families without ever menstruating, be considered sick? Must they not rather be considered as following a more strictly natural course, as regards reproduction, than their sterile and menstruating sisters? 10. If puberty, when the organs are fully prepared to fulfil the procreative office, is not the natural period for reproduction to begin, when else is that function to commence? 11. A menstruating female is peculiarly liable to congestion and inflammation from cold; no physiological function possesses such a liability; even during normal reproduction, a female can tolerate ordinary exposure. 12. Finally, the coincidence of puberty and the height of female beauty tells us plainly that this is the period for the beginning of the procreative office. Assuming the female to have attained an approximate or perfect development, with no inherited tendency to disease, and no injurious agencies acting on the reproductive organs, there can be no doubt, under these circumstances, that impregnation during one of the several ovulatory periods that usually precede the establishment of menstruation at the pubertic age, is strictly in accordance with nature, and the surest means of maintaining typical perfection, both functional and structural, of the reproductive organs. To prevent the occurrence of "the abnormality, menstruation," procreation would have to be continued every year or two until the menopause, necessitating, *apparently*,

the bearing of twenty or thirty children. But, says Dr. King, this does not follow, because the menopause would, in that case, probably occur much earlier than is now usual; and, consequently, the number of children would not much exceed the present average.

I would not for a moment be understood as supporting Dr. King's "pathological" idea; for it has been refuted, and, I think, conclusively; still, I confess that the thought had often occurred to me since I first learned to comprehend the symptoms of menstruation, that it was a strange provision of nature by which a purely normal and physiological process was instituted, which, even in a perfectly healthy woman, incapacitates her from the usual free exercise of her household and other duties, and exposes her to various dangers, besides being often accompanied by more or less suffering, during four or five days of every month of the average thirty-three years of her procreative life, and thus causes her to lose something over five years of her life in a certain degree of inactivity. Therefore, Dr. King's paper, showing, as it did, marked ability in the handling of the subject, was unusually interesting to me, and served to arouse my sense of the remarkable.

The exceptional oddity of his views left no doubt in my mind, when I accepted his paper for publication, that they would be widely discussed and severely criticised. This, Dr. King, I know, expected; and I, for my part, was perfectly willing it should be so, hoping that new facts, bearing on the vexed subject of menstruation, might be elicited thereby. Accordingly, two reviews of the "New Basis for Uterine Pathology," appeared soon after its issue, one by Dr. THEOPHILUS PARVIN, of Louisville (*Practitioner*, Sept., 1875); the other by Dr. W. H. STUDLEY, of this city (*American Journal of Obstetrics*, November, 1875), both of which gentlemen, comprehending the proper spirit in which such a proposition should be met, attacked and refuted Dr. King's philosophical and historical arguments with the same weapons.

In his desire to prove his point, Dr. King, unfortunately for his theory, neglected to ascertain whether all of his statements were actually founded on fact; and thus rendered it comparatively easy for his critics to dispose of his arguments; which Dr. Studley, whose review is the most complete, takes up and answers seriatim, as follows:

Argument 1. The mere declaration that a thing is so, is not logical evidence of the truth of the assertion.

Argument 2. Parturition also is a physiological process, and is surely neither latent nor painless; neither are the physiological wants, hunger, thirst and sleepiness, altogether free from unpleasant feelings. Why, therefore, should menstruation not be accompanied by some disagreeable symptoms?

Argument 3. The anus and urethra of a baby, the vulva of a puerperal woman, are natural emunctories; no one would think of omitting to supply them with an artificial appendage to receive their discharges; the same is the case during menstruation.

Argument 4. The constantly recurring physiological laceration of the Graafian follicles is attended with the rupture of blood-vessels and hemorrhage. Surely this is not a pathological process.

Argument 5. The universality, periodicity and continuity of habit of menstruation make it a law of the animal economy; epistaxis is neither universal, periodical, nor continual in that degree, and can in no wise be compared with menstruation.

Argument 6. The investigations of Oldham, Tilt, Ashwell, Ritchie, Hirsch and Slavjanski prove conclusively that ovulation is by no means coincident with menstruation, and that ova may be discharged at any time during the intra-menstrual period. One must think, from a *mechanical* view of the subject, that conception is least likely to take place during the menstrual flow. It certainly is acknowledged to occur most readily immediately after the catamenia; but this (Dr. Studley thinks) is due to the washing away of the cervical secretions or the tenacious mucous plug obstructing the cervical canal by the menstrual blood, thus rendering the canal patent. (Dr. Studley does not remember that Kristeller of Berlin looks upon this mucous plug hanging from the cervix

as the ladder by which the spermatozoa climb into the cervical canal, and therefore next to indispensable to impregnation. P. F. M.) For the study of the menstrual phenomena in animals, Dr. King is referred to sluts, monkeys, man-like apes, &c.

Argument 7. It is asserted by numerous travellers among savage races, that the Mongolian races, the Esquimaux, Samoiedes, Hindoos, &c., menstruate, and at an early age; and Dr. Studley certifies to the harmony of the Indian women of this country with their Caucasian sisters as regards menstruation.

Argument 8. Dr. Studley proves, by citing Bishop Patrick's commentary of the passage quoted from the Bible by Dr. King in support of his view, that the latter's interpretation is wrong; and shows also by mentioning various other verses in Scripture, that menstruation was common in ancient times.

Argument 9. In face of the overwhelming majority of *menstruating* women who have borne large families and enjoy good health, what do Dr. King's few rare exceptions of non-menstruating women, who have done the same, prove? Nothing whatever, except the heat of an enthusiast to make his point.

Argument 10. Dr. King confounds the physiological *capacity* of an organ with the necessity of its physiological *action*. Because the uterus is capable of reproduction, it does not follow that it must at once be set to work, any more than the brain of a child should be taxed to its utmost, or a boy at puberty should be immediately mated, in order that his generative organs may not deteriorate by inactivity. We all know how injurious such a course would be.

Argument 11. Menstruation must be compared to, and is singularly parallel with the closing period of the process of reproduction, the puerperal state; it is scarcely necessary to ask which of these two, the menstruating or the puerperal woman can bear more exposure.

Argument 12. The personal beauty of the female is not the pre-eminent cause of the sexual passions and the sexual act; but the latter are aroused by a physiological process in the organs of generation—in the female, ovulation, ante- and post-menstruation; in the male, the repletion of the vesiculæ seminales. Whether the height of beauty in the female is coincident with the puberile, or rather pre-puberile, age (as Dr. King will have it) is, to say the least, exceedingly doubtful. Where shall these ovulatory, ante-menstrual, marriageable females be found, whom Dr. King pictures as the ideal mothers of the human race? Not among the savages, and, certainly, least of all, among our civilized races.

Dr. Parvin, whose critique preceded that of Dr. Studley, meets Dr. King's assertions with almost identically the same objections as those briefly enumerated above; and the two together, I am afraid, leave no alternative for Dr. King but to withdraw his "New Basis" from the scientific stage. A sequel to this "New Basis" appeared in the *American Journal of Obstetrics* for November, 1875, under the title "ON THE ETIOLOGY AND PATHOLOGY OF AREOLAR HYPERPLASIA OF THE UTERUS," in which Dr. King maintains that the "areolar hyperplasia" of Dr. Thomas arises as follows: During prolonged celibacy, and a consequent prolonged menstrual congestion, a moderate amount of connective tissue begins in the uterus as a formative tissue, a new growth; during a succeeding pregnancy this new growth proceeds rapidly *pari passu* with uterine development; after delivery (owing to imperfect uterine contraction, subinvolution, and increased congestion) the new growth of connective tissue proceeds more rapidly than ever, and the womb is now recognized as an enlarged, indurated organ, while the usual symptoms of uterine disease become well marked. In the case of a virgin uterus, the environment of celibacy remains constant, the organ is slowly, and without much inconvenience to the female, wending the even tenor of its way towards a structural atrophy that is naturally designed, in the end, to adjust the organ to its total want of function. Nature will not support the nutrition of a useless organ, she will allow no sinecures; therefore organs performing no useful purpose become prone to disease, degeneration and decay. Hence, paraphrasing Sir William Gull, who said: "Were I to make a man, I do not think I would put

tonsils in him;" Dr. King ventures the statement: "Were I to make an 'old maid,' I do not think I would put a uterus and ovaries in her."

In the Inaugural Address before the Dublin Obstetrical Society already referred to, Dr. Lombe Atthill speaks of the light thrown on the treatment of uterine diseases by the recent investigations of Williams. Atthill points out the importance of properly discriminating as to the time when an intra-uterine application should be made, in accordance with the intended effect. If the case be one of menorrhagia, care should be taken to make the application only immediately after or a few days subsequent to the termination of a menstrual period; because the application even of a styptic to the uterine cavity, immediately prior to the menstrual flow, will prematurely destroy the vitality of the already desintegrated lining membrane; and if the patient be suffering from amenorrhœa, the application should be made shortly before the expected return of the catamenia, which will thereupon often be found to come on rapidly and painlessly. Carbolic acid is the agent used by Atthill. Some time ago, I accidentally made the same observation, and have since found that care in choosing the proper time for the application proved of material benefit, especially in menorrhagia, where the post-menstrual application of strong tincture of iodine showed itself very efficient in arresting and decreasing the next menstrual flow, although I am not yet convinced that the benefit was owing mainly to the bare condition of the uterine wall, claimed by Williams to exist at that time.

DR. FINKLER, of Kiew, after examining seven specimens of DYSMENORRHEAL MEMBRANE, obtained from four patients, concludes that the membrane is the mucous membrane of the uterus, inclusive of the blind extremities of the glands; but that in some cases the deeper layer of the membrane and the terminations of the glands remain in the uterus. The membrane is the product of disease, and not of impregnation or abortion, nor is it the product of the increased physiological action which takes place in the generative organs periodically; but it is the result of a pathological condition shown by the character of its elements, by the great accumulation of granulation cells; by the blood and fibrine found in the tissue, by the swollen, glistening or indistinct appearance of the epithelium, by the persistence or recurrence of the disease. All the membranes examined gave the amyloid reaction with iodine and sulphuric acid, and Dr. F. thinks this to be the real pathological condition. The cause of the detachment of the membrane lies in the accumulation of granulation cells and the flow of blood into the mucous and submucous tissue.

DR. R. SCHRETER, second physician to the insane asylum, "Schweizerhof," discusses the RELATION OF THE MENSTRUAL FUNCTION TO MENTAL DISEASE (*Beiträge zur Geburtshülfe und Gynäkologie*, Berlin, III., 3, 1875), taking, as a basis for his observations, 212 female patients of the asylum. Of these, 110 were married, 102 unmarried; 93 were afflicted with mere *melancholy*, 87 with *mania*, and 42 with various conditions of *mental debility*; their ages varied from eleven to fifty years. The results of his careful observations are the following: 1. In the great majority of cases with mental disease, a menstrual irregularity of some kind existed. 2. Those patients whose menstrual function was perfectly normal, were either cases of slight mental disorder; or, if the disease was well marked, as was usually the case, were instances with doubtful prognosis. 3. These menstrual anomalies usually commenced at an early date, and were influenced by the slightest symptoms of the mental disease, but did not ordinarily become prominent until the psychosis itself had developed. 4. Amenorrhœa was the predominating form, especially alternating with the mental affection, and continuing a greater or lesser time. 5. Irregularities as to time and duration, and qualitative, and particularly quantitative, deviations (profuse in 39 cases, in only two of which, however, the menorrhagia appeared to exert a favorable influence on the mental condition), were very frequent. 6. If the menstrual disturbance was less prominent at the beginning of the psychosis, it nevertheless became very marked at the height of the disease, independently of the age and standing of the patient, or exacerbations of the psychosis accompanied the

menstrual molimina, or occurred simultaneously with, or immediately after, the flow. 7. Finally, at the time of recovery, all menstrual irregularities generally disappeared, and the perioical influence of the catamenia on the mental condition became lost sooner or later, and no direct or alternating relation could be detected between them.

DR. W. D. HAGGARD, of Nashville, Tenn., in an excellent paper on "THE ANALOGY BETWEEN EARLY ABORTION AND DYSMENORRHOEA" (*Virginia Medical Monthly*), points out how little is generally known on the subject of early abortions, and how liable we are to confound those occurring as early as the fourth or sixth week, with menorrhagia or dysmenorrhœa. He offers the in my opinion, well-founded supposition that many of the cases of dysmenorrhœa or moderate menorrhagia occurring in apparently perfectly healthy, but unaccountably sterile, married women, whose social and marital relations are all that could be desired, are in reality nothing but cases of repeated conceptions and abortions, entirely unsuspected either by the physician or the patient. He thinks that organic and nervous irritability are the most important factors in the production of these, and of all, abortions; and claims that abortion occurs most frequently at the end of the first four weeks; not, as is generally taught, at the third month. He relates a case in point, in which he cured a sterility of seven years' duration in an otherwise perfectly healthy woman (the slight gaping and congestion of the external os of whose uterus first led him to surmise the true facts), by a cold shower bath every morning, followed by active friction of the skin, gentle laxatives, tonics, and nervous sedatives; and, chiefly, perfect rest, and opium enough to keep her slightly under its influence. At the approach of the next menstruation, sixty drops of laudanum were given by enema night and morning, a blister was applied to the inside of each thigh, and the period passed without any return of the abortive symptoms. This treatment was continued, mainly at the time of the expected, but not occurring, menstrual flow, for five months, and the lady went to term and was safely delivered. The practical significance of this case is obvious; viz., never to omit examining macro- and microscopically the menstrual discharge in a case of chronic dysmenorrhœa or slight menorrhagia, in an otherwise healthy sterile married woman, in whom no special cause for the dysmenorrhœa or sterility can be discovered.

BATTEY'S OPERATION OF NORMAL OVARIOTOMY forms the subject of a paper by Drs. YANDELL and McCLELLAN, of Louisville, who interviewed Dr. Battey, and publish a verbatim report of their conference in the *American Practitioner* for September, 1875.

Battey gives the following indication for the operation: "Any grave disease which is either dangerous to life or destructive to health and happiness, which is incurable by the recognized resources of our art, and which we may reasonably expect to remove by effecting the change of life." It is not proposed for any case curable by any other method or means; neither amenorrhœa, dysmenorrhœa, nymphomania, or any particular disease, is an indication for its employment. The operation is done simply to stop diseased or pernicious ovulation, and to effect the change of life. Dr. Battey has changed his original plan of operation from abdominal to vaginal, neither does he now use ligatures and sutures. The patient is placed in Sims' position, the uterus drawn down, the posterior vaginal cul-de-sac and peritoneum opened with scissors to the extent of 1½ inches, the forefinger introduced, and the uterus, ovaries, broad ligaments, and Fallopian tubes examined; an ovary is then drawn down to the opening, seized by the forceps, a stout ligature is passed around the gland as a guide to the écraseur, the chain of which is slowly tightened so as to sever the attachments in ten or twelve minutes; the same is done with the other ovary, the wound is sponged out, the vagina cleaned, and the patient put to bed after an almost bloodless operation. Dr. Battey, up to Sept., 1875, had operated ten times, with eight recoveries and two deaths. In all of these cases except the first, in which, through accident, they were not examined, the ovaries were cystically degenerated. In three cases only one ovary was removed; in two of these removal of the

other ovary subsequently became necessary; in the third, a second operation appeared imminent. Thus it would certainly seem hazardous to remove only one ovary. Nothing like proper menstruation had been observed by Dr. B. after the operation. The sexual desire, however, had remained wholly unimpaired in the married cases; one unmarried lady felt "conscious of no change of her feelings in any respect." In no case did the subjects of this operation appear unsexed; therefore, the term "spaying" applied to it by Dr. Matthews Duncan (*Address British Medical Association*, 1875), is entirely unjustified. Dr. B. is satisfied that the women upon whom he operated "*lost nothing whatever by the operation*." As regards the curative influence of the operation on the symptoms for which it was performed, in the cases operated upon during 1875, scarcely sufficient time has elapsed to permit a definite opinion: unfortunately, the results obtained after the first four operations, as well as after the only two other procedures of the kind by Thomas and Sabine, have by no means assured the profession that the benefit intended by, and claimed for, the operation by Dr. Battey is really achieved. If it does not unsex a woman, and if a certain degree of that periodical nervous process known as menstruation is still maintained, after the removal of both ovaries, by *normal* ovariectomy (a misnomer, as Battey himself says; Sims suggests *Batteyising* as a name for the operation), how can we be certain that the nervous disorders for which it was performed will really disappear after the operation? As Thomas says: "It is too young as yet to be decided upon; and is unquestionably a procedure which may be greatly abused." Hitherto there has been no sign of such abuse; nor do I think it likely, unless the opinion of the profession about it undergoes a decided change.

THE practice of DRAINAGE OF DOUGLAS'S CUL-DE-SAC IN OVARIOTOMY has not gained in supporters during the past year. In this country, I believe I may safely say, that it is scarcely performed at present, abdominal drainage after Peaslee and Thomas being preferred. In Germany it has found several supporters. The best, indeed almost the only, reference to the subject, of late, was made by Prof. Carl Schröder at the session of the Erlangen Physico-Medical Society, May 10, '75. His remarks were originally intended for insertion in the article "Ovariectomy," in Vol. X. of *Ziemssen's Encyclopædia*; but, arriving too late for publication there, appeared first in the *N. Y. Med. Record*, Sept. 25, '75. They are so interesting and to the point, that we cannot refrain from reproducing them entire, finding that an abstract would detract too much from their value. Prof. Schröder said that after Peaslee had first recommended draining off the exudation from the abdominal cavity as the most efficient mode of treatment in cases of peritonitis, Sims seized this idea, and came to the conclusion that Douglas's cul-de-sac had best be punctured and kept open in every ovariectomy, certainly in all in which a peritoneal exudation was to be expected, on account of already existing peritoneal irritation or of extensive adhesions. The train of thought which Sims followed was, he said, essentially as follows: He assumes, and bases his assumption upon a series of fatal cases taken from Spencer Wells's operations, that the great majority of those dying after ovariectomy succumb to septicæmia, and that this is caused by an exudation which accumulates in the abdominal cavity, and undergoes decomposition into an ichorous fluid. Thus he arrives at the conclusion that the chief aim of a rational system of treatment must be to remove this exudation from the abdominal cavity, but that this can only be done easily and simply when the necessary precautions are taken during the operation. Very many in Germany have assented to these deductions, which Sims has advanced with the persuasive mastery of statement which is peculiar to him. Nussbaum especially adopts this idea with enthusiasm, and anticipates from it a new curative measure in ovariectomy; and Spiegelberg is also warmly in favor of it. I see, he says, in these views an essential danger for ovariectomy, and cannot recognize the correctness of Sims's deductions. For, agreeing as I do with Sims, that most of those operated on succumb to septicæmia, I still cannot concede that the reddish serous exudation plays the dreadful part that Sims ascribes to it; for daily experience teaches us that

transudations and exudations in the abdominal cavity have in themselves no tendency to decomposition and the production of septic states. In fact, I have frequently satisfied myself that, even after ovariectomies, the exudation is not the bugbear that Sims considers it. I have three times operated under circumstances where an exudation into the abdominal cavity might certainly be expected, without the occurrence of the least trace of a septic process. In the first case I operated where an exudative peritonitis already existed. After the operation, the meteorism and vomiting persisted for a few days, but soon improved, and recovery ensued without any disturbance. In the other two cases, to be sure, death occurred, but only after twelve and fifteen days respectively; and from a complication, viz., tetanus, which was not in any degree dependent upon septic processes in the abdominal cavity. In the first of these cases, the whole anterior surface of the tumor had contracted adhesions to the abdominal wall, so that the recognition of the peritoneum and the separation of the tumor from it presented great difficulties. From the extensive surfaces of cicatricial adhesion, an exudation would necessarily take place. In spite of this, there was perfect healing of the abdominal wound; there was no trace of peritoneal irritation; and when death, occurring on the ninth day, furnished the opportunity for an autopsy, the abdominal cavity was found perfectly healthy, and there was no suspicion of a decomposing exudation. In the last case, the adhesions were not so extensive as they were firm, and could only be separated with great difficulty. Here there existed a reddish, serous transudation in the abdominal cavity, which constantly reaccumulated, even during the operation, and a part of which had to be left in the abdominal cavity. The patient had hardly the least sensitiveness, very little fever, and was perfectly well after eight days. She had a normal temperature and a strong appetite; in fact, showed no septic phenomena at all, death also ensuing from tetanus as before, but on the fifteenth day. Small fibrinous flocculi were, it is true, found in the abdominal cavity upon the intestines, also in a few places small deposits of pus, which could easily be scraped off with the blade of the knife; and in the true pelvis there was a reddish, serous fluid. Notwithstanding there were no septic phenomena, and she would undoubtedly have recovered had the calamitous tetanus not appeared as an intercurrent disease. Now, how does it happen that in one case an exudation undoubtedly present occasions no disturbances whatever, does not even perhaps furnish slight symptoms of peritonitis; while, at another time, with a very slight operation, involving scarcely any injury of the peritoneum, there are associated the most violent symptoms of septic peritonitis? According to my conviction, this difference depends wholly upon whether infection has or has not taken place. In its absence the exudation is perfectly harmless, and is easily absorbed by the peritoneum without irritation; should it occur, however, the exudation becomes decomposed — i.e., where there was no exudation, a violent peritonitis sets in, which furnishes a rapidly decomposing exudation. If this view be correct, it is evident that our treatment must be directed, not to the accumulating secretion but to the prevention of the infection from which the whole trouble arises. Now, although, in spite of all our experiments on vibriones, we still do not know precisely in what the infective substance consists, I am, nevertheless, certain that it is conveyed from without; and that its conveyance is, as a rule, by the hands, the instruments, or other appliances of the operator and his assistants. If, therefore, we wish to ward off the infective substances, we must operate in healthy places, and must attend most carefully to the absolute cleanliness of our hands, linen, clothing, instruments, sponges, etc.; also, in order to disinfect as far as possible the air which forces its way into the abdomen, it is desirable to operate under the spray of carbolic acid, as is my custom in every case. If with such painstaking exactitude, we guard against infection, the exudations do not decompose, and consequently give rise to no septic phenomena. Under these circumstances, then, drainage is unnecessary, and if unnecessary, ought to be omitted; as, in any event, it makes the operation more complicated, difficult, longer, and more dangerous. I should therefore decide upon drainage

during the operation, only in case I believed—a state of things which of course should not happen—that the patient had become infected, or in case decomposing masses, from some suppurating cyst, *c. g.*, had found their way into the abdominal cavity. Drainage of the abdominal cavity assumes a very different position as a therapeutic measure against a septic peritonitis which already exists. For, although the exudation be neither the original cause nor the only symptom of the septic condition, it must still be conceded that its removal is highly desirable. It is true this is then difficult to accomplish. Douglas's cul-de-sac is easy to puncture only when an exudation is inclosed in it. Then only does a tumor form behind the uterus, which is easy to get at. It is precisely in these cases, however, that its evacuation is not absolutely demanded; for the exudation, being encapsulated, is rendered harmless, and does not lead to absorption or perforation. If, however, there is a free exudation in the abdominal cavity, it does not bulge forward to Douglas's cul-de-sac. It is then difficult and dangerous to puncture, for fear of injuring the neighboring parts. The need of it is felt with especial frequency in puerperal peritonitis. But here the neck of the anteverted uterus lies so close to the rectum that we do not know where to make the puncture. In such cases we might think of removing the exudation by incision of the abdominal walls. The abdominal cavity, however, cannot be satisfactorily washed out through a simple incision in the abdominal walls. So the only efficient procedure, it seems to me, in such cases, is to perform laparotomy, and then to perforate and establish drainage through Douglas's cul-de-sac from within. Perhaps this will yet become the treatment for septic peritonitis, although it requires great confidence to undertake this operation in a woman suffering from general peritonitis. In conclusion, let me once more state my views precisely, that the exudation after ovariectomy is not in itself the cause of the septicæmia, but is, on the contrary, perfectly harmless, unless it decomposes; but that decomposition only occurs after infection, and that consequently the important point is not the removal of the exudation, but the avoidance of infection.

DR. KEITH, of Edinburgh, publishes his *FOURTH SERIES OF FIFTY CASES OF OVARIOTOMY* in the *British Medical Journal*, June, '75. In this number there were but six deaths. Of these, one was a case of a woman aged fifty-three, who had an acute suppurating cyst weighing twenty-nine pounds, and having extensive parietal, intestinal, and hepatic adhesions. Another was a solid, malignant tumor, weighing fourteen pounds. Another had universal adhesions, especially firm in the pelvis; the tumor weighed forty-three pounds. Another was a case of solid, malignant tumor, with cancer of the peritoneum; the age was forty-four. The fifth and sixth were both cases occurring in young women with extensive adhesions. In all the cases of death the cause was septicæmia, and he thus summarizes them: "In the six fatal cases the cause of death was septicæmia. The first was a case of acute suppurating cyst with pyæmic fever. It seemed a hopeless undertaking, but equally hopeless-like cases of suppurating cysts had recovered. In the second and fourth the tumors were malignant. In both there was red serum in the abdomen, and both had fluid in the pleura at the time of the operation. Of the other three cases, in two the operations were very severe, lasting nearly three hours in the one, and upwards of two hours in the other. The latter was a badly constituted woman, and had suffered from acute kidney affection, with convulsions. One was drained, the other was not. Earlier operation might have saved both. In the last fatal case, the patient was perfectly well till the sixth day, and recovery seemed sure. In a few hours the temperature rose to 106°, the pulse to 170, and she died four days after. Thus it will be seen that the fatal results are, in most cases, accompanied with very good reasons." Dr. Keith says he has had fewer operations than formerly, but this he attributes to the increasing frequency of ovariectomy as an operation, and believes that only the more severe cases come to him. Since the last series was published in November, 1872, he has been able to diagnose and cure by a single tapping four cases of serous cyst of the broad ligament. Dr.

Keith concludes by saying, that after trying all methods, he has come back to the actual cautery in treating the pedicle, and believes it to be the best of all intra-peritoneal methods for securing it. When his numbers are larger, he proposes to publish all the cases in detail in which it has been used. It was introduced by Mr. Baker Brown. He has used sulphuric ether in all his cases.

In an exceedingly interesting article, Dr. GEORGE H. BIXBY, of Boston, describes A NEW INSTRUMENT FOR THE READY AND EFFECTIVE USE OF THE DOUBLE CURRENT IN THE TREATMENT OF SUPPURATING CAVITIES AND PELVIC DRAINAGE, which he calls the *double trocar*. It consists of a straight or curved canula 16" long, by $\frac{1}{4}$ " in diameter, divided by a horizontal septum into two equal chambers, the inferior extremity of each being fenestrated to the extent of an inch. Superiorly, each chamber is connected with a branch, the afferent and efferent, with rings and guard for retention. Into this canula fits a flexible double trocar with common handle, the two points of which come together and form a single point when the trocar is forced home. By means of the fountain syringe, a copious and uninterrupted double current is secured, the entrance of air is absolutely avoided, and the apparatus becomes one which, the trocar having once been introduced and secured by tapes, any attendant can use properly whenever desirable. Dr. Bixby recommends the instrument in the following conditions—with straight trocar and canula: 1. *Pelvic abscess*. 2. *Retro-uterine hamatocoele*, if the amount and nature of the discharge, after spontaneous or operative evacuation of the cyst, called for injections and drainage. 3. In *pelvic abscess and hamatocoele*. 4. *Pelvic drainage after ovariectomy*. 5. *Treatment of the uterine cavity after the removal of intra-uterine and interstitial fibrroids*. 6. *The treatment of the uterine cavity in acute puerperal endometritis and in offensive post-partum vaginal discharges*. (Dr. Bixby identifies himself with the septicæmic origin of puerperal fever.) 7. *For cold and astringent injections in post-partum hemorrhage*. 8. *In the treatment of chronic cystitis in the female*. Dr. B. says that, after a fair trial of other disinfectants, he has returned to the use of a five per cent. solution of carbolic acid (4 drachms to 16 ounces of glycerine, soap and water) as by far the most efficacious.

Skipping the last four conditions, I will return to the fourth, the EMPLOYMENT OF PELVIC DRAINAGE AFTER OVARIOTOMY, as the one to which Dr. Bixby himself most particularly refers; and which to me, and I doubt not the profession generally, is the most interesting.

Dr. Peaslee reported the first case of primary drainage through Douglas's cul-de-sac; the patient owed her life to the measure. He has used drainage in every case of ovariectomy since 1855, in which there was anything to drain, but has always preferred to drain through the abdominal wound. According to Dr. Bixby, to Dr. Gilman Kimball, of Lowell belongs the credit of having developed and perfected this method. Dr. Kimball has practiced vaginal drainage in eleven cases, from 1867 to 1875, in the three first of which the ligatures only were passed through the cul-de-sac; in five others the ligatures through the canula; and in three the canula only; two of these eleven cases died, one of tetanus on the 20th day, the other of septicæmia; in all the nine recoveries, threatening symptoms showed themselves, only to be speedily relieved on the discharge of more or less fetid fluid through the drainage-opening or canula. Spencer Wells has only three cases, all of secondary pelvic drainage, with one recovery and two deaths, both from septicæmia; the case of recovery was a desperate one, and the woman doubtless owed her life to the employment of vaginal drainage. Dr. Bixby appears to recommend pelvic drainage for all cases of ovariectomy; at least he suggests passing a current of tepid slightly carbolized water through the abdominal cavity as soon as six hours after the operation, and to continue this daily, until the return of a perfectly clear fluid from the canula shows that the latter tube and drainage are no longer necessary. If there be local or general signs of putrid accumulation, however, the injection should be copious, frequent, and highly disinfectant; and, by shutting off the efferent current, the whole abdominal cavity may be thoroughly cleansed.—Of the utility and even vital importance

of intra-peritoneal injections, abdominal and vaginal, in all cases where extensive adhesions or peritonitis entail oozing of sero-sanguinolent fluid or intra-peritoneal empyema, and subsequent putrescence, there can be no doubt whatever. An unfortunate case of mine during the past year, where ovariectomy was performed during inflammation of the cyst and sub-acute peritonitis after tapping, as the only chance of saving the life of the patient, and the latter, after doing well for four days, suddenly developed septicæmia and died on the sixth day, notwithstanding copious intra-peritoneal injections of a dilute solution of salicylic acid, has led me to think whether *continuous* gentle irrigation of the whole peritoneal cavity by means of a perforated drainage tube passing from the abdominal wound through Douglas's cul-de-sac into and out of the vagina, the afferent tube being connected with a large elevated vessel, would not be beneficial from the very first in such desperate cases of intra-abdominal putrescence. The experience of Peaslee, Thomas, Spencer Wells and others would show that only in such extreme cases is the perforation of Douglas's cul-de-sac necessary; that in ordinary cases it is scarcely justifiable, because more likely to do harm than good (witness Sims' retraction of his former unqualified support of it for all cases); and that simple abdominal drainage will usually answer all purposes.

DR. JAMES R. CHADWICK, of Boston, while reporting a case of ovariectomy, the patient dying from exhaustion dependent on constitutional derangement from chronic alcoholism, (unknown to Dr. C. when he operated), mentions a, in his opinion, NEW AID TO THE DIFFERENTIAL DIAGNOSIS BETWEEN OVARIAN TUMORS AND LARGE UTERINE FIBROIDS. The tumor could not be reached per vaginam, and the large amount of ascitic fluid prevented a wave of fluctuation from extending from the epigastrium to the uterus. The patient was placed in the knee-elbow position, with the idea that a heavy uterine fibroid would of itself sink in the ascitic fluid until it rested on the abdominal walls, and thus became subject to palpation, the vagina being drawn out like a funnel and the cervix more or less retracted by the traction of the tumor; whereas, if it were an ovarian cyst, its position would be but little changed, the vagina would be less distorted, and the impulse from above would not be readily transmitted to the uterus, as in a fibroid. Two contingencies might nullify the inferences to be drawn from these indications, viz., adhesions, and a partly solid ovarian tumor. However, adhesions are rare in uterine fibroids, and solid multilocular ovarian tumors are generally situated in the pelvis and easily accessible. The diagnosis of an ovarian cyst, without adhesions, proved to be correct.

After being obliged to open the abdominal wound four hours after the operation, to permit the evacuation of a large quantity of serum, the presence of which had thrown the patient into a state of extreme collapse, removing the clamp, and dropping the pedicle, Dr. C. found himself compelled on the fifth day to puncture the enormously distended intestines with a fine trocar to evacuate the gas, which was impeding respiration. In this connection it occurred to him that another purpose might be fulfilled through the aspirator-cannula, namely, the injection into the intestine of brandy, beef-tea or other nutrient fluids, and of solutions to excite peristaltic action or soften and break down hard scybalous masses, when enemata and purges have failed. In a subsequent case of puerperal peritonitis, Dr. C. made a trial of this new method, the aspirator needle having been introduced into the transverse colon for the evacuation of gas, and injected a dilute solution of whisky; this was soon followed by lancinating abdominal pains, clearly due to peristaltic action, which resulted in the speedy expulsion of two large scybala from the rectum, which had previously been empty. The woman died two days later from septicæmia. Dr. C. (*Amer. Journal Obstetric*, Nov., 1875.) thinks that this method, besides affording in some cases of obstinate vomiting and rectal obstruction perhaps the only means of conveying a sufficient amount of nutriment to the patient, is capable of being utilized in the treatment of very obstinate cases of intestinal torpor, fecal impaction, and the like.

THE DANGER OF THE EXPLORATIVE PUNCTURE OF AN OVARIAN CYST IS

emphasized by DR. A. M. FAUNTLEROY, of Staunton, Va. (*Trans. Med. Soc. Virginia, Va. Med. Monthly*, Nov., 1875, in the report of a case of ovariectomy, a month previous to which the cyst was punctured with the hypodermic syringe needle and a syringe of fluid withdrawn for the purpose of diagnosis. Three or four days later, the patient complained of increased abdominal uneasiness, and suffered from frequent paroxysmal attacks of pain during the following week, which were thought to be due to local or partial peritonitis. At the operation, evidently fresh adhesions were found between the cyst and abdominal wall and colon. The patient died from exhaustion through emesis and diarrhœa. Dr. F. says that, although the unfortunate issue can in no wise be attributed to the puncture, still he inclines to the opinion that the explorative puncture, even with the finest needle, should be resorted to, only when its diagnostic aid is indispensably necessary for the differentiation of ovarian from other tumors.—A much more striking case (already briefly referred to in this Report) of the danger of tapping on ovarian cyst, even with a fine aspirator needle, occurred in my practice during the past fall. The patient had never been subjected to any surgical interference, and objected very strongly to the radical operation for the ovarian tumor, but wished relief from the abdominal distress and the dyspnœa caused her by the cyst. In order to temporize and prepare her for the ovariectomy, I tapped her with an aspirator needle of medium size and withdrew about four quarts of fluid, (the patient was of small stature, and the abdominal walls very tense and unyielding, hence the disproportionate amount of distress,) using extreme care to exclude air from the cyst-cavity and not to manipulate the abdomen too much. Although much relieved for several days, on the fifth day paroxysms of severe abdominal pain commenced, accompanied by slight increase of temperature and great frequency of the pulse, and a marked degree of exhaustion, all doubtless due to subacute peritonitis. Within twelve days from the date of the tapping, the exhaustion had increased to such a point as to make an immediate removal of the cyst the only "foriorn hope" of possibly saving the life of the patient—an opinion in which Dr. T. G. Thomas, who kindly saw the case with me, entirely concurred. On opening the immensely tympanic abdomen, the whole surface of the cyst was found united to the abdominal, pelvic and visceral peritoneum by broad fibrinous adhesions of recent origin, which were easily separated by the hand without bleeding. The peritoneum was strongly injected throughout, and the cyst contained a large quantity of purulent, flocculent fluid. The patient rallied immediately after the operation, and did well for four days, when the effects of the subacute peritonitis showed themselves in the development of an exceedingly offensive peritoneal effusion, which resulted in the death of the patient from septicæmia two days later, notwithstanding frequent intra-peritoneal injections with a solution of salicylic acid. I confess only extreme necessity would induce me to venture on tapping an ovarian tumor in future, although I have formerly done so repeatedly without the least evil consequences.

At a meeting of the New York Obstetrical Society, held May 18th, 1875, (*Amer. Journal Obstetrics*, Feb., 1876,) DR. PEASLEE said that a polycyst of the ovary should never be tapped unless the operator is ready to perform ovariectomy within twenty-four hours, which will generally be found necessary; that he considered the aspirator in these cases no safer than the ordinary trocar and canula; and that it makes but little difference whether the opening is slightly larger or smaller. In a case on which he was going to operate on the following day, inflammation of the cyst followed tapping with the aspirator within forty-eight hours.

At that time I gave little heed to this warning, but sad experience has since led me to dread what I formerly considered a very innocuous operation.

THE DIAGNOSIS OF OVARIAN CYSTS AND THE INDICATIONS FOR THEIR TREATMENT.—DR. RHEINSTÄDTER, of Cologne (*Berl. Klin. Woch.*, May 31, 1875), thus reduces to formula his views of the indications that can be drawn from the fluid obtained by puncture from suspected tumors. 1. The presence of par-albumen does not at all prove the existence of a hydrovarium. 2. From its absence we cannot argue with certainty the non-existence of cystic disease of

the ovary. 3. There is great probability of a hydrovarium, if we find par albumen abundant, with a viscid condition of the fluid, like barley-water, and with an abundant deposit of cellular detritus, and large round cells that are swollen or undergoing fatty degeneration. 4. The presence of well-formed, nucleated cells of cylindrical epithelium collected in groups or rows speaks positively for hydrovarium, especially when this microscopic investigation agrees with the gross appearances and the chemical constitution of the fluid. Dr. Rheinstaedter lays down the following as the *indications* for treatment. The danger of the operation always stands in direct relation to the danger of the disease itself, and this sometimes gives little trouble for many years, and is even sometimes spontaneously cured. A woman, then, with a moderately large ovarian cyst, which is stationary, and occasions no special difficulty, should not be operated on unless she urgently desires it. An expectant treatment should be adopted, including general hygienic measures, the avoidance of all causes of sexual irritation or excitement, the wearing of a body bandage, and careful attention to any complications. The patient should not marry, because of the danger of pregnancy as a complication. If, however, the cysts grow rapidly, so as to interfere with the functions of the body, and occasion loss of strength and severe neuralgic pain, extirpation is indicated. There are also certain circumstances, such as hemorrhages into the cyst threatening life, that call for immediate extirpation, as do also suppuration or ulceration of the cyst, perforation followed by peritonitis, or symptoms of incarceration of the bowel in pseudo-membranous bands. Puncture, either with or without the subsequent injection of iodine, is now generally rejected as a radical mode of treatment. It is uncertain, because there may be more than one cyst; it may cause circumscribed peritonitis, and adhesions which will complicate an ovariectomy when it has to be performed. Puncture with drainage from the abdominal walls is generally rejected, because the sac cannot be completely emptied of its ichorous contents. Puncture through the vagina, on the other hand, is regarded favorably in case of small cysts firmly attached in the pelvis, and it can be combined with drainage and the washing out of the cyst with medicated fluids, a mode of treatment which may be adopted even in the case of large cysts; and they can be reached in this way, if ovariectomy is refused. We should endeavour to persuade the patient to the latter, however. For purposes of diagnosis, however, puncture can and should be employed in almost all cases, either through the vagina or abdomen, according to the accessibility of the tumor. Puncture may also serve as a palliative in cases where the tumor cannot be removed on account of adhesions, or where there is the complication of pregnancy occasioning pressure symptoms, where the internal organs require prompt relief. The contra-indications of ovariectomy are pointed out as above. It may, however, be indicated sometimes in pregnancy, when the symptoms due to pressure are urgent and cannot be relieved by puncture, or where there has occurred rupture of the cysts. In such cases it is preferable to either artificial abortion or artificial premature delivery. Ovariectomy is also contra-indicated in the presence of serious disease of internal organs which threatens to end fatally; also in cystic carcinoma.

DERMOID CYST OF THE OVARY.—M. TERRIER, in *Bulletin Gén. de Thérap.*, March 15, 1875, narrates the case of a patient who came under his care for a large tumor occupying the right iliac fossa, and presenting all the characters of an ovarian cyst.

An exploratory puncture revealed the existence, in this tumor, of epithelial cells, of hair, and of a thin grayish-white liquid. An operation was decided on, and was executed without difficulty, the results being favourable. The temperature never exceeded 100.5 F. Without appreciable cause the urine became retained at the end of five days. Retention of urine is not uncommon after ovarian abdominal section, and it is the best practice to draw off the urine, and spare the patient the discomfort and straining which often accompany voluntary micturition in such cases.

Microscopic examination of the sac showed that it presented, on its internal

aspect, the appearance of skin. In fact, hair, fatty granules, sudoriferous glands, etc., were found.

AN EXTRA OVARY.—In the Société de Biologie. DR. DE SYNETH (*Le Mouvement Médical*, 20th June, 1875), mentions the following interesting case: A newborn child that died a few hours after birth had—the body offering no anomaly—on the border of one of the ovaries six or seven small cysts—pedunculated. One of these was remarkable on account of its form and consistency; and, upon being examined more closely, it proved to be a normal ovary, with its epithelium, its tubes and follicles, having in the centre an ovum, with its macula germinativa.

This case appears to be the only one of its class, and is believed to be especially noteworthy from the probability that, with the existence of this ovary in the living body, we should have had an interesting case of extra-uterine pregnancy. In literature only two cases something similar are reported; these are given by Americans, and will be found in the *Annales de Physiologie*.

SEROUS OVARIAN CYSTS.—DR. PANAS read a paper at a recent meeting of the Académie de Médecine, and drew the following conclusions, which are contained in *Le Mouvement Médical*:

1. That among the cysts called ovarian, there is a class of unilocular cysts containing a special fluid, the treatment of which is as simple as it is certain in its results.

2. The characters of the cystic fluid are, complete absence of viscosity, perfect diaphaneity (with occasional exceptions), poverty in proteinous material (modified albumen), and its relative richness in alkaline salts, principally chloride of sodium. Slightly or not at all precipitated by heat and nitric acid, the liquid in question is precipitated by alcohol. In this respect this fluid bears a certain analogy to that found in the spermatic cysts of men, as we may be convinced by comparison of the two fluids.

3. We are at present ignorant as to whether the point of origin of these cysts is actually in the ovary, or is it not rather in the par-ovarium (body of Rosenmüller).

4. The treatment of these cysts is still more simple than that suggested by Boinet, who proposed puncture by means of a trocar, followed by injection of iodine. A simple puncture by the trocar is sufficient to bring about a cure, by removing the fluid completely or even partially.

5. By this process, not only is all danger avoided, but even the slightest pain to the patient. In a word, the treatment of these cysts is easier than that of simple or spermatic hydrocele in men, which requires, almost always, the subsequent employment of caustic or strongly irritating injections.

This I believe to be the opinion now generally entertained by prominent ovariologists in this country, and, if I mistake not, also in Great Britain. Exceptions to this rule, of course, occasionally occur.

MR. SPENCER WELLS reports an additional case, in which he performed OVARIOTOMY TWICE IN THE SAME PATIENT (*Journal Great Brit.*, July, '75), the first ovary having been removed in May, 1870, the second in June, '75; the patient made a good recovery. At the first operation he merely punctured a small cyst in the remaining ovary, preferring to give the still youthful patient the chance to bear children, and that the disease might not return, as he had seen occur in several other cases, rather than increase the risk of the operation by removing both ovaries at once. Up to the time of publication he had performed 710 ovariectomies, among which were six performed for the second time on the same patient, with a result of four recoveries and two deaths. It thus appears that the puncture of one or more small cysts in the remaining ovary is sufficient, in the great majority of cases, to prevent the development of those cysts, and that only actual degeneration of the greater portion of the second ovary would call for its immediate removal.

PROF. KÖBERLE reports a SUCCESSFUL CASE OF OVARIOTOMY IN A GIRL OF THIRTEEN (*Gaz. Med. de Strasbourg*), which is rendered remarkable by the employment of the stomach pump for gastric disturbance, which occurred on the eighth day, with sour eructations, greenish vomit, and persistent retch-

ing. General peritonitis seemed imminent, when K. finally succeeded in removing the gastric gases with the pump, and injected water. This was repeated several times at intervals of ten to fifteen hours, each time with a more favorable result. She was discharged on the twenty-eighth day. This is the third case in which Köberlé attributes the recovery of the patient to the stomach pump.

DR. G. NEPVEN contributes a most valuable article on the RUPTURE OF OVARIAN CYSTS, referring to no less than 155 cases, 128 of which ruptured into the peritoneal cavity, 11 into the large intestine, the others perforating the abdominal wall, bladder, vagina, and uterus. Of 127 of these cases, 63 proved fatal.

A case of the rare affection, ENCYSTED DROPSY OF THE PERITONEUM, in which SUPPURATION had occurred, and ABDOMINAL SECTION was performed, with RECOVERY, is reported by DR. J. EWING MEARS, of Philadelphia. The patient was forty years of age, mother of six children, from the birth of the last of which, seven years previously, the trouble is dated. During the past year her health began to fail, and the tumor grew in dimensions. The abdomen was enlarged to the size of pregnancy at term, and fluctuation was tolerably distinct. A positive diagnosis not being possible, Dr. M. advised an exploratory incision, as the extreme and increasing debility of the patient called for active interference. On attempting to separate the numerous adhesions between the anterior wall of the tumor and the peritoneum, the cyst was opened and two gallons of pus evacuated. The cyst walls were found to be formed above by the large omentum, and below and on both sides by the closely adherent intestines. Under careful drainage from the lower corner of the wound and intra-peritoneal injections, the wound closed in the sixth week and the patient recovered. Abdominal section and free drainage undoubtedly seems the proper plan of treatment in these cases, in preference to mere tapping.

A very rare CASE OF REMOVAL OF A RETRO-PERITONEAL CYSTIC TUMOR, "FIBROMA MOLLUSCUM CYSTICUM ABDOMINALE" (Virchow), is reported by VIRCHOW (*Arch. für. Path. Anat.*, Vol. 63, No. 4). The tumor was removed by SPENCER WELLS in Pomerania, and contained $12\frac{1}{2}$ pints of pus. The right ovary, which, with a portion of the tube, was in contact with the tumor, and perfectly healthy, was removed with it, in order to permit more effectual control of the hemorrhage. The left ovary was normal, and was not touched. The patient recovered. Virchow says that the tumor is essentially a cystoid; that it closely resembles a soft uterine myoma or myo-sarcoma, especially as regards softening, inflammation, and ulceration; that this growth was originally solid, and that the successive cystic formation and inflammatory action resemble the formation and subsequent inflammation of the subcutaneous bursæ. Analogous forms occur, not merely in the ovary itself, but also in the ligaments and the vicinity of the ovary.

In ovarian therapeutics we stand on the eve of a great revolution! DR. SEMELEDER, late of the city of Mexico, at present in this city, under the title "NO MORE OVIARTOMY," reports five cases of ovarian cysts, which he radically cured by electrolysis. The tumors were of various sizes, the largest reaching three fingers above the umbilicus. The needles were inserted directly into the tumor; the number of elements and length of application varying from two to ten minutes, according to the susceptibility of the patient. In one case a cure was effected in six weeks, thirty-eight sittings being held; in another nearly six months were required; in the remaining cases an intermediate time was necessary for a cure. The operation was performed without anæsthesia, and the patients were not confined to their beds. The incalculable value of a remedy which would do away with perhaps the greatest triumph of modern surgery, the operation of ovariectomy, need not be pointed out. Whether the new method will stand the test of time and a larger array of cases, remains to be seen. For my part, however much I may hope that Dr. Semeleder's as yet small experience will prove fruitful, I do not fear but that

some ovariectomies will still remain for us younger gynecologists, and perhaps for our successors.

In a thoroughly logical and scientific article on THE INFLUENCE OF POSTURE ON THE HEALTH OF WOMEN (*Obst. Jour. Gr. Br. and Ire.*, Nov., 1874, Jan. and June, 1875), the eminently practical bearing of which is obvious, DR. J. H. AVELING very justly says, that the great frequency of female disorders at the present day cannot possibly be attributable to natural causes. Leaving out the numerous sexual derangements which can be accounted for by the accidents occurring at critical periods both in the married and single states, by constitutional or specific causes, want of fresh air and exercise, errors in dress, late hours, excessive dancing or riding, and other imprudences, there still remains a large class which must be referred solely to habitually defective posture. This most important factor in the production of uterine disease is certainly not sufficiently appreciated, neither by the profession nor by the female sex itself, who are so seriously affected by their neglect. The pernicious effects of gravitation, pressure, impeded reflux of blood, active or passive congestion, on the exceedingly moveable internal sexual organs of the female, when unduly protracted or habitual, are so evident as scarcely to need demonstration.

Of the several varieties of posture, the erect, sitting, reclining, recumbent, and prostrate and kneeling postures, Dr. A. says that they are all proper, if properly employed, and if one is not persisted in for too long a time.

1. The *erect posture* can be maintained only a short time, unless the body be kept in motion; rest in the vertical posture is contrary to nature; in it the body becomes wearied, and the evil effects of prolonged gravitation in one direction and of impeded venous circulation are produced.

2. In the ordinary *sitting posture* on high chairs (one of the "machines which civilization has invented for the torture of mankind, and of woman-kind more especially"), the legs dangle, circulation in the thighs is impeded, and swollen feet, ulcerated legs, and varicose veins are the result; this evil is by no means remedied by using soft easy-chairs, for they enable the person to sit too long in one position, thus again favoring stagnation and gravitation.

3. The *reclining posture* would be natural and beneficial were it properly employed; but most women recline on the back, in which position the pelvic organs are pressed towards the outlet of that cavity; this dorsal reclamation should be entirely avoided by women.

4. The *recumbent posture* is of great service in restoring the balance of the circulation; but, like the reclining posture, it is beneficial only if not practised on the back.

5. The *prostrate and kneeling postures* are natural; and if properly employed (with bowed head), salutary and beneficial in removing pelvic hyperæmia and congestion; unfortunately for the female sex they are not in vogue.

The postures nearest approaching the normal, and the least injurious, are decided by Dr. A. to be the following: *erect*, only when the body is in motion; *sitting* on a perfectly level plane, as the child sits on the floor, or on a slightly elevated seat, like the Turkish divan; *reclining*, on the right or left side and elbow, or on the abdomen and elbows; *recumbent*, on the side, right or left, on a level plane, neither soft nor hard. None of these positions, however comfortable and beneficial it may be for a time, should be maintained until weariness ensues.

The displacements of the pelvic organs produced by posture vary greatly in degree in accordance with the nature and constancy of the posture. The organs most liable to displacement are:

1. The *bladder*—retroversion or prolapse, from continued over-distension during the erect posture, aided by too slight pelvic inclination and relaxed vaginal walls; 2. the *uterus*, the great mobility of which renders it particularly liable to dislocation:—prolapse and eversion of the external os, retro-prolapse, (prolapse of a retroverted uterus), elongation (the sacro-uterine ligaments being tense, the weight of the bladder in the erect posture drags down and elongates the cervix uteri, without materially displacing the fundus), torsion

(generally by tumors attached to the fundus, the body being in the horizontal posture), ascension (in the lateral recumbent position, the uterus is relieved from intra-abdominal pressure and gravitates away from the vulva, thus replacing itself), antorsion¹ (facilitated by the normal anterior inclination of the womb, therefore exceedingly frequent), retrorsion (favored by the dorsal recumbent and reclining postures, especially when the uterus is enlarged and its supports relaxed, as after confinement), dextrorsion and sinistrorsion (especially the former, because most women are accustomed to sleep on the left side); inversion (only under exceptionally favorable circumstances, as after confinement, or through tumors hanging from and dragging down the fundus), protrusion (ventral, inguinal and other herniæ); 3, the *ovaries and Fallopian tubes* (the former by their loose attachment are liable to gravitate about in the pelvic cavity, either independently or with the uterus, and are frequently found prolapsed behind the uterus, especially in retrorsion of that organ), prolapsion, ascension (if enlarged, in the recumbent posture, they may slip up as far as the diaphragm by the own weight), protrusion (in inguinal, crural, and other herniæ); 4, the *rectum*—prolapsion. Displacements of the vulva, urethra and ureters through gravitation scarcely ever occur, except as consequences of the above-named deformities.

The postural treatment for these various displacements consists principally in the adoption of a posture exactly the reverse of that which originated or favored the displacement; as a rule, the lateral reclining or recumbent decubitus should take the place of the erect posture whenever practicable; in certain cases, as of prolapsion and retrorsion of the uterus, and prolapsion of the ovaries, the prone or knee-elbow position is the one to be preferred.

The pelvic disorders of hyperæmic origin fairly attributable to posture are chiefly the following: varix, hemorrhage, hematoma, thrombosis, hypersecretion, œdema, hypertrophy, ulceration and hyperæsthesia, all or any of which may occur in each or all of the pelvic organs. The postural treatment of all is pretty much the same, viz.: the change from the usual erect to the recumbent posture, if necessary with raised hips, occasionally to the knee-elbow position.

It is scarcely possible to give, in a report of this kind, more than an exceedingly brief general review of Dr. Aveling's interesting paper; its numerous valuable hints and suggestions can be appreciated only on careful perusal.

A more special employment of posture, combined with air-pressure, in gynecological practice has been recommended by DR. HENRY F. CAMPBELL, of Augusta, Ga., (*Atlanta Med. and Surg. Jour.*, May, 1875,) in a paper entitled "POSTURE, PNEUMATIC PRESSURE, AND MECHANICAL APPLIANCES IN UTERINE DISPLACEMENTS." He asserts that all uterine displacements, but particularly retro-deviations, can readily be replaced in the knee-and-breast (not the knee-elbow) position, aided by pneumatic pressure, the air being introduced into the vagina by opening the vulvar orifice; in that position the abdominal viscera gravitate away from the pelvis; but this traction would not in itself replace the dislocated womb, were not "the suction broken" by the admission of air into the vagina, which forcibly distends that canal and instantaneously replaces the uterus. He recommends, not only that every uterine displacement be replaced in this manner by the physician, and that no pessary be introduced in any other posture than the knee-breast, in order to avoid the forcible pressing-up of the dislocated portion by the instrument; but, above all, that every woman suffering from uterine displacement be instructed to assume the knee-breast posture, and replace her own uterus by air-pressure every night on retiring to bed. This, a very simple and inexpensive instrument, devised by Dr. Campbell, and called by him the "pneumatic self-repositor," enables her to do with perfect facility and convenience. It consists of a glass tube, 2½" to 3" long, with a bulbous extremity, similar to a plain cylindrical speculum, and coming in various sizes, the mere momentary intro-

¹ Dr. Aveling has coined new words: *prolapsion*, *antorsion*, *retrorsion*, *dextrorsion* and *sinistrorsion*, for the old familiar prolapsus, anteversion, &c.

duction of which by the patient herself, permits the air to enter and replace the uterus. A night's rest, with unstretched uterine ligaments and unimpeded circulation, will, in Dr. Campbell's opinion, go far in favoring a restoration to a perfectly normal position of the organ.

Dr. SOLGER, of Berlin, has accidentally made a similar observation of THE INFLUENCE OF POSTURE AND PNEUMATIC PRESSURE ON THE REPOSITION OF A RETROFLEXED GRAVID UTERUS (*Berlin Obst. Soc.*, May 11, '75; *Berlin Beitr. z. Geb.*, IV., 1); having succeeded in replacing an almost incarcerated retroflexed uterus in the fourth month by air-pressure, after vainly trying other means, the air having accidentally entered the vagina between his two fingers, while he was endeavoring, in the knee-elbow position, to draw away the cervix from the pubis, and thus dislodge the fundus.

Exactly the same lucky accident occurred to me a few months ago (entirely independent of the observations of Drs. Campbell and Solger, whose papers I had not then read), while attempting to replace a retroflexed gravid uterus of the tenth week, in which all other means had failed; the patient being in the knee-elbow position, I lifted up the perineum sharply with Sims' speculum, in order to seize the cervix with a tenaculum, when I suddenly noticed the vagina to be distended like a balloon, and found the cervix almost as far back as the fundus had been, which had disappeared from the sacral excavation; the uterus was replaced, and I kept it so by at once introducing an Albert Smith pessary, which I removed at the beginning of the fifth month.

I have been thus explicit in referring to these several papers, even so far as to encroach on the field of Obstetrics for a moment, because I am convinced that posture, with or without pneumatic pressure, is a remedy of great value in the treatment of uterine displacements, and is by no means sufficiently appreciated and practiced by the profession; and because I believe that the two measures combined, intelligently and perseveringly employed, would be extremely beneficial to the female sex.

Following in the lead of Prof. Schatz, of Rostock, who employed manometry to test the amount of intra-uterine pressure or expulsive force during parturition, Dr. ELY VAN DE WARKER, of Syracuse, (*N. Y. Med. Jour.*, April, 1875), has studied the NORMAL MOVEMENTS OF THE UNIMPREGNATED UTERUS by means of the manometer, an instrument consisting substantially of a rubber bag, distended with water, and communicating with a column of mercury, the oscillations of which cause a pen to move and trace curve-lines on a cylinder covered with blackened paper. The measurements of voluntary expulsive efforts were made with a manometer without the pen and cylinder, but merely with a column of mercury in a bent glass tube. The forces causing the minor uterine movements are twofold: 1. *The contraction of the abdominal muscles*; 2. *Visceral (abdominal) pressure*. Both these forces vary in different women, in accordance with the contractile power of the muscles, and the relations of the axes of the pelvic cavity and the abdomen. The uterine movements due to posture (sitting, standing, squatting, lying), and those due to respiration, coughing and articulation, are classed under cause 2; those uterine movements dependent on cause 1, are entirely voluntary, and show themselves during voluntary expulsive efforts, which may be exceedingly forcible at times. If the minimum expulsive force exerted during labor is from four to eight pounds, according to Poppel and Duncan, it is evident that the non-pregnant woman possesses voluntary expulsive power at any time equivalent to an easy labor, and sufficient to produce a displacement of the uterus under favorable circumstances. Posture has a marked influence on voluntary expulsive effort, as it also has on visceral (abdominal) pressure, as instanced by a difference of pressure equivalent to 5-10 of an inch of mercury between the positions of sitting and standing, and 6-20 of an inch between those of lying and sitting. In the squatting positions, the pelvic organs are entirely relaxed from superincumbent visceral pressure.

Respiration movements. These are very simple, and equal in rise (inspiration) and fall (expiration), the oscillation being almost absent (a mere wavy line) during quiet respiration in the horizontal posture. *Articulation curve.* The tracings

during standing and conversing are of almost endless variety, articulate words being indicated by wavy points in the downward curve. In *coughing*, the up and down tracings are quick and sharp, as might be expected. *Vibration curves* are most marked during walking, although marked in all uterine movements, except those of natural respiration. As one foot touches the floor in advancing there is a slight upward movement in the distal side of the instrument, equivalent to a descent of the womb; as the opposite foot is raised and advanced, there is a descent in the mercury, corresponding to an ascent of the organ. These two main movements are complicated by angular irregularities probably due to vibrations. If adhesions exist, the curve-lines are less pointed and much less sudden and quick, showing marked restraint to free uterine movement. *Pseudo-cyclical expulsive curves*. These are caused by bearing down or straining, and are voluntary; there appears to be little, if any, vaginal contractility engaged during this effort. This expulsive force varies greatly in different women; from three to five-tenths of an inch vertical uterine motion appears to be the average; some women can scarcely force the mercury up 2", while others can exert a force equal to 4", but none could hold it at that point. The *Movement curve* expresses the varying amount of vaginal tension from abdominal visceral pressure during the following positions: (*a*) On sitting up from the horizontal position, a sudden rise from zero; (*b*) on standing up, a slight further rise; (*c*) when stepping upon the floor, a vibration movement; (*d*) on squatting, a sudden fall of five to eight-tenths of an inch; (*e*) on bending forward, a slighter descent; (*f*) on resuming the standing position, a quick but tremulous ascent; (*g*) on sitting down, a slight fall; (*h*) on lying down, a return to zero. *Action of the vagina in uterine movements*. The upper part of the vagina partakes of uterine movement, as evidenced by vibration; but if the uterus descends below the point of elastic vaginal resistance, that canal then brings into play inhibitory vaginal action. The contraction of the vagina is a measure of its power as a supporting column. This fact was proved by the greatly impaired resistance of the vagina to a distending force in four cases of cystocele, compared to the contractility in the other subjects, in whom the parts had maintained their functions unimpaired. The four cystocele subjects, with a mean vaginal distension of 7.7 fluid ounces, gave a mean pressure of eight-tenths of an inch; the healthy subject, with a mean vaginal distension of 4.8 fl. oz., gave a mean pressure of 1.04 inch, a difference of .24 inch of pressure, which is nearly doubled by the greatly smaller volume of displacing force in the last class. Voluntary vaginal expulsive effort, and intro-abdominal pressure are both about equally diminished in these cases of impaired vaginal contractility. The four cases of uterine prolapse with cystocele give a mean voluntary expulsive effort of 1.3 inch in all positions; four subjects, with normal vaginae, however, give 2. x inches. Another evidence of the value of original contractility as a factor in the sustaining power of the vagina, is the fact that the vaginae of those subjects afflicted with cystocele were capable of twice the amount of distension, without exciting pain, as in the other cases experimented upon. This fact is certainly due, not to the greater capacity of the former vaginae, but to their loss of the faculty of resisting distending force.—Dr. Van de Warker has evinced much mechanical and experimental ingenuity in these investigations, which I trust he will see fit to pursue still further.

In a paper on "THE DIAGNOSIS AND TREATMENT OF THE CURABLE FORMS OF FIBROID TUMORS OF THE UTERUS," (*Obst. Trans. Gr. Br. & Irid.*, Dec., '75) DR. ALFRED MEADOWS says, that the curability of a fibroid tumor of the uterus will depend, in the main, on its position relative to the uterine walls; therefore the exact diagnosis of that position is essential to a proper plan of treatment. For that purpose, he recommends that special attention be paid to several points: 1. The degree and direction of cervical displacement revealed by digital examination. 2. Changes in the os and cervix—as regards the patency of the former, and as regards the size and consistency of the latter. 3. The use of the uterine sound, noting the exact dimensions and shape of the uterine cavity, the direction taken by the sound.

4. The connection between external movement by the hand of the tumor, (if it be large enough), and the oscillations of the handle of the sound in utero. Tumors entirely within the uterus do not displace the cervix; if in the anterior, or posterior, or the lateral walls, the uterus is pushed in the opposite direction. A closed os, with small, nearly normal cervix, are almost positive indications of incurability; a patent os, however, is a good sign, for it indicates growth towards the uterine cavity, and consequently greater facility of removal. Fibroids in the anterior wall are less frequent than in the posterior, as they are also more difficult to diagnose, and more liable to be followed by inflammation after surgical interference, which is probably due to the greater quantity of cellular tissue between the cervix and the bladder.

Of all the internal and external remedies, absorbent and oxytocic, Dr. Meadows employs and values only two, one belonging to the former class, the chloride of calcium, as liquor calcii chloridi, which appears either to control the hemorrhage, or occasionally arrest the growth of the tumor; and the other, ergot, which is to be given only when there is plenty of hemorrhage or other discharge, and where there is some increase of size of the uterus and elongation of its cavity, in accordance with the direction and proportion of the tumor. Both these drugs must be given perseveringly for months; no permanent improvement can be expected under three months. Only the interstitial variety of uterine fibroids is really amenable, save by surgical interference, *i. e.* by enucleation. This is done by dilating (preferably) or dividing the cervix, incising or cauterizing (with actual cautery) the most dependent or salient parts of the tumor; and then either at once enucleating it entire from its bed with the fingers and a strong forceps (care being taken not to work through the uterine tissue into the peritoneal cavity, as has been done), or waiting several days, and then proceeding at intervals. The more the tumor can be safely detached, the more foreign it becomes, and the more the uterus will endeavor to expel it. No other instrument than the forceps is needed to enucleate the growth; the entire absence of a pedicle, unless the operator makes one by drawing on the tumor, renders an *écraseur* unnecessary.

The only curative treatment of sub-peritoneal fibroids is gastrotomy, and only such cases are fit for this operation, in which the tumor is well out of the pelvis, so as hardly, if at all, to be felt per vaginam, and the cervix with a good part of the body, is free from disease; in which the tumor is freely movable, without very much affecting the cervix, and in which the sound shows but a very slight increase in length of the uterus in comparison with the size of the tumor. Those tumors which are attached to the uterus by a pedicle, or are so placed upon the fundus uteri as to permit of its body being used as a pedicle, are the most favorable for operation.

THE USE OF THE ACTUAL CAUTERY IN THE ENUCLEATION OF FIBROID TUMORS OF THE UTERUS, referred to by Dr. Meadows, has been described and illustrated by DR. GREENHALGH, in 5 cases, in a paper read before the *Royal Med. and Surg. Soc.*, Oct. 26, '75, (*Obst. Gt. Br. & Ir.* Dec. '75.) Its advantages over the knife and enucleation consist in, facility of application, the slight amount of pain, absence of bleeding; in the charred opening being unfavorable to absorption; in the ready dilatation of the opening without bleeding; in permitting manipulation through the opening immediately after its use; in the rapid destruction of portions of the tumor, whereby its size is reduced, its lower segment rendered conical, and its detachment, expulsion or removal facilitated. Spontaneous expulsive efforts shortly follow its use. Where the tumor is very large, or the patient much reduced from hemorrhage, the gradual detachment of the growth from its capsule is advisable, as also the removal at each cutting of only so much of the tumor as is external to the opening, thus keeping the opening dilated; in case of sloughing, the speedy destruction of the tumor by the cautery, or its removal by the *écraseur* are indicated. Of Dr. Greenhalgh's five cases, three recovered by gradual spontaneous enucleation of the tumor after the cautery, one died of peritonitis from perforation of the adherent intestine, and the fifth of embolism after the removal

of nearly the whole tumor. Three were intra-mural, one intra-uterine and one extra-mural.

Dr. Barnes remarked, during the discussion, that he was not at all prepared to accept the use of the cautery in this manner, for it very closely resembles the "gouging" process, and would not guard against the low necrotic inflammation, terminating in septicæmia, to which these growths have a particular tendency after operation.—I think that, as a rule, surgeons prefer the knife and enucleation by the fingers and vulsellum forceps, as practised by Sims, Thomas, Emmet, Meadows and others, as safer and equally efficacious. Still in some extreme cases the cautery may be useful.

This practice of cutting into an interstitial uterine fibroid, or 'exciting inflammation within its structure by the use of caustics, is strongly deprecated by Dr. THOMAS ADDIS EMMET, who in a paper on "THE TREATMENT AND REMOVAL OF FIBROIDS FROM THE UTERUS BY TRACTION" (*Trans. N. Y. State Medical Soc.*, 1875), says that, while during eight years he has never lost a case of uterine fibroid in which he removed the tumor by traction (as presently to be described), the only instance during this period, in which he deviated from this method by cutting into the tumor and leaving it to break down, proved fatal. No man has the power to limit to the tumor the inflammatory process which he thus excites; and were the death record as well known as the result in the recovering cases, "no conscientious man would ever attempt to destroy a uterine tumor by disintegration." The operation of the removal of uterine fibroids by traction has been adopted by Dr. Emmet for a number of years; but only about two years ago, while removing a tumor in this manner, did he appreciate the *rationale* of the process. He had noticed that, by strong and steady traction with a double tenaculum or ligature, a sessile uterine fibroid could gradually be dislodged and removed piece by piece; he had also observed a depression of the uterine contour opposite the site of the tumor as the latter was drawn down. In March, 1874, during the operation above referred to, he perceived that the uterine depression, which he had taken for beginning inversion, diminished as the base of the fibroid became smaller; and it suddenly occurred to him that this gradual pedunculation of the tumor was owing to the vigorous muscular contraction of the uterus, especially around the base of the fibroid, by which the latter was gradually constricted and crowded out of its bed. The blanched, bloodless appearance of the tumor plainly indicated the amount of constriction of the pedicle. After the removal of the fibroid, barely a small depression could be felt by the finger in the uterine wall, where the tumor had been attached; the pedicle had become attenuated to the size of a lead pencil. In two of the cases reported by Dr. Emmet, the tumor removed in this manner (pieces being sliced off to make room as it was brought down) weighed 7 and 4½ lbs. respectively. Dr. Emmet says that, while he has successfully removed by traction a number of small fibroids where death has frequently occurred under other circumstances, whenever the tumor was larger than a pigeon's egg he has endeavored mainly to control the hemorrhage and force the growth from its bed towards the uterine canal. This he has attained by constant use of rectal and intra-uterine suppositories of ergot (Squibbs' aqueous extract, gr. xv. to gr. xx to a suppository), which act more powerfully from the uterine than rectal mucous membrane. As soon as the fibroid projects sufficiently, it is seized with a double tenaculum, passed as high up on the growth as possible, and gradually enucleated by traction, which may have to be kept up as long as 1½ hours. The finger-nails of the hand in the uterus and scissors may aid in the removal, if necessary. Loose shreds and portions of the capsule should be removed, hemorrhage arrested, and uterine contractions excited by the injections of hot water, and the uterine cavity injected or painted with Churchill's strong tincture of iodine, which is an excellent styptic and disinfectant. The uterus is occasionally packed with cotton soaked in glycerine, or alum-cotton, and the vagina tamponed; frequent injections of carbolyzed warm water are, however, invariably made. The patient is kept in bed until all discharge ceases or the bed of the tumor fills up. "Under no consideration

would I introduce the persulphate of iron into a cavity," says Emmet. "It is in itself no styptic, and only coagulates the blood, which rapidly decomposes and becomes the source of septic infection."

Dr. W. H. BYFORD, of Chicago, analyzes (*Medical Examiner*, July 1st, 1875.) one hundred and three cases of FIBROUS TUMORS OF THE UTERUS TREATED BY ERGOT, the histories of which he has obtained from journals and correspondents, and answers the question, whether ergot will effect a cure of fibrous tumors of the uterus conclusively, as he thinks, in the affirmative. Twenty-three cases out of the whole number are reported cured; in thirty-eight more the tumors were diminished in size, and the hemorrhage and other disagreeable symptoms removed; nineteen of the remainder were benefited by the relief of the hemorrhages and leucorrhœal discharges, while the size and other conditions of the tumors were unchanged. Of the total number, only twenty-one entirely resisted treatment. This shows results decidedly favorable in eighty-two of the one hundred and three cases. We may still further appreciate the favorable effects of the treatment, by the consideration that in twenty-one cases it was suspended, which is as great a number as resisted treatment. It is a noticeable fact that some of the cases in which the treatment was suspended, were very much benefited by it. The great obstacle to arriving at accurate results, has been the difficulty in carrying out the treatment. Not much uniformity has been observed in the manner of using the ergot. Some recommend and use it hypodermically only, while others administer it hypodermically, internally by the stomach, and in the form of suppositories in the vagina and rectum. The principal objections to the use of the hypodermic method are, the pain inflicted by the needle, and the inflammation and suppuration which ensue in a large proportion of cases. On this account, many patients who began treatment refused to continue it, and their cases were abandoned. Where there has not been too much exhaustion, or too great gastric irritability, ergot has been given internally with beneficial results in a majority of instances, while in a few it seemed to have no influence whatever, where marked benefit had been observed when it was given hypodermically.

Observation. Dr. Byford thinks, seems to show that a fibrous tumor of the uterus may be effected by ergot in three ways:—

1st. It is gradually disintegrated and absorbed. In this way it disappears without any violent or disagreeable symptoms.

2d. Its nutrition is so interrupted as to produce a rapid destruction of its vitality; and hence decomposition within the capsule, and a semi-putrid mass expelled. This process is accompanied with evidences of inflammation of the uterus and toxæmia, more or less grave, according to the size of the tumor, the length of time between the commencement of decomposition and the expulsion of the tumor, and the vital resistance of the patient.

3d. The tumor, in nearly its original condition, is totally or partially expelled from the cavity of the uterus, attended with varying degrees of inversion of the organ. In this condition it becomes amenable to surgical processes for completing its removal.

Dr. E. P. ALLEN, of Geneva, Ill., reports (*Ibid.*) a case of fibroid tumor of the uterus, in which hypodermic injections of ergot twice caused phlebitis of the lower extremities.

Prof. BILLROTH (*Wiener Med. Wochenschr.* 1, 1876) in an article "ON LAPARO-HYSTEROTOMY" discusses the question of the justifiability of the removal of large uterine fibroids by abdominal section, (*gastro-hysterotomy*), and says that up to 1874, the great danger of the operation, the usually comparatively slight discomfort occasioned by the tumors, occurring as they do of such size generally only in women above the age of thirty, and the vivid and horrible recollection of two operations at which he assisted, in which an ovarian tumor was diagnosed, and a large uterine fibroid appeared on dividing the peritoneum, the patients both dying of peritonitis, had made him a decided opponent of this operation. During that year, however, he performed one, and during 1875 two such operations, the first two with fatal result, but the last successful. The reasons which induced him to change his mind and

avow himself an adherent to the necessity of gastro-hysterotomy, are the following:

1. The, to him, new experience, that it is not very rare to find enormous uterine fibroids developing in a short time, even in young girls and married women, which not only distress the patient very much, but also endanger life through œdema and marasmus.

2. The increasing experiences which are continually being made in the most difficult cases of gastrotomy, of which he has performed about fifty.

3. The exceedingly favorable results which he has seen obtained by Karl von Braun in enucleations of fibroids and extirpations of the uterus through the vagina; and

4. Particularly the well-known book of Péan and Urdy: "*L'Hystérotomie; De l'ablation partielle ou totale de l'utérus par la gastrotomie.* Paris, 1873."

Billroth says that Péan's book interested him particularly by its "excellent methodical treatment of the subject, and the modesty of the author in describing the methods peculiar to himself. Doubtless the operation of laparo-hysterotomy has been carried by Köberlé and Péan from the standpoint of accidental success to that of a methodical scientific and artistical procedure; it has become a settled problem in the art of surgery."—It has always appeared strange to me that the French, who have shown so much reluctance to risk the comparatively small dangers of ovariectomy, and who have had such poor success with that operation, should have been so exceedingly fortunate with its much more hazardous sister, gastro-hysterotomy, which here, in America, the birth-place of ovariectomy and the home of many of its most ardent supporters and successful performers, has been looked upon as immeasurably more perilous, and thus far has met with very little favor.

DR. S. POZZI (*Progrès Méd.*, August 14, 1875), writing on this subject, gives his conclusions, as follows:—

1. Abdominal hysterotomy in the treatment of fibroid tumors of the uterus, is, though yet quite novel, perfectly justifiable in many cases, and deserves a definite rank among the operations of surgery.

2. There is no comparison between the indications for gastrotomy in cases of fibroid uterine tumors, and for ovarian cystic disease. The fatal tendency in the latter class of diseases calls for, and justifies, an operation in the larger number of cases, which is not indicated in the immense majority of large fibroid tumors.

3. The operation should be reserved for fibroid, or fibro-cystic tumors of rapid or excessively rapid evolution, and accompanied by dangerous symptoms.

4. The fibroid tumors, which are larger than those referred to above, even when they produce alarming symptoms, require less heroic treatment. Their natural tendency to decrease in size, or to establish toleration of their presence, is well recognized. Experience has demonstrated, that an expectant treatment in these cases is followed by less mortality, than operations for hysterotomy.

5. When, in consequence of an error in diagnosis, gastrotomy discloses a uterine tumor, and not an ovarian cyst, ablation is preferable to leaving the operation unfinished, even though the fibroid tumor be not pediculated.

The operation of REMOVAL OF LARGE UTERINE FIBROIDS BY GASTRO-HYSTEROTOMY has been performed only *nine* times during the past year, as far as I have been able to ascertain, with six recoveries and three deaths.

1. THOMAS KEITH, Edinburgh; fibro-cystic tumor of uterus (*Lancet*, May 15, 1875); patient unmarried, æt. 53 years. Supposed to be ovarian. Neck of uterus thick as the arm, too large for clamp. Steel wire around neck, just above vagina. Uterine cavity not elongated. Fixation in lower angle of wound with strong needle. Weight of tumor, 11 lb. Recovery. Left for home on thirty-seventh day.

2. THOMAS KEITH, Edinburgh; fibro-cystic tumor of uterus (*Ibid.*). Patient single, æt. 44 years, serofulous, anæmic person. Insisted on operation. Tumor found adherent to right iliac fossa, right lumbar region, and partly to abdo-

minal wall. Adhesions separated by hand. Steel wire ligature. Left ovary removed with tumor. Much oozing from adhesions; patient pulseless when placed in bed. Abdominal drainage tube. Stump in lower angle of wound, cicatrised in five weeks. Returned home in six weeks.

3. THOMAS KEITH, Edinburgh; fibro-cystic tumor of uterus (*Edinb. Med. Jour.*, March, 1876). Patient æt. 40 years. Rapid growth. Operation Feb. 15, 1875. Ovaries, uterus, tubes, and cystic broad ligaments. Steel wire around pedicle; stump secured in lower angle of wound; silk and horsehair sutures to abdomen. Weight of tumor, after expulsion of serum, 8 lb. 4 oz. Uterine cavity enlarged to 3", unlike other two cases. Alarming secondary hemorrhage from stump on 9th day, arrested by iron-cotton and pressure; separation of slough on fifteenth day. Recovery; returned home on thirty-second day.

4. C. H. F. ROUTH, of London, (*Obst. Soc. Trans., Obst. J., Gr. Br.*, Aug., 1875); tumor 17½ pounds, of stony hardness; no clamp; ligatures; hyperpyrexia; temperature as high as 107°; pulse, 144; repeated iced baths, as long as three-quarters of an hour; temperature reduced thereby 6° to 7°; recovery.

5. JAS. R. CHADWICK, of Boston, (*Boston Med. & Surg. Jour.*, 1875); tumor 4 pounds, 22" to 23" in circumference; uterine cavity, 7"; patient 54 years of age; double whorled ligature through cervix. Favorable until seventh day; suddenly tetanus; death eighth day.

6. W. G. DRAKE, Atlanta, Ga. (*Atlanta Med. Jour.*, Aug., 1875); removal of entire uterus and Fallopian tubes; recovery. Patient menstruated regularly from the vagina afterwards.

7. THEODOR BILLROTH, of Vienna. (*Wiener Med. Wochenschr.*, 1 and 2, 1876). Multiple Uterine Fibroids. Patient single, æt. 38 years. No adhesions; incision from umbilicus to symphysis; removal of the upper portion of the tumor piecemeal; double wire ligature around cervix, according to Péan; uterus, tubes, and both ovaries removed. Death from collapse.

8. THEODOR BILLROTH, (*Ibid.*). Multiple fibroids. Patient æt. 19 years; single, emaciated, very much debilitated; strong adhesions to omentum; six double ligatures of omental adhesions; ovaries as large as small oranges; ligature *en masse* below the broad ligaments; left ovary only removed. Pedicle comparatively slender; clamp; one drainage tube through the vagina, three through abdominal wound. Recovery; patient left her bed in the eighth week. Weight of tumor, 34 lb.

9. E. BOCKEL (*Centralbl. für Chir.*, 1, 1876). Fibro-cyst, mistaken for ovarian. Tumor between the two horns of a uterus bicornis. Extirpation of uterus and both ovaries; pedicle in lower angle of wound. Patient died of peritonitis in three days.

To these may be added another remarkable case of gastrotony only, by ROBERT BARNES: Large conglomerate of malignant colloid tumors, 23 pounds in weight, proceeding from the omentum; silk ligatures. Recovery.

Two cases of gastro-hysterotomy, performed at the Woman's Hospital in this city, by Drs. T. G. Thomas, in 1874, and E. R. Peaslee, in February of this year, can only be mentioned here cursorily, as they occurred outside of the year 1875. Both patients died within forty-eight hours, of shock and beginning peritonitis.

If I have omitted any cases, either published or unpublished, I shall be glad to rectify the omission in the next number, if the operators will send me reports of their operations.

At a meeting of the *Société de Chirurgie*, GUENIOT presented the following conclusions on the COMPRESSION OF PELVIC ORGANS BY UTERINE FIBROIDS, drawn by him from the observations of Hæe (*L'Union Méd.*, Aug., 1875), and other surgeons:

1. Uterine fibroids, fortunately, very rarely give rise to a complete and enduring occlusion of the intestine, but on the contrary, may frequently induce a more or less serious disturbance in the evacuation of the urine, defecation and the functions of the nerves of the pelvis.

2. Albuminuria and various serious affections of the kidneys occasionally

result from this impediment to the evacuation of the urine. (In 13 out of 14 cases observed by Hæc, death resulted from uræmic intoxication; in eight cases the intestinal canal was also more or less occluded by compression.)

3. It is by no means the largest fibromas which give rise to the worst cases of compression; the most dangerous in this regard are those tumors which are seated in the small pelvis, and which develop within its solid bony walls, instead of rising up into the abdominal cavity.

4. With compression there are various complications which enhance and excite these perilous conditions, such as large accumulations of hard excrement, peritonitis, peritoneal adhesions, ulcerations of the intestine, atony of the intestine, as a result of long-continued use of opiates, etc.

5. So long as the tumor remains in its place, the treatment, wherein enterotomy has played a prominent rôle, is almost entirely powerless; whereas, on the contrary, cases are constantly relieved where it is possible to force the tumor up into the abdominal cavity. (Hæc, in two cases, the patients being in the knee-elbow position, succeeded in pressing up the tumors with the hand in the vagina, and thus saving the patients; the fibroids were prevented from re-entering the pelvic cavity by a proper abdominal bandage.)

DR. HEYWOOD SMITH discusses the well-worn subject of STERILITY FROM UTERINE MALPOSITION AND CONSTRICTION OF THE CERVICAL CANAL (*British Med. Jour.*, July 3, '75), and draws some conclusions from his experience, which seem to me worth reporting, coinciding, as they do, exactly with my own observations. He says that sterility is more frequently caused by antelexion of an acute degree than by any other displacement, except, perhaps, extreme retroversion; because, in the former, the os uteri is lifted out of the posterior cul-de-sac and placed in a position unfavorable for the imbibition of semen. Hospital or dispensary patients generally seek relief for the dysmenorrhœa which almost invariably accompanies the antelexion; in the upper classes, however, the consequent sterility is the condition for which relief is generally sought. The treatment consists in incision, and subsequent dilatation of the narrow canal, particularly the internal os, the dilatation being persevered in for months, and the enlarged canal being kept patent by the occasional passage of a thick sound. Only in this manner will it be possible to prevent the constriction from closing until impregnation ensues, and parturition permanently cures the case. Dilatation alone, by tents or graduated dilators, will rarely be found sufficient to ensure permanent patency.—This has been precisely my own experience in quite a large number of cases during the past year, in which the unwillingness of the patient to submit to the use of the knife, restricted me to tents and Peaslee's and Ellinger's dilators, with the result of finding the cervical canal, after persistent and painful dilatation during several months, if untouched for a few weeks, as narrow as before treatment.—Dr. Smith warns us not to divide the external os too freely, as its excessive patency interferes with imbibition. He also recommends the wearing of an intra-uterine stem after incision. Pregnancy may not occur for a year or more, and still the canal should be kept patent by occasional dilatation.

DR. THAD. A. REAMY, of Cincinnati, publishes (*Clinic*, Aug. 21, 1875) a SUMMARY OF RESULTS FROM BILATERAL SECTION OF THE CERVIX UTERI, obtained by him in fifteen cases, which were the only ones out of several hundred cases of dysmenorrhœa, in which the operation seemed indicated. Of these fifteen, there were *cured*, 1, a virgin, marriage and conception four years after, easy delivery; *relieved for periods varying from three to six months after operation*, 4, in all of whom the symptoms returned with more or less violence; in two subsequent eversion of the lips of the cervix requiring operative interference more than the original difficulty; *permanently relieved but not cured*, 3; *followed by pregnancy going to term* 1; *followed by pregnancy and abortion, apparently from condition of cervix*, 1; *no improvement whatever*, 3; *operation followed by peri-uterine and pelvic cellulitis, imperilling the patient's life*, 2. He concludes by saying, that he considers the bilateral section of the cervix uteri unwarranted, except in rare cases of cervical deformity attended with antelexion; it does but little or no good, often induces conditions more to be deplored than

that for which it is employed, as eversion, etc., and is frequently attended with danger. There are other means at our command, which he does not specify, but which he has found far more satisfactory, and the result of which in a large number of cases he proposes to report at some future time.—I trust Dr. Reamy will soon fulfil this promise; for there are few gynecological operations, the utility and justifiability of which for the cure of dysmenorrhœa and sterility, strange to say, is less settled, than that of the much-lauded bilateral division of the cervix uteri. I have heard very weighty authority in this city express a doubt as to whether the operation is ever productive of benefit in sterility; and still I am confident that it is one of the very first measures thought of and often carried out in cases of constriction of the external os, conoid cervix, and flexion. Accurate and carefully compiled statistics, from numerous sources, of a large number of operations, and the ratio of success as regards relief from dysmenorrhœa and cure of sterility, would settle the mooted question much better than any mere expression of individual experience or belief, and it is time that some step be taken to place the practice in its proper light, either as a benefit or, as a very large part of the profession contend, as an operation to be condemned as productive only of evil.

DR. AUGUSTUS F. ERICH, of Baltimore, in an article entitled, "OBSERVATIONS ON THE TREATMENT OF DISPLACEMENTS OF THE UTERUS," (*Baltimore Physician and Surgeon*) lays down certain rules, and makes a number of practical suggestions, which I shall endeavor to illustrate by commenting on the conclusions which he draws at the end of his paper:

1. "While there are certain cases of uterine displacements, the reduction of which ought not to be attempted," (such as uteri bound down by adhesions and tumors), "there are many that do require mechanical support."

2. "The uterine sound or repositor should not be used in correcting any of the malpositions of the uterus." If the fingers and probangs will not replace the organ, it will surely resist any force that may be safely exerted by the slender metallic lip of a sound or repositor within its cavity.—Dr. Erich, however, does not tell us what to do if the fingers and probangs do not replace the uterus; I think that I express the opinion of the majority of gynecologists, when I say, that I should be sorry to miss the sound or repositor *entirely* in the reduction of some degrees of uterine displacements; but, of course, the operator should be sufficiently expert and careful to properly estimate the degree of force with which he elevates the uterus.

3. "Abdominal supporters have a tendency to increase the downward pressure in the pelvis," and should not be worn when the uterus is displaced.—I think Dr. Erich will not find himself supported in this decision by the experience of others. How is it that a proper *cinture hypogastrique* or abdominal supporter with supra-pubic pad, often entirely relieves the distress of a patient suffering from anteversion, particularly with hyperplasia, and enables her to dispense with an intra-vaginal support? Improperly applied or constructed, as they often are, with the upper strap tighter than the lower, abdominal supporters undoubtedly do more harm than good; but with a sufficient number of straps so adjusted as to press the abdominal contents upwards and backwards, not merely backwards, and kept in place by suspenders, I have frequently found them exceedingly beneficial.

4. "No direct pressure against the os ought to be made by any pessary," because it produces excoriation.—Very true, if it can be avoided, but how else are we to support those old hypertrophied totally prolapsed uteri, which overcome even the most improved forms of cup-and-stem pessaries, and which we would probably soon find crowding down also the slender bars of Dr. Erich's procidentia pessary—(to be described hereafter)?

5. "Anteversion can be corrected by simply drawing the cervix forward, without any direct action upon the body of the uterus."

6. "Retroversion can be corrected by drawing the cervix back into the hollow of the sacrum, the body of the uterus being supported by the anterior wall of the pelvis in anteversion, and the posterior wall in retroversion."

7. "Pessaries for anteversion need not extend behind the os," because by

doing so, they distend the posterior cul de-sac, pull the cervix back and thus really increase the anteversion.—The overlooking of this important point has been, I think, one of the reasons why antro-displacements of the uterus have been found much less amenable to mechanical treatment than retro-deviations, in which this very stretching of the posterior cul-de-sac, as is done by all retroversion and retroflexion pessaries, is exactly the desideratum. Dr. Erich deserves great credit for pointing out this mistake, but I do not see how his U-shaped pessary (one end of which is slightly longer and rests on the posterior wall of the vagina, the other in the anterior cul-de-sac, while the centre of the arch *does* go behind the os and occupy the posterior cul-de-sac—so his diagram represents it) is going to correct it. The latest anteversion pessary of Dr. Thomas, which positively does not reach behind the cervix, but sharply pushes up the anterior vaginal wall, seems to me to fulfil this indication, as it certainly corrects the displacement better than any contrivance of the kind I have ever used.

8. "In procidentia, when an ordinary retroversion pessary cannot be retained, Zwanck's pessary should be tried; and if that fails to keep the uterus within the vagina, Erich's procidentia pessary is indicated."—This consists of thick annealable brass wire covered with vulcanized rubber tubing, and bent in the shape of a ring, loosely surrounding the os, all the pressure being exerted on the vaginal walls near the insertion into the cervix; this ring is open anteriorly, the two ends of the wire being bent downwards and running out of the vulva at each side of the urethra, thence separating until each of them reaches the anterior superior spinous process of the ilium, where it ends in a loop or eye, to which is attached a broad tape, by means of which the pessary is suspended from the shoulders, the tapes being crossed once on the back to prevent their slackening on stooping. Micturition and defecation are not interfered with by this instrument; it can readily be moulded by the physician to suit each case; it can easily be removed and replaced by the patient night and morning; it is more firmly and comfortably supported than by a belt around the waist; and, if as efficacious and durable in practice as its construction and principle are simple and plausible, certainly appears to be an excellent contrivance. I shall certainly give it a fair trial. As for Zwanck's pessary, I had thought it obsolete among advanced gynecologists; it certainly ought to be.

9. "The great desideratum in the mechanical treatment of uterine displacements consists in pessaries that may be shaped and moulded at the bedside, to meet the requirements of the case."—No doubt, but I must beg to question whether "Erich's flexible pessaries," composed of thick annealed brass wire, covered with acid-cured rubber tubing, are not "in any way affected by the secretions of the vagina." The universal experience has been that any kind of rubber pessary, except those made of polished *hard* rubber, is acted upon after a while by the acid vaginal secretions, irritates the mucous membrane and gives rise to an offensive discharge.

10. "Dilatation of the cervix by means of sponge tents softens its tissues and facilitates the relief of flexions."

11. "Intra-uterine stems are retained by the posterior wall of the vagina lying in contact with the os, and require no special means for their retention."—Yes, in antro-displacements; but in retroflexion the cervix does not necessarily press against the anterior or posterior wall of the vagina; and the stem will slip out, unless the uterus be positively anteverted by a lever pessary.

Dr. E. advises the introduction of a stem at once after removing the sponge tent, so "that the uterus, in recondensing and hardening, will be as it were moulded upon this stem;" this, I think, is good advice, certainly for the relief of the flexion, although not every patient may bear the irritation of a stem at that very time. Dr. E. says: "To accomplish a permanent cure of uterine flexions by this plan of treatment, I have generally found it necessary to have the stem worn for about six weeks, arranging it so as to include two menstrual periods in this time." I know that I, and all other gynecologists in the world, would feel as though the philosopher's stone had been found, if we could be positively convinced that uterine flexions could really be permanently cured by

so simple and painless a procedure as dilating the cervical canal with a moderate-sized sponge tent and wearing an intra-uterine stem pessary for six weeks. Unfortunately, as far as I am aware, Dr. E. seems to be the only one who is so successful.

12. "Sponge tents should be very slightly, if at all, tapering." Only by dilating the cervix sufficiently with not more than two tents, can we escape the dangers attending their use; for all deaths reported therefrom occurred after using a larger number than two. Therefore, the tents should be so prepared as to procure sufficient dilatation with at most two, which is not generally possible with the ordinary conical fine-pointed tents, which do not equally dilate the cervical canal and internal os. Dr. E. uses only three sizes, equal to $2\frac{1}{2}$ of the points of Nos. 5, 9 and 13 bougies.—I quite agree with Dr. Erich's objection to the conical tents, which have the only advantage of being more easily passed into a narrow cervical canal.

13. "Flexible sponge tents can be made by carefully manipulating ordinary tents between the fingers."—By *flexible sponge tents*, Dr. E. means tents which are moulded in the fingers to the curve of the uterine canal, in which shape their introduction into an acutely flexed uterus is greatly facilitated.

In an article on "THE CONSTITUTIONAL ORIGIN OF LOCAL UTERINE DISEASES; COMPARATIVE ADVANTAGES OF CONSTITUTIONAL AND LOCAL TREATMENT," Dr. C. D. PALMER, of Cincinnati, (*Clinic* IX, 13 and 14.) gives us the results of his experience as follows:

1. Chronic uterine disease and constitutional derangement usually coexist, on which account a cure can rarely be secured by either alone. A removal of the local trouble by exclusively local means can be obtained only before the general health is involved.

2. A local treatment, no matter how well directed, unassociated with general management, while temporarily beneficial, will uniformly be found to be unsuccessful in accomplishing a complete and permanent cure.

3. Local improvement can generally only be commensurate with the general. This is especially true in all cases of intra-corporeal forms of endometritis, wherein some serious constitutional dyscrasia lies at the root of the evil.

4. Constitutional treatment is of importance, not simply for the healthy modification of disorders of special functions associated with general ill health, but for the direct influence which general medication has upon the local organs involved. A large proportion of all chronic uterine affections being dependent upon constitutional conditions, it follows that constitutional treatment is of first and paramount value.

A triumphant argument in favor of MECHANICAL SUPPORTS IN THE TREATMENT OF UTERINE DISPLACEMENTS is furnished by DR. F. B. WATKINS, of Richmond, Va. (*Virga. Med. Monthly*, Nov., 1875), who publishes a tabulated report of 227 cases of displacements of the womb, in 215 of which he employed mechanical supports. Of these 215, there were 139 cases of retroversion, 49 of anteversion, and 27 of prolapsus. The results of treatment—mechanical and other—are noted as follows: *Retroversion*, complete recovery, 114; partially relieved, 23; slight or no improvement, 12. *Anteversion*, complete recovery, 34; partially relieved, 9; slight or no improvement, 7. *Prolapsus*, complete recovery, 22; partially relieved, 4; slight or no improvement, 2. Total: complete recovery, 170; partial relief, 36; slight or no relief, 21. Those cases marked "Recovery complete," have either remained under the Doctor's eye, or have corresponded with him, and thus satisfied him that there has been no relapse. The form of support preferred by Dr. Watkins is after the Holge's "closed lever" principle, the material, block tin wire or hard rubber, each pessary being moulded exactly to fit each particular vagina. The points of support of the pessary are not in any one particular place, but everywhere all along its line of contact with the vagina; the entire vagina is its support or base. A well-fitting abdominal supporter will aid an internal pessary very materially in correcting an anteversion. All external supports with fixed abdominal and vaginal ends should be discarded, because they interfere with the proper natural mobility of the uterus.

In this respect all gynecologists agree with Dr. Watkins; witness the cup-and-stem supporter for prolapsus devised by Dr. Thomas, the stem of which moves up and down on a spring in order to insure mobility of the uterus. One serious objection, however, to this instrument has lately come to my notice, in a case where it, in other respects, was worn with perfect comfort, namely, the production of imperfect but protracted sexual excitement, similar to that induced by masturbation, and consequent nervous depression; this sensation was so evidently caused by the friction of the supporter in the vagina—disappearing when it was removed, and reappearing within half an hour after its re-introduction—that I was reluctantly obliged to discontinue its use in that case. I regret that Dr. Watkins does not give us any satisfactory substitute for all the external supports he discards, nor does he mention the variety with which he cured his 22 cases of prolapsus.

DR. JOSEPH R. BECK, of Fort Wayne, Inda., reports a somewhat extraordinary case of REDUCTION OF A RETROVERTED UTERUS AFTER SIX ABORTIONS (*Phila. Med. Times*, Oct. 2, 1875). Repeated unsuccessful attempts at reduction of the retroversion, which was of the second degree, the cervix being situated above the pubic arch, and the fundus wedged tightly under the promontory of the sacrum, had been previously made, with different methods, by two other physicians, and by Dr. B. himself; which failure, in the opinion of all three physicians, was due to firm pelvic adhesions. Finally Dr. Beck suggested that his colleague, Dr. Dills, "introduce his hand into the rectum, break up any existing adhesions, forcibly and at all hazards, push the fundus off from and above the promontory of the sacrum, while he at the same time made forcible traction on the os with the tenaculum, and applied heavy pressure from without through the abdominal wall. After a few seconds application of the tremendous force which they were thus able to command, the pelvic adhesions suddenly gave way, the fundus returned to its proper position, with a perceptible jump, while the os and cervix correspondingly descended. . . . The patient had no anæsthetic, (!) suffered only a trifling amount of pain," was able to walk to her carriage, and has had no further uterine treatment.—Whether this method of reduction of an adherent retroverted uterus, however necessary and successful in this case, can be considered "an easy and elegant mode of reduction for any retroversion," (as Dr. Beck terms it) appears to me exceedingly questionable. The sensibilities of this patient must certainly have been blunted by her repeated abortions; and I think Dr. B. is more to be congratulated on his good luck, that the "tremendous force" with which he *easily* and *elegantly* reduced the displacement, did not bring on pelvic peritonitis or cellulitis, than on the success of a method by no means new, but too hazardous for most operators, especially without an anæsthetic.

DR. CARL VON ROKITSKY, JR., describes the TREATMENT EMPLOYED IN VIENNA FOR UTERINE HEMORRHAGE in the *Wiener Klinik* I, 4, April, 1875. Although he gives us no original views, still the clear and precise manner with which he lays down indications and treatment, entitle the paper to a passing notice. His directions are the following:—*Active uterine hemorrhage*, particularly endometritis hemorrhagica, neither constant nor transitory cold applications, but frequently-changed cold compresses to the abdomen, (?) leeches to the vagina or to the lower portion of the abdomen just before the menstrual period; mild laxatives and tonics. *Menorrhagia from general debility*, tonics, iron, systematic hydrotherapeutics; in case of need, stimulants. *Passive hemorrhage*, the most usual form of profuse menstruation, cold, and local astringents, best of which is nitrate of silver (mixture of alum and sulphate of copper brings on uterine colic); the sound should always precede the use of these agents, in order to ascertain the situation, condition, and irritability of the uterus. The lunar caustic should be used only after dilatation of the cervix, a piece about an inch long is introduced by the forceps, or in a quill, the caustic being broken off when it is in position; the pain is slight, lasting but a short time; nausea and vomiting may occur. The first application should be unusually mild, until the sensibility of the uterus has been tried; the caustic may also be introduced through a caustic-holder, or "uterus pistol." Cauteri-

zation one, two, three or four times, every second, third or fourth day, will generally control the hemorrhage, ergot will guard against a relapse. Digitalis, tincture cannabis indica, rue, savin, etc., are nearly useless. There are two methods of cauterizing with fluids, cotton-wool soaked and introduced into the cavity of the uterus, or intra-uterine injection. If care be taken previously to dilate the cervix and to inject very slowly, drop by drop, only three, four, or six drops, the danger of the latter manipulation is reduced to a minimum. (?) Only when there are inflammatory processes in or near the uterus is cauterization of the uterus of any kind to be avoided. Dr. Windelband has recently recommended injections of hot water in menorrhagia and post-partum hemorrhage. An excellent mechanical remedy to arrest hemorrhage is the sponge-tent, which, if the hemorrhage results from some excrescence or hypertrophy of the uterine mucosa, often at once cures the disease. It should only be used in cases of extreme necessity, and then not be left in longer than six or eight hours. Vaginal tampons, plain or astringent, never do more than temporarily to prevent the egress of blood. In *hemorrhage from uterine fibroids*, dilatation of the cervix and injection with tincture of iodine is often beneficial, and hypodermic injection of ergot will be found very useful (also in cases of metrorrhagia from debility, local relaxation and active congestion—P. F. M.) For hemorrhage from cancer, the curette and actual cautery, and when the eschar falls, Wynn Williams's solution of bromine (1 : 5 of water), which may also be injected into any remaining cancerous nodules.

M. NOËL GUNEAU DE MUSSY strongly recommends the use of HEAT IN METRORRHAGIA according to the recommendation of Dr. Chapman, rubber bags filled with hot water of a temperature of 46° C. (115° F.), and higher, being applied to the sacral region, and renewed every two or three hours. In all cases the hemorrhage, even the most obstinate, ceased.

A singular, but not very rare phenomenon in the course of fibrous polypi of the uterus, namely, their intermittent protrusion from the uterine cavity, and subsequent disappearance, which had been noticed by Lisfranc, Aran and Larcher is described by DEMARQUAY AND SAINT VEL (*Annales de Gynéc.* April 1875), under the title of "FIBROUS POLYPI WITH INTERMITTENT APPEARANCE." What at first thought might seem a very strange occurrence, in reality becomes exceedingly simple when its etiology is inquired into. An examination, for instance, reveals a dilated external os, and within the cervical canal is detected a smooth rounded tumor, evidently a polypus. The nearer the menstrual epoch this examination has been made, the more distinct will be the dilatation of the cervix and the tumor. A second exploration, made after the catamenial period, does not succeed in discovering the polypus, and the orifice of the uterus is found closed. That there was a polypus there is unquestionable, that it has not been expelled spontaneously is equally certain; consequently it can only have been absorbed or have withdrawn beyond reach into the uterine cavity. The former supposition is untenable—absorption of a fibrous polypus has not been known to occur—moreover, the tumor is again palpable at or near the next menstrual epoch, when the os again opens, consequently the latter theory must be the true one. What, then, is the explanation of this phenomenon? Simply this: At the time of menstruation, the uterus becomes engorged with blood, its cavity diminishes through the swelling of the mucous membrane, the polypus naturally also becomes more turgid, and by pressure excites the uterus to more or less severe contractions, with the view of expelling its contents, which finally result in the dilatation of the cervical canal and the descent of the polypus. As soon as the menstrual hyperæmia disappears, the tumor again contracts, the cervical canal closes, and the tumor slips back into the uterine cavity, not to reappear until the congestion of the next menstrual period repeats the process. One condition is essential to this phenomenon, namely, that the polypus be sufficiently vascular. The propriety of examining during the menstrual flow in such cases is evident, and the authors are of opinion, that the operation for removal of the polypus is perfectly justifiable at the same time, arguing that an operation during menstruation would be much easier and less likely to be followed by evil consequences, than if the

tumor be allowed to retract, the os to close, and the dilatation of the latter with sponge tent be then required to enable the tumor to be reached.

Dr. GRAILY HEWITT, commenting upon a case of PERIODICAL CONVULSIONS DUE TO ACUTE ANTEFLEXION OF THE UTERUS, said that the cause of the paroxysms was undoubtedly to be sought for in the compression of the delicate nervous filaments pervading the uterine tissue at the point of flexion, especially the concave side of the bend. This spot is the one most exquisitely painful during sounding. The monthly periodicity of the paroxysms is easily explained by the greater degree of uterine congestion, and consequently greater compression at the menstrual epochs. After rectifying the flexion by a Graily Hewitt cradle pessary, the convulsions suddenly disappeared.

Dr. G. WARRING CURRAN describes what he calls UTERINE ASTHMA (*The Practitioner*, Dec. 1875), a variety of asthma not unfrequently met with in patients suffering from uterine tumors, especially fibroids of the uterus or appendages. The paroxysms are more severe during the menstrual epochs, and the disease is unusually obstinate, remedial measures being beneficial only when directed to the uterus. Thus, if the asthma precedes menstruation, the flow should be encouraged by warm bathing and small doses of ergot; if the discharge is excessive, it should be controlled by the same drug. The agent which will relieve the asthma at any time is belladonna, given in the form of extract, by the mouth or in suppositories, or applied on a piece of lint to the dorsal and lumbar spine.

The employment of STRONG NITRIC ACID IN THE INFLAMMATORY DISEASES OF THE UTERUS has not met with the encouragement and approval in this country which, in my opinion, it deserves. The strong recommendation of this agent by Dr. HENRY E. WOODBURY, of Washington (*American Journal of Obstetrics*, February, 1875), appears to be endorsed by Dr. E. CHENERY, of Boston, who reports great success with this practice in several cases of endometritis and menorrhagia (*Boston Medical and Surgical Journal*, Dec. 9, 1875). A case of the former kind, which had not yielded to other acids and the curette, was speedily cured by nitric acid applied thoroughly to the whole inner surface of the organ after dilatation of the cervix. Drs. Chadwick, Bixby, Lyman, and Baker corroborated Dr. Chenery's experience, but warned against the promiscuous application of the acid to the cavity of the uterus proper, unless the patient can keep her bed afterwards.

I have repeatedly used NO_3 , both in my office and at the home of the patient, in endocervicitis and metrorrhagia, and have been exceedingly well pleased with its action. The great obstacle to its regular extemporaneous employment is the necessity of thoroughly dilating the cervical canal before each application; but this is partly obviated by the diminished frequency of application required, generally only once a month. Where the cervical canal is patulous, no agent will cure an endometritis so rapidly as NO_3 . Lombe Atthill's success with the remedy should induce the profession to use it more generally. It is certainly much less painful than nitrate of silver, and no more so than iodine or carbolic acid, and no more dangerous than either.

Following Dr. Woodbury's recommendation, Dr. LEBLOND and Dr. SIREDEY have both employed it with good success (*Annales de Gynécok.*, November, 1875). Dr. JAMES BRAITHWAITE (*Obstetrical Journal of Great Britain and Ireland*, October, 1875), likewise recommends it highly in ulceration of the os and endocervicitis, claiming that its use obviates the necessity for the frequent introduction of the speculum (the great objection to which manipulation, in itself, I do not see), because a few applications, at intervals of several weeks, generally suffice to cure the disease.

Dr. EDWARD J. TILT does not absolutely endorse Dr. Braithwaite's exceedingly high opinion of nitric acid in endocervicitis; and thinks that in comparatively recent cases, nitrate of silver, tincture of iodine, or carbolic acid, suffices; chronic cases are benefited most by the acid nitrate of mercury or by nitric acid; hyper-chronic endocervicitis, with considerable cervical hypertrophy, requires potassa cum calce or some strong acid. Weaker caustics, or mechanical support, or division of the cervix, etc., are mere palliative

measures in the latter class of cases; only strong caustics will make a decided impression on them.

DR. PALASCIANO, of Naples, reports a successful case of AMPUTATION OF THE ENTIRE BODY OF AN INVERTED UTERUS, which could not be returned, leaving only the neck.

A CASE OF UTERINE POLYPUS, IN WHICH HYSTERICAL CONVULSIONS WERE A PERMANENT SYMPTOM, is reported by DR. A. E. SPALDING, of Winnebago, Illinois (*Medical News and Library*, September, 1875). The convulsions dated from the appearance of constant metrorrhagia, two years before, and disappeared completely on the removal of the cause of the hemorrhage—an intra-uterine polypus of the size of a hen's egg.

A CASE OF PERI-UTERINE HÆMATOCELE FOLLOWING ABORTION, is related by DR. S. SCHRANK, in the *Med.-Chir. Centralblatt*, for July 23, 1875. After an abortion at three months, in consequence of a fall, the hemorrhage still continuing, a vaginal examination showed the os to be open and the uterine cavity empty; and vaginal injections of ice-water, ergot, and cold compresses to the abdomen were ordered. During the night occasional gushes of blood, mixed with portions of the decidua, took place. The next day the patient was found extremely blanched, fainting at any movement. No blood was escaping from the vagina, which, as well as the uterus, contained a quantity of coagula. On palpation, a tense swelling of some size, not tender on pressure, and not connected with the uterus, was detected. The symptoms and the suddenness of the appearance of the tumor indicated the presence of an hæmatocele. The blood had evidently passed from the uterine cavity, through the Fallopian tubes, into the peritoneal cavity, an exceedingly rare occurrence when there is no obstacle to its discharge from the os, even though the Fallopian tubes be pathologically patent, as probably was the case in this instance.

DR. LEBLOND strongly recommends the crayons of iodoform introduced into therapeutics by Gallard, and which are used advantageously in superficial ulcerations of the neck of the uterus, extending into the uterine cavity. These crayons are introduced into that cavity and kept there by a tampon of cotton placed in contact with the neck of the uterus. The formula for these crayons is: R Finely powdered iodoform x. grammes; powdered gum arabic 1 centigramme; mucilage sufficient to make a pilular mass. Divide into ten equal cylinders four centimetres long, and dry in the air for twenty-four hours. These cylinders may easily be divided into any desired length. (*Annales de Gynécologie, Progrès Méd.*, May 15th, 1875.)

DR. SALE, of Vienna, reports treating successfully several cases requiring INTRA-UTERINE MEDICATION by introducing GELATINE CAPSULES containing the desired agent into the uterus by means of a pincette.

What seems to me a very excellent plan is recommended by Dr. E. R. PALMER, of Louisville (*Louisville Med. News*, I. 4,) in the treatment of GONORRHOEA IN FEMALES. This affection is often very obstinate, and having presumably been cured, is found still to possess infecting properties, owing probably to the persistence of the disease in some of the deeper-seated vaginal folds. He therefore places the patient in the knee-elbow position, separates the labia, and allows the air to rush in and distend the canal. By this means the vaginal mucous membrane becomes perfectly smooth and every fold is effaced. The vagina is then cleansed by injecting it thoroughly with cool water from a Davidson's syringe, the water being allowed to escape, and the whole vagina is filled to the brim with an astringent solution (preferably, because not staining, sulphate of zinc one tablespoonful to a pint and a half of water), which the patient retains for a few minutes. This application is to be made twice a day, and a *complete* cure of the gonorrhœa may be expected in from two to ten days. Why would not this procedure be beneficial in chronic leucorrhœa, especially in cases where the patient is not able to visit her physician every day, and have the necessary local application of astringents and tampon made by the speculum? This application in knee-elbow position may be made quite as readily by the nurse or any female attendant, as by a

physician, and it would seem probable that many cases of leucorrhœa might thus be cured, which now go on getting, if not worse, certainly not better, under ordinary injections and occasional applications. I have lately employed the knee-elbow position with air-expansion in hospital practice, while making intra-uterine applications with the cotton-wrapped applicator, and have been pleased to see that the influence of gravity seemed in a great measure to obviate the often unavoidable flow of the fluid from the applicator on to the vaginal mucous membrane when the cervix was not dilated.

DR. THOMAS MORE MADDEN recounts another case of METRO-PERITONITIS FOLLOWING THE USE OF THE ORDINARY FEMALE SYRINGE (*Trans. Dublin Obstet. Soc., Obstet. Journal, Gr. Br. and Irel.* April, 1875), which occurred in his practice. The attack came on during the use of an astringent injection prescribed for profuse leucorrhœa, the uterus being slightly prolapsed, and the os patulous. The fluid, forcibly propelled by the almost unavoidable unequal compression of the bulb of the syringe, had doubtless entered the open os and penetrated into the Fallopian tube. The lady barely escaped with her life after an illness of several weeks. Dr. Madden relates similar experiences reported by Tilt, Bennet and Bernutz, and condemns the indiscriminate use of the ordinary vaginal syringe as dangerous and inconvenient. In its place a vaginal irrigator should be used from which a steady gentle stream descends, the force of which can be regulated at will, with scarcely any inconvenience to the patient. With this advice I concur heartily, indeed; for some time past I have been in the habit of recommending the instrument known as the "fountain syringe" (consisting of a large rubber bag, to be hung up when filled, and a long rubber tube, with different mouth pieces), in all cases where gentle and prolonged injection of the vagina and cervix is desirable, and where formerly I permitted the use of Davidson's or some similar rubber syringe. Care should be taken that the nozzle of the syringe should not possess a central aperture, and if one is already present it should be plugged. Patients have repeatedly returned, telling me that the single vaginal injection with a Davidson's syringe of a weak solution of tannin had caused them such severe abdominal pain as to confine them to their beds for several days. The observation that these complaints were principally made by women with naturally wide external uterine orifices, or in whom bilateral division of the cervix had been performed a short time previously, or whose cervical canals I was dilating with Ellinger's or Peaslee's dilators, speedily led me to surmise the true cause of the sudden pain, and to recommend less force in the injection or the use of a fountain syringe.

DR. ALEX. B. PADDOCK, of Knoxville, Tenn. (*St. Louis Med. and Surgical Journal*, Dec., 1875) reviews Dr. Madden's remarks, and relates three cases from his own practice where vaginal injections were followed by abdominal tenderness; several other cases reported by him do not, however, come under this category, as they illustrate the danger of *direct* intentional injections of fluid, medicated or not, into the *uterus itself*, a danger now universally recognized.

DR. RUFUS B. HINTON, of Philadelphia, reports three cases of HYSTERICAL CONVULSIONS (*Phila. Med. Times*, July 31, '75), one of which had been treated in vain by the bromides, valerian, etc., which were speedily and permanently relieved by the inhalation of the NITRITE OF AMYL, in three to five drop doses.

A writer in the *Revue Médicale* calls attention to the danger which physicians may run from the EXCITATION OF THE SEXUAL ORGANS OF WOMEN WHEN UNDER THE INFLUENCE OF ETHER OR CHLOROFORM that excitation occasionally reaching such a pitch as to cause the patient to fancy that she has been subjected to violence. Two cases are related from his own practice, in one of which the physician was accused of rape by the patient, when she recovered, although the husband and a dozen women had been in the chamber all the time. In another, he saved himself by calling in the friends as soon as he noticed the expression of venereal excitement. The third case is that of a lady who came to the writer, complaining that a surgeon had violated her while under chloroform for a trivial operation; the delusion was evident, and the

patient speedily disabused. I myself remember several cases in which chloroform given to women during labor produced evident sexual excitement; and one instance where the administration of ether to a young, vigorous girl, for the removal of a wen, induced frantic efforts to repel the attack of an imaginary violator, as clearly shown by the language and actions of the girl. Luckily the presence of another physician to give the ether had been secured—a precaution which is thus shown to be advisable in administering ether or chloroform to females for another reason, than the mere need of assistance.

A very simple and efficient OPERATION FOR CYSTOCELE has been devised and practised by PROF. STOLTZ, of Nancy, and is described by HEYWOOD SMITH, who performed it once successfully, as follows: A circular piece of the anterior vaginal mucous membrane of about $1\frac{1}{2}$ " in diameter is removed, and a single ligature of stout silk "run" (*i.e.* the stitches made in and out), about one-eighth of an inch from the margin of the wound, all round it, in the same way as a thread would be run round the edge of a bag to draw its mouth in, the end of the ligature being brought out close to the place where it was first inserted. The two ends are then drawn quite tight and tied, the wound appearing as a small corrugated point. In performing this operation, the chief point to be attended to is, that the ligature should be passed at a sufficient distance from the edge of the wound, and that each stitch should be of sufficient depth to prevent its soon cutting out. This operation is much more easily performed and more permanent in its results than Sims' or Emmet's operations, because the whole raw surface (the interior of the bag, as it were) is brought into contact, and consequently a greater amount of contraction and more firm union ensues than in the other methods.

Smith's patient was discharged cured, and when seen two months later, both uterus and bladder remained in their natural position.

Another case of that rather rare operation, EXCISION OF THE COCCYX FOR COCCYGDYNIA, originally proposed and performed by Dr. J. C. Nott, is reported by DR. BURNHAM, of Lowell (*Boston Med. and Surg. Jour.*, May 27, 1875). The cause of the coccygodynia was a fall received 29 years before, four years after which the patient began to suffer from the most excruciating neuralgia of the pelvic organs, especially anus and rectum. The two lower fragments of the bone were found displaced, pressing upon the rectum, and interfering with walking and sitting. They were removed without difficulty, and the result was a complete cure of the coccygodynia, and great improvement in the health of the patient.

Two additional cases of this operation are reported by Dr. Mursick, of Nyack, N. Y., in the *Am. Jour. for Med. Sciences*, January, 1876.

VESICAL IRRITABILITY and CATARRH are becoming more and more amenable to treatment, since the practice of DILATING THE URETHRA AND THE NECK OF THE BLADDER has become more popular through the writings of Heath, Simon, Noeggerath, and numerous others. T. PRIDGEON TEALE has, during the last eight years, frequently treated vesical irritability in the female by dilatation of the neck of the bladder, curing absolutely about a third of 30 or 40 cases so treated by himself and friends (*The Lancet*, Nov. 27, '75). He slowly distended the urethra by means of Weiss's dilator, until it admitted two fingers; slight laceration occasionally occurred, causing pain for a few days; the intended incontinence did not always result, but the tenesmus was relieved. In a few cases the incontinence lasted a few weeks; in one case several months; twice permanent incontinence ensued.

DR. J. W. HOWE, of this city, reports a typical case of cystitis in a female, cured by dilatation of the neck of the bladder. The urethra was dilated by instruments and the fingers, until a glass speculum, nearly three-quarters of an inch in diameter, could be introduced. The urine dribbled from the bladder until the fifth day, when the sphincter resumed control. A rapid and complete cure of the cystitis, which had resisted other treatment over two months, resulted from the dilatation.

An entirely novel application of urethral dilatation has been made and proposed by DR. E. NOEGGERATH, of this city, in a paper entitled "THE VESICO-

VAGINAL AND VESICO-RECTAL TOUCH," read before the N. Y. Obst. Society, Feb. 16, '75 (*Am. Jour. Obst.*, May, 1875). His method consists in palpating the uterus and its lateral appendages by the finger, introduced through the urethra into the bladder, the index finger of the other hand being at the same time in the vagina or rectum. No mention of this proceeding can be found in the text books. He reported having examined thirteen cases in this manner, and to have plainly and distinctly felt the outside of the whole uterus, one or both Fallopian tubes, either in part or to their full length, and in two instances an ovary. No inconvenience whatever resulted from the operation, with the exception of temporary incontinence and local pain for a few days, and even that not in every case. The dilatation was performed in his office, or if anæsthesia seemed desirable, at the house of the patient. Dilatation may be either rapid, in one session, or gradual, in several sessions. For the rapid process, Holt's stricture dilator, Busch's or Ellinger's cervical dilator, Molesworth's rubber dilator, or graduated steel sounds may be used; for the gradual process, graduated bougies or laminaria. The preference should generally be given to rapid dilatation, usually under ether, unless the stoicism of the patient renders anæsthesia unnecessary, and ample time is left for gradual dilatation. If it is desired to explore the upper portion of the uterus, it will generally be found necessary for an assistant to pull the uterus down with a double hook, with the points turned outward, which is passed into the cervical canal. Should it be necessary to examine also the annexa of the uterus, an addition is made to the double hook in the shape of the upper $2\frac{1}{2}$ " of the ordinary uterine sound, which is attached to the central points of the hook, and by the means of which the uterus can be drawn down, and turned to either side of the pelvis, as required. The indications given by Dr. Noeggerath for his method, are: 1. For the diagnosis of obscure tumors in the tissue or in the neighborhood of the womb; 2. To complete the diagnosis of inversion of the womb; 3. To recognize the exact condition in cases of suspected congenital absence or deformity of the uterus; 4. For the early diagnosis of pregnancy; 5. To guard against injuring the bladder in removing part of the supra-vaginal neck, as practised by Huguier and Pippingsköld, in hypertrophy of that part. Dr. N. expressly says, that the vesico-vaginal and vesico-rectal touch is to be confined in its application to certain morbid conditions of the womb, and must by no means be considered a supplement to the ordinary gynecological examination.

Five months after the reading of Dr. Noeggerath's paper, Professor GUSTAV SIMON, of Heidelberg, published the report of a large series of investigations, in which he had been engaged for some time, with the intention of showing the limit to which the female urethra can be dilated without causing dangerous laceration or permanent incontinence of urine, and the various uses to which this operation can be put.—(*Volkmann's Klin.*, Vorträge 88, issued July 15, 1875.)

Simon uses for dilatation a series of smooth hard-rubber, conical plugs, whose diameters are graduated by intervals of one millimetre, the smallest being three quarters of a centimetre, and the largest two centimetres in diameter; thus the largest plug has a circumference equalling 6.3 centimetres, and is about as thick as the forefinger. These plugs are preferable to all other dilating agencies, such as the fingers, various forceps, and many-branched dilators, inasmuch as by their means we can accomplish rapid dilatation with the least possible risk of laceration or injury to the peri-urethral tissues. Anæsthesia being established, for the operation is otherwise very painful, the first step consists in slitting up the external meatus, which is the narrowest part of the urethra; three small slits, two above laterally, of a depth of one-fourth of a centimetre, suffice for all purposes, and are harmless. The plugs being then successively inserted, up to the largest, it becomes easy to introduce the forefinger, and if at the same time the precaution is taken of passing the medius into the vagina, almost the entire length of the forefinger can be utilized for intra-vesical manipulations.

The limits of dilatation are as follows: In adult women, 2 centimetres (.8 of

an inch) in diameter, 6.3 centimetres (2.4 inches) in circumference, can be used without detriment; in sufficiently urgent cases, dilatation may be carried up to a circumference of 6.5 to 7 centimetres (2.5 to 2.7 inches) without the production of any lasting inconvenience. Beyond this latter limit, however, Simon asserts that dilatation would entail a risk of permanent incontinence. In girls aged from eleven to fifteen years, the highest degrees of completely innocuous dilatation seem to be reached when a circumference of 4.7 to 5.6 centimetres (1.78 to 2.14 inches), equalling diameters from 1.5 to 1.8 centimetres (.54 to .63 inch), has been attained. In girls from fifteen to twenty of age, the maximum circumferences range from 5.6 to 6.3 centimetres (2.14 to 2.45 inches), equalling diameters from 1.8 to 2.0 centimetres (.63 to .78 inch). In exceptional cases, justifying the infliction of temporary incontinence, the limits so fixed might be slightly exceeded.

The urethra having been so dilated as to admit the forefinger, it becomes possible to execute a very complete exploration of the bladder, especially by means of the bimanual method of palpation; many operative procedures are also facilitated, and new operations are rendered possible. The indications for the employment of Simon's dilatation of the female urethra are set forth as follows: (1.) The diagnosis of diseases of the mucous membrane of the urethra and bladder, by digital exploration, and by endoscopic examination. (2.) The diagnosis of calculi and foreign bodies. (3.) The extraction of such bodies. (4.) The application of caustics in certain affections of the bladder. (5.) The treatment of fissures of the urethra. (6.) The diagnosis of defects in the vesico-vaginal septum, when the vagina is closed. (7.) The diagnosis of the seat and extent of growths and tumors in the vesico-vaginal septum. (8.) The extirpation of tumors, especially of papillomata, from the mucous surface of the bladder. (9.) The discovery and extraction or excision of renal calculi from the vesical part of the ureter. (10.) The opening of hæmatometra in certain cases. (10.) The cure of colo-vesical or entero-vesical fistula by cauterization of their vesical orifice.

An exceedingly novel and curious application of urethral dilatation has further been made by SIMON, viz.: THE PROBING AND CATHETERIZATION OF THE URETERS FROM THE BLADDER, the idea of which originated with Dr. Tuchmann, of London, who experimented on male individuals, largely on himself. By searching for the inter-ureteric ligament, and fixing it with the finger, a thin probe or catheter can easily be passed into the mouth of the ureter on either side of the ligament, and up to the pelvis of the kidney. He performed the operation of sounding the ureter eight times and the catheterization seven times in eleven different women; in two instances he did not succeed, but the last thirteen attempts were all successful. The practical utility of this manipulation will be for the diagnosis and removal of renal or ureteric calculi, stricture of the ureter and the evacuation of some varieties of hydronephrosis. As yet no such occasion has presented for its employment.

H. BENDELACK HEWETSON reports the case of an unmarried woman, 36 years of age, who had suffered for 15 years from most distressing IRRITABILITY OF THE BLADDER, following acute cystitis, which was PERMANENTLY CURED BY THE FORCIBLE DILATATION OF THE URETHRA AND NECK OF THE BLADDER until the two forefingers could be passed into the bladder.

MR. BELL showed the specimen of an exfoliated mucous membrane of the bladder to the Medico-Chirurgical Society of Edinburgh (*Edinb. Med. Jour.*). Three months previous the patient had been delivered of a still-born child; about a month afterwards Mr. Bell was consulted concerning incontinence of urine. At the mouth of the urethra, which was largely dilated, there was detected what seemed to be a tumor. The fingers could be passed between the bladder and this in all directions. The mass was finally detached and withdrawn. It was found to be the whole mucous membrane of the bladder, coated with phosphate. The patient made a good recovery, though there is still a little incontinence of urine if she walks about much.

MR. CHRISTOPHER HEATH of London (*Lancet*, Dec. 11, 1875) has been

practising RAPID DILATATION OF THE FEMALE URETHRA IN FISSURE OF THE URETHRA AND CHRONIC CYSTITIS with great success. In cases of painful micturition for which no cause could be detected, and which were not amenable to ordinary treatment, rapid dilatation of the urethra with the finger, guided by a director in some cases, commencing with a polypus forceps, procured a speedy cure. A tear of the sub-public portion of the mucous membrane was invariably produced, which might be the continuation of a fissure to which he had attributed the dysuria (as in *fissura ani*), although he had never succeeded in discovering one. Permanent incontinence never resulted. In cystitis with purulent urine, H. has detected, as he believes, an ulcerated condition of the mucous membrane (the whole surface of which can easily be brought into contact with the examining finger, by pressing up the bladder from the vagina); he has applied a strong solution of nitrate of silver (as strong as $\frac{1}{2}$ iij to $\frac{1}{2}$ j, on a mop passed through a small vulcanite speculum, the remedy being exceedingly well borne by the bladder, and has found the urine acid and clear in twenty-four hours.

An interesting case of MALE HERMAPHRODISM is reported by DR. L. ROGERS, of Sandy Hill, Pa. (*Cincinnati Lancet and Obs.*). Miss R., of German descent, aged thirty-eight, had always passed for a woman, first came under medical observation on account of a sensation as though something had given way in the pelvis. An examination showed apparently normal external female genital organs, except that the place of the clitoris was supplied by a penis three inches in length in its flaccid state, which was attached to the upper junction of the labia minora by a web-like membrane. Below this penis, which was perforated and communicated with the bladder, was the female urethra, also communicating, as it seemed, with a separate bladder. The vagina scarcely admitted a catheter, but was easily dilatable, and terminated in a cul-de-sac; there was no uterus, and the patient had never menstruated. In the vagina was found a small tumor like a polypus, attached by a long pedicle to the root of the penis. At the request of the patient the tumor was removed, and found to resemble closely a testicle, its weight was four drachms. The pedicle consisted of an artery and a cluster of tortuous veins.

The operation of POSTERIOR COLPORRAPHY, according to Simon, of Heidelberg, for complete prolapse of uterus and vagina (removing a V-shaped flap of mucous membrane from the posterior wall, point near cervix, and uniting by numerous sutures), has been performed three times in this city during the past year—twice by DR. WM. T. LUSK, at Bellevue Hospital; once by myself. In Dr. Lusk's cases the result was perfect; in mine the predominance of the cystocele renders a second operation for this deformity necessary.

But few new operative modifications have been introduced and placed on trial at the WOMAN'S HOSPITAL in this city during the past year.

DR. THOMAS has endeavored to overcome the frequent failure of the operation for lacerated cervix by various devices, all of which are designed to secure more perfect and permanent adaptation of the edges of the wound, and to obviate the too strong tension of the sutures. For this purpose he has employed three different plans: 1. Before paring the edges of the laceration, two long silver wire sutures are passed through the whole cervix at either side of the prospective *normal os*, and a simple china shirt-button with two holes is slipped on the two wires at each side of the cervix, and slid down to the mucous membrane; the two wires on each side are then twisted, and the anterior and posterior lips thus approximated. The four long wires are used to guide and steady the cervix while the paring of the approximated edges of the laceration on either side is accomplished, and while the sutures are introduced and twisted, both of which procedures are greatly facilitated thereby. After the operation is completed the button-wires are cut off close to the buttons, and the latter are not removed until after the lateral sutures have been taken out and evident union has occurred. An additional advantage of this button plan is that the normal os is thus kept free from encroachment, and the danger of paring and closing it is avoided.

2. To take off tension from, and avoid the cutting of, the sutures in the operations for cervical laceration and vesico-vaginal fistula, the shield represented in Fig. 1 has been used. It consists of simple sheet-lead, and is whittled out

FIG. 1

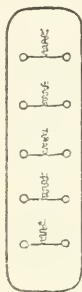


FIG. 2

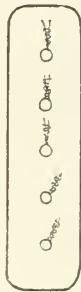
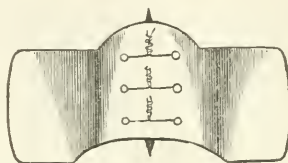


FIG. 3



on the spot to suit the length of each wound. The ends of each suture are passed through the two holes in the shield and twisted. It will readily be understood how tension is thus lessened, a fact not at all obvious in the shield devised by Bozeman for the same purpose in fistula (Fig. 2), where *both* wires pass through the *same* hole and are then twisted.

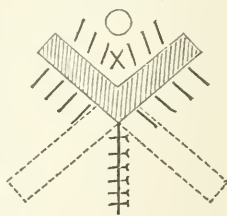
3. Another contrivance to secure close adaptation of the edges of the wound, even below the surface, and also to remove tension on the sutures, is the one shown in Fig. 3. It is also made of sheet-lead, and the approximation of the surfaces of the wound can be increased or diminished by merely pressing together or loosening the two sides of the flexible shield. While considerable hopes were for a time entertained for these devices, Dr. Thomas has finally, as he has just informed me, returned to the old simple interrupted suture; finding that, after all, his results were equally good, and that the benefit accruing from the above innovations hardly compensated for the extra trouble they incurred. I should not have mentioned them, had they not seemed to me exceedingly plausible and ingenious, and did I not think that in certain difficult cases of lacerated cervix or fistula they might really prove invaluable.

Dr. EMMET informs me that he has modified his operations for cystocele in cases where the redundant vaginal tissue still projects from the vulva immediately behind the urethra after the old operation, by paring off two strips of the original denudation towards the descending rami of the os pubis, and uniting the two edges of each pared surface by sutures. (Fig. 4.) The pared surfaces of the whole operation, before the sutures are united, then look like a St. Andrew's cross. The modification accomplishes its object by putting the sub-pubic portion of the vagina on the stretch.

The only work on Diseases of the Female Sexual Organs which appeared during the past year, is the English edition of vol. X. of Ziemssen's Cyclopædia, by Professor Carl Schroeder, late of Erlangen, now occupying the Chair of Obstetrics at Berlin, left vacant by the death, in December last, of the lamented Eduard Martin. The qualities of the book, the points in which it surpasses our native textbooks—etiology and pathology—and those in which the latter greatly excel—clinical history and thera-

FIG. 4

URETHRA



CERVIX

peutics—have been fully discussed in the various periodicals. It is not necessary for me to repeat them, so much the more as the work is probably in the possession of most of the members of the profession. For my part, I regret that I cannot extend the same exceedingly favorable criticism to Prof. Schroeder's second great effort, as I did several years ago to his first, his incomparable "Manual of Obstetrics."

Under the auspices of the Berlin *Gynecological Society*, an off-shoot from the Berlin *Obstetrical Society*, a new gynecological journal has made its appearance during the summer of 1875, the "*Zeitschrift für Geburtshülfe und Frauenkrankheiten*," *Journal of Obstetrics and Gynecology*—edited by PROF. E. MARTIN and DR. H. FASBENDER. Only two numbers have thus far appeared—its issue being at uncertain intervals—both of which contain many interesting articles, especially the second number, which reached us almost simultaneously with the news of the death of its editor-in-chief, Prof. Martin. Who will now take chief lead of the "*Zeitschrift*," and whether it will retain its character for excellence in the future, remains to be seen.

Foremost among those subjects in which the past year has witnessed an advance, must be mentioned the chapter which forms the introduction and perhaps the nucleus of this report—the normal structure of the uterine mucosa, before, during and after menstruation, the nature of the latter function, and its relation to ovulation.

Second, in my estimation, comes the treatment of uterine fibroids—by ergot, enucleation, traction; above all, by gastro-hysterotomy, the percentage of recoveries from which operation (6 recoveries out of 9 operations = 66 $\frac{2}{3}$ %) certainly seems to promise a bright future for this still much dreaded measure.

Third, the vesico-vaginal and vesico-rectal touch, and the daily increasing popularity of the method of treating diseases of the female urethra and bladder by dilatation of the urethra and local applications.

Fourth, the growing appreciation of the influence of posture on the health of the female sex, and the recognition of the value of posture, especially if aided by pneumatic pressure, in the treatment of uterine displacements.

The identity of the first three points with those mentioned in my Report for 1874, shows that the progress of Gynecology during the past year, has been confined more to continuing old researches and perfecting existing methods, than to making new discoveries. Let us hope that this year, from which so many bright promises are dated, may worthily open the second century of American gynecology, and enrich our specialty with numerous original contributions from all parts of the world.

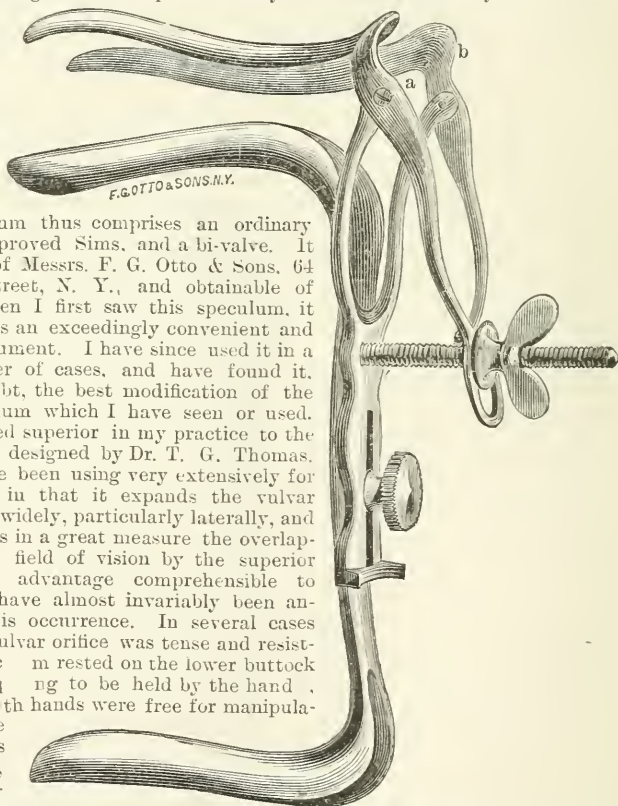
NEW INSTRUMENT.

COMBINED BIVALVE AND SIMS' SPECULUM.

THE speculum shown in the annexed cut is a combination of Nott's bi-valve and Sims' duck-bill speculum, and is chiefly designed to enable the operator to dispense with the otherwise almost invaluable services of an assistant or nurse while using the duck-bill speculum in the semi-prone position. The slit under the screw in the handle permits the anterior branches to be slipped farther forward than represented in the cut, and thus enlarges the vulvar aperture of the instrument. By placing the patient on her back at the edge of the examining-table, the speculum may be used as an ordinary bi-valve, and

by removing the screw and detaching the anterior branches, a simple and efficient Sims' speculum is left.

This speculum thus comprises an ordinary Sims, an improved Sims, and a bi-valve. It is the idea of Messrs. F. G. Otto & Sons, 64 Chatham Street, N. Y., and obtainable of them. [When I first saw this speculum, it struck me as an exceedingly convenient and useful instrument. I have since used it in a large number of cases, and have found it, without doubt, the best modification of the Sims speculum which I have seen or used. It has proved superior in my practice to the modification designed by Dr. T. G. Thomas, which I have been using very extensively for some time, in that it expands the vulvar orifice more widely, particularly laterally, and thus obviates in a great measure the overlapping of the field of vision by the superior buttock, an advantage comprehensible to those who have almost invariably been annoyed by this occurrence. In several cases where the vulvar orifice was tense and resisting, the speculum rested on the lower buttock without requiring to be held by the hand, and thus both hands were free for manipulation. While using this speculum, several simple modifications have occurred to me, by which the utility of the instrument might be considerably increased: 1. By simply grooving the other end of the handle, the sliding-branches could be reversed and used with the smaller blade also. 2. By putting a hinge at the angle of the smaller blade, it can be folded into the concavity of the speculum (Dawson's modification of Sims), and thus be out of the



way when the instrument is used as a bi-valve in the dorsal decubitus. 2. The screws at *a* and *b* may be made to terminate on the inner surface in a short hook, to which the handle of the ring tenaculum (originally devised by Nott) can be attached when the cervix has been seized by it; the uterus is then held as firmly fixed as though the tenaculum were held by the hand. 4. The anterior blades might be so constructed as to permit of their being shortened or lengthened, as in the original Nott speculum. All these modifications, except the last, would but very slightly increase the price, and in no way add to the complexity of the instrument. Other additions might still be made, but would only render the speculum too complicated and costly.—P. F. M.]

REVIEWS AND NOTICES OF BOOKS.

A TREATISE ON THE DISEASES OF INFANCY AND CHILDHOOD. By J. LEWIS SMITH, M.D. Third revised edition, with illustrations. Philadelphia, Henry C. Lea. 1876. Pp. 724.

All who are acquainted with the facilities enjoyed by the author through his large private and hospital practice for the study of diseases peculiar to children, need not be told how eminently fitted he is to write a book on this subject. He has carried the present edition down to the latest date, and has considerably enlarged and thoroughly revised it.

The chapters on Röteln and Cerebo-spinal fever are new, and that on Diphtheria has been almost entirely re-written, in accordance with the most recent observations on the origin of that disease. Dr. Smith's experience in this dreadful scourge has been unusually large, and no one is better qualified than he to discuss and treat it, and no conclusions are more reliable than those drawn by him. The style is easy, often approaching the conversational, and absolutely free from "dryness." The author's descriptions on all subjects are clear and comprehensible, and his deductions positive and precise, wherever practicable. We feel that we are only echoing the voice of the profession by joining in the universal recommendation which the book has received.

CYCLOPEDIA OF THE PRACTICE OF MEDICINE, VOL. IV. DISEASES OF THE RESPIRATORY ORGANS. By Dr. Fraenkel, Prof. von Ziemssen, Prof. Steiner, Dr. Riegel, and Dr. Fraentzel. Translated by Drs. J. Burney Yeo, J. Solis Cohen, J. Brayton Ball, George M. Lefferts, and Edward W. Schauller. Edited by Albert H. Buck, M.D. Wm. Wood & Co., New York, 1876. Pp. 805.

This volume contains the chapters on General Diagnosis and Therapeutics of Diseases of the Nose, Pharynx, and Larynx. Diseases of the Nose, catarrhal inflammations of the laryngeal mucous membrane, croup, diseases of the trachea and bronchi, and diseases of the pleura. The remainder of the diseases of the respiratory system (pneumonia, hyperæmia and anæmia of the lungs, atelectasis, emphysema, gangrene, neo-plasmata, pulmonary consumption or chronic pneumonia, tuberculosis), by Profs. Jørgensen, Hertz, Ruehle, and Rindfleisch, appeared in Vol. V, several months ago. The sections on Pertussis and Edema, New Growths, Ulcers, Neuroses, etc., of the Larynx will follow in Vol. VII., the bulk of Vol. IV. being already much greater than any previous one.

In view of the great popularity and reputation which the series have already achieved, it is necessary to say only that the present volume in every respect equals its predecessors.

A MANUAL OF GENERAL PATHOLOGY. By ERNST WAGNER, M.D., Prof. in the University of Leipzig. Translated from the sixth German edition, by JOHN VAN DUYN, M.D., Prof. in the Syracuse University, and E. G. SEGUIN, M.D., Clin. Prof. in the Coll. Phys. and Surg., N. Y. Wm. Wood & Co., N. Y. 1876. Pp. 728.

Prof. Wagner is so widely known as one of the first German authorities in pathological matters as to render repetition of the fact almost superfluous. That such men as Profs. Van Duyn and Seguin have thought his work worthy of being translated, is in itself sufficient evidence of its excellence.

MEDICAL THERMOMETRY AND HIGH TEMPERATURE. By E. SEGUIN, M.D. Wm. Wood & Co., N. Y. 1876. Pp. 446. With 84 illustrations.

A TREATISE ON SURGERY, its Principles and Practice. By T. HOLMES, M.A., Cantab. Philadelphia, Henry C. Lea, 1876. Pp. 960. With 411 illustrations.

Mr. Holmes is favorably known to the profession through his work on the Surgical Diseases of Children. The size of the type of the present volume enables the author to introduce a very large amount of information, and, as far as we can perceive, every chapter has received the attention it deserves. The work is certainly very full and comprehensive; whether it is equally original is, perhaps, open to doubt. A number of the illustrations are new, and their execution is, for the most part, good.

ON POISONS IN RELATION TO MEDICAL JURISPRUDENCE AND MEDICINE. By ALFRED SWAINE TAYLOR, M.D., F.R.S. Third American, from Third revised English edition. With 104 illustrations. Philadelphia, Henry C. Lea. 1875. Pp. 788.

MORTUARY EXPERIENCE OF THE MUTUAL LIFE INSURANCE COMPANY OF NEW YORK, FROM 1843 TO 1874.

FIFTH-DISEASES AND THEIR PREVENTION. By JOHN SIMON, M.D., F.R.C.S. First Amer. Ed. Boston, James Campbell, 1876. Pp. 96.

COMMUNICATIONS HAVE BEEN RECEIVED from DRs. FORDYCE BARKER, N. Y., on "Vaginal Enterocoele"; BACHE MCE. EMMET, N. Y., on "Diseases of the Bladder, connected with Uterine Displacements"; H. T. HANKS, N. Y., on "Unusual Uterine Hemorrhages"; F. P. FOSTER, N. Y., on "The Pro-lapsed Arm in Transverse Presentations"; BEVERLEY ROBINSON, N. Y., on "The Nature of the Diphtheritic Poison and its Treatment by the so-called Disinfectants"; A. JACOBI, N. Y., on "Masturbation and Hysteria in Young Children" (conclusion from Feb. No. received too late for this number); L. G. W. LIMPET, N. Y., on "Clinical Observations in Uterine Electro-Therapeutics"; GEORGE T. HARRISON, N. Y., on "The Treatment of Post-Partum Hemorrhage"; T. CURTIS SMITH, Middleport, Ohio, on "Pelveo-Peritonitis"; WM. LENNECKER, Chicago, Ill., on "The Surgical Treatment of Primary Retroflexion of the Uterus."

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AND
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VOL. IX.]

JUNE, 1876.

[No. 2.

ORIGINAL COMMUNICATIONS.

VAGINAL HERNIA, OR VAGINAL ENTEROCELE.

REMARKS BEFORE THE NEW YORK OBSTETRICAL SOCIETY, ON A CASE COMMUNICATED BY B. A. CLEMENTS, M.D., SURGEON U. S. A.

BY
FORDYCE BARKER, M.D.. etc., etc.

VAGINAL Hernia of the intestines is, comparatively, of rare occurrence; but a very considerable number of cases have been reported, and the character and phenomena of the affection should be thoroughly understood by all obstetricians. The protrusion takes place at that point of the peritoneal floor of the abdominal cavity which is protected by only one layer of serous membrane, usually lateral and posterior to the uterus; but, in some few cases, it has occurred lateral and anterior to the uterus. It has, in most cases, been developed suddenly, in consequence of violent shocks from falls or strong physical efforts—while, in some few instances, it has seemed to result from difficult parturition; and, in some rare cases, it has been so slow and gradual in its development, that the time of its

commencement has been unknown. The intestine which most frequently protrudes is the ileum ; but in some cases, the colon and the cœcum have been involved.

In one case, in which M. Levret had the opportunity of making an examination after death of the body of a woman who had enterocele in the left side of the vagina, he supposed the hernia to be congenital, or at least, that it had existed from early life, for the acetabular portion of the ileum was much inferior in point of relative position on the side of the hernia than on the other. In this case, the sigmoid flexure of the colon was the part of the intestine which protruded.

The first recorded case was published in the *Memoirs of the Royal Academy of Surgery*, in the early part of the 18th century, by Garengéot. It occurred in a woman about a month after her fifth confinement, as a result of a strong effort in lifting a heavy weight. After a careful examination, Mons. de Garengéot became satisfied that it was a hernia, and succeeded in reducing it by taxis. But in order to convince himself that it was a hernia of a kind of which he had never heard or read of, he desired the patient to get up and walk, and cough strongly. The hernial tumor was again formed, which he succeeded in reducing a second time. He kept the patient in bed until he contrived a pessary which the patient could wear, and she never afterwards suffered any inconvenience from the hernia.

Hain reports one case which occurred in a young unmarried lady, in consequence of violent and repeated efforts to evacuate the bowels, when suffering from obstinate constipation.

Sir Astley Cooper, in his great work on *Hernia*, mentions several cases. He expresses the belief that the reason of its being comparatively rare is, that the oblique position of the pelvis is unfavorable to its production. In the erect, as well as in the sitting posture, the intestines fall rather upon the symphysis pubis than on the posterior part of the pelvis ; and when thus gravitating into the anterior part of the pelvis, they push the uterus against the rectum, and close the space which would be otherwise existing between them ; for he says : " Upon passing my fingers in the dead body from behind the uterus in the cavity of the pelvis, in women who have died a few weeks after delivery, I have found that I could thrust the

reflection of the peritoneum between the uterus and rectum, readily down to the peritoneum."

The symptoms which usually attend vaginal hernia, when suddenly developed, are acute pain, with a sense of fulness in the vagina; in some cases the pain rapidly extending over the whole abdomen, nausea and vomiting, painful and difficult micturition; and, when occurring in the later period of pregnancy, threatened premature labor, and in some cases hemorrhage. When the tumor is developed during parturition, the pains are most atrocious, but are different from the ordinary labor pains, and they arrest the progress of the labor. A soft, yielding tumor is found in the lateral portion of the vagina, generally posterior, but sometimes anterior, to the uterus, which, on pressure, may give a slight gurgling. The pulse usually becomes very quick, there is hurried respiration, pallor, and other symptoms of shock.

When existing before, or developed during labor, it constitutes a very serious complication; but I can find no instance recorded where it has led to a fatal result, either to mother or child—perhaps for the reason that the recorded cases were in the hands of competent and intelligent men who recognized the true cause of the difficulty and removed it by appropriate measures.

Smellie mentions three cases in which this complication with labor existed. In the first case, he gives the details, briefly, of two labors. In the first labor, the hernia could not be reduced, and inflammation and strangulation of the intestine followed. She had a large discharge of blood after the labor, but by fomentation and warm emollient cataplasms, the stricture was overcome and the hernia reduced. On the occasion of her next labor, the intestine was forced down again. The progress of this labor was rapid; Dr. Smellie introduced his hand into the vagina before the head had descended into the cavity of the pelvis, and pushed it up above the sacrum, and thus was enabled to reduce the hernial protrusion. The membranes were ruptured by this operation, the head was forced rapidly down into the pelvis, and delivery was speedily accomplished. In another case, communicated to him by Mr. Stubbs, of Bedfordshire, delivery was accomplished by a similar method of procedure. In the third case, the first appearance of the

hernial tumor occurred nine months before pregnancy, but the patient was accustomed to reduce it herself. But about five weeks before the full period of gestation the tumor had acquired such a volume that she was not able to reduce it all, and for some days she suffered great pain. While Dr. Smellie was examining the tumor, it ruptured, and there was a small discharge of blood and pus, and a full half pint of thin, greyish fluid, when the patient exclaimed that the intestine was gone up, and that she was now free from pain, which before had been so violent. The patient went to her full term, and was delivered by one of the pupils of Dr. Smellie. Some months after her confinement, she was examined by Dr. Smellie, who found that the tumor had kept up. The ruptured part of it presented an appearance of firmness. Five months after, he says, the rupture, unfortunately, reappeared, in consequence of the patient overstraining herself, and she soon afterwards again became pregnant. But the tumor was reduced by one of Dr. Smellie's pupils, and she was again safely delivered at the full term of pregnancy.

M. Hain, in his essay on hernia, relates a case in which entero-vaginal hernia resulted from a fall, which, on the following day, was followed by considerable hemorrhage; and subsequently the husband had occasionally recognized a small tumor in the vagina. Her fourth pregnancy occurred soon after, and in the later months of gestation she frequently experienced pains in the region of the uterus, which caused her great anxiety, as they were quite different from any that she had had in former pregnancies.

When labor came on, it progressed as usual, until the head engaged in the pelvic cavity, when the parturient pain was suddenly arrested by another of great violence. During this pain the head seemed to recede, instead of advancing. On examination, M. Hain found a tumor about the size of a walnut, on the upper and right portion of the vagina, which was exceedingly tender to the touch, and which imparted to the finger a sensation precisely the same as he before had experienced on applying his finger to cases of inguinal and crural hernia. Labor was finally completed without a reduction of the tumor, but this was easily accomplished after the delivery was over. Twenty-four hours after, the hernial tumor

was again formed and again reduced. On the next day the pains returned with great violence. The lochiæ were suppressed, a fever of considerable severity supervened, the abdomen became distended and exceedingly tender to the touch. As he says, a tempest of phlogistic symptoms followed, raging for several days. It is evident, from the description, that the patient had a sharp attack of peritonitis. In two subsequent labors there was a recurrence of similar attack. One labor was terminated by the use of the forceps. This patient eventually recovered, and the hernia seems to have been radically cured.

All the cases reported seem to have eventually been cured by plastic exudation, which sealed up the aperture through which the intestine protruded.

I can find but one fatal case on record, and this is reported by Gunz. A surgeon, it is believed, supposed the hernial tumor to be an abscess, and plunged his bistoury into it. The incision was followed by a protrusion of the cœcum, and a great part of the colon, which were strongly propelled through the incised aperture by the involuntary efforts of the patient.

The protruded bowels were not attempted to be reduced. Gangrene supervened, and the poor woman died.

Vaginal hernia, or vaginal enterocoele, as it is now termed, must be rare, as but a small number of cases have been reported. In the sixteen volumes of Transactions of the Obstetrical Society of London, there is not a single case reported. Yet from the fact that I have met with three cases, I am inclined to believe that it may have sometimes occurred when its true character was not suspected.

CASE I.—My first case was in a lady in labor in her first confinement. I saw her in the early stage of the labor, when she gave no history which caused apprehension; and on a careful vaginal examination, I discovered nothing abnormal. On visiting her four hours subsequently, I found that everything was going on well until some twenty minutes before my arrival, when the pains became very severe, and the character of the pains was quite different from ordinary bearing-down pains. Her appearance also alarmed me, as her countenance had a bad expression, her skin was hot, her pulse was very rapid, 124, if I remember right, and each pain was attended with most distressing vomiting. On vaginal examination, I

found the os well dilated, the head had descended into the pelvic cavity; but on the right side of the vagina I discovered a soft, yielding tumor, excessively sensitive to the touch, which was increased in a marked degree as to size by the pains, which seemed to have no effect on the foetal head. At first I was greatly puzzled as to the nature of the tumor; but, after a few moments, I recalled to mind a case published by the late Professor Meigs in his translation of Colombat de l'Isère on Diseases of Women, and I was soon convinced that I had a case of vaginal enterocele. I placed the patient profoundly under the influence of chloroform; and then, after pushing the head above the sacrum, I easily reduced the tumor, but kept up for some minutes pressure with my hand. This, after a little time, seemed to excite uterine contractions, the head soon forced my hand out; and, in less than a half hour, delivery was completed. The convalescence was rapid, and her recovery was complete.

CASE II.—The second case was that of a lady in the seven and a half month of her fifth pregnancy. She had taken a cottage for the summer at Orange, N. J. In stepping off the platform at the railway station, she suffered a severe shock from a partial fall, which she averted by a strong effort. She was at once seized with a violent pain in the vagina, and felt something protrude from the vulva. With difficulty she was carried into a house near the station. I saw her between five and six in the afternoon, about seven hours after the accident occurred. She then had severe peritonitis, with all the characteristic symptoms, which it is unnecessary for me here to detail. On account of the excessive tenderness of the parts, it was impossible to make a vaginal examination until she was brought fully under the influence of chloroform, when I found a soft tumor protruding from the vulva, and I was not long in determining the existence of a vaginal hernia fully as large as my fist. This was easily reduced, and fifteen drops of Magendie's solution was used hypodermically. I introduced into the vagina a lint pessary, saturated with tannin (Aquæ ζ ij, Tannic Acid ζ i.). Ten drops of Magendie's solution of morphia were used hypodermically every two hours during the night. I omitted to state that while she was under the influence of the chloroform I introduced a catheter, and drew off

twenty ounces of water. On the following day, I took out a competent nurse, and taught her how to introduce the lint and tannin pessaries, the use of which was continued up to the time of her confinement. Eleven days after the accident she was brought back to this city. Five weeks and three days after the accident she was confined, after six and half hours of perfectly normal labor, and without any appearance of the vaginal tumor. Her recovery was rapid and complete.

CASE III.—The third case I saw in consultation with two other medical men. The patient had borne seven children. All her labors had been tedious, over twenty-four hours, and were followed by rather severe hemorrhage and a protracted convalescence. When I saw her, she had been in her eighth labor twenty-eight hours, and she was in a state of violent hysterical mania, while her attendants, her husband, nurse and her sister, and the two physicians, were all completely demoralised. The first eighteen hours of the labor had gone on as in previous parturitions, when suddenly she began to shriek with pain, as she never had done in former labors. She was wholly uncontrollable, and would permit no one to touch or assist her. An eminent obstetrician had been called in consultation, but he left disgusted, as he could get no examination, and the patient refused chloroform. I however succeeded in getting her to inhale it. On first making an examination, I thought her symptoms were due to an over-distended bladder, as I could not find that any water had passed for nearly twenty hours. After drawing off a very large quantity by the catheter, I made a more careful examination, when I found the head beginning to descend in the pelvic cavity, the cervix well dilated, and in addition very distinct evidence of a vaginal enterocoele of considerable size. I succeeded in reducing it with some difficulty. After waiting an hour for uterine action, which was entirely absent, as the patient was very much exhausted, I applied the forceps and delivered her. She was very ill from peritonitis for nearly a week, during which time I saw her daily, but she eventually made a good recovery.

CASE IV.—The following case has been communicated to me by B. A. Clements, M.D., Surgeon U. S. A.

The history of this case is condensed from notes taken at the bedside, and is given as nearly as possible in the words then used.

Mrs. —, age 32 years, above average height and of naturally vigorous constitution, was married at 21 years of age. Had two children—the first a year after marriage—the second two years ago. Has never had a miscarriage or abortion, nor a leucorrhœal discharge of any kind. About a year after the birth of her first child, she was supposed to be suffering from retroversion of the uterus, and wore a pessary with relief for some eight months. Between this time and the birth of her second child, a period of about seven years, she was in poor health, mainly from severe hemorrhoids, for which she underwent an operation in the winter of 1871 that resulted in a complete cure. Her health then greatly improved, and before the birth of her second child in December, 1872, she had greatly increased in weight and strength. After the birth of this child she continued to enjoy much better health than for many years previous, though she suffered habitually from constipation.

On July 16th, 1874, being then in the seventh month of pregnancy with her third child, she made a mis-step from a street crossing; and after several ineffectual but violent efforts to recover her footing, fell forward upon her hands and knees, without however striking her abdomen. She was not pained by the fall, and was able to regain her carriage and rode to her home, three miles distant, complaining only of feeling much "jarred." Soon after reaching her home, she felt an unusual inclination to stool, and on going to the water-closet she discovered a protrusion from the vulva which she herself replaced. On examination very soon afterwards, a somewhat pear-shaped mass was discovered in the vagina, and on ocular inspection it was found to protrude to the margin of the vulva, and was of a pale pink color. It was neither entirely soft nor solid, and was evidently intestine covered by vaginal mucous membrane. It was readily reduced with the fingers without pain being caused. There was no discharge of blood, and neither at this nor at any other or subsequent time was there any indication of a rent in the vagina. During this night she had many symptoms of impending miscarriage, but they passed off within twenty-four hours. She was kept in bed for five weeks; but descent of the tumor invariably occurred when she went to stool.

No change of importance occurred; she went to the end of

her term, and was confined after a brief labor, without any difficulty referable to the hernia, on September 12th, 1874.

For a few days prior to her confinement her urine was albuminous, and her face swollen, and thirty-six hours after delivery, after complaining of intolerable headache, she was seized with violent convulsions, of which she had five in seven and one-quarter hours. It is unnecessary to remark on this complication, further than to say that she was treated by free bleeding (after the third convulsion), chloroform, and morphia, and recovered rapidly.

September 21st, nine days after delivery, she felt exceedingly well, and being in the best spirits she rose from bed, and while on the chamber vessel, at 9 A.M., she was suddenly seized with violent pain in the uterine region. Attention is particularly invited to the accounts to be given of these attacks. The pain rapidly increased in severity—she had shivering, cold extremities, nausea, faintness, and severe headache. The pain soon became intolerable, and at 9.20 A.M. it was necessary to administer chloroform by inhalation; half a grain of morphia was also injected hypodermically. By 1 A.M. her pulse had risen to 120. At 11.10 A.M. I placed her in the “knee-chest position,” and dilating the vagina completely, she in a few minutes thereafter expressed very great relief. Her abdomen became distended and sensitive to pressure, and the pain continued, but not with such severity, and by 4.30 P.M. her pulse was 124. At this time she was seen by Dr. S. G. Moses, of St. Louis, who, on making a digital examination, at once discovered the protrusion in the vagina—the tumor being about the size and shape of a distended thumb of a glove; it was not dense nor quite soft, and extended to about on a line with the os uteri. It was readily reduced, and a soft small sponge inserted as a pessary. By 8.30 P.M. all her symptoms were considerably alleviated. The following day she was better, her bowels moved at 11 A.M., and by the afternoon her pulse had fallen to 100, but her abdomen was still tender and distended. On the 23d in the evening her pulse was 76, but she still complained of her abdomen; much flatus was passed early in the morning of the 24th, which gave her great relief, and at 10.30 A.M. she had a free movement from the bowels, which afforded immediate and great relief to the distended and tender abdomen.

The soreness of the abdomen did not entirely disappear until the 25th, four days after the beginning of the attack.

September 27th.—A digital examination revealed the presence of the tumor in the vagina; it was compressible and easily reduced, and when reduced there was left a small mass of soft folds of the mucous membrane. The position of the spot at which the protrusion occurred was at the bottom of Douglas' cul-de-sac and a little to the patient's right side.

September 29th.—On this day the hernia was found soft, small, and easily reduced, but pressure at the bottom of the cul-de-sac gave pain. Whilst the tumor was still down, in order to determine more precisely whether it was composed of the small intestine or of the rectum, I carefully injected into the rectum three measured quarts of tepid water, and then again examining the tumor it was found to have been in no manner affected. Hence it was concluded to be the small intestine—and that opinion is still confidently entertained up to this time, February 7th, 1876.

Second Attack, October 16th, 1874.—At 8 A.M., whilst stooping down, she was again seized with sudden violent pain in the uterine region—it was attended with shivering, cold extremities, and nausea, and the abdomen became rapidly distended and sensitive. Morphia was freely given, she was placed in the knee-chest position, which gave some relief, and the pain subsided in the course of the afternoon. The next day the tumor and the part at which it protruded were very sensitive, so much so that I did not then attempt to reduce it, though it seemed easily reducible. An operation from the bowels gave great relief. The sensitiveness of the tumor did not disappear until the 18th, when she was again comparatively well.

She was able to ride out daily from the 25th to the 30th of October, but it was found that the exercise caused the hernia to increase in size. October 31st, her menses appeared. Irrigations of two gallons warm water were daily used, and it was found necessary to use daily some form of purgative. Her general health seemed fair, and she again rode out on November 21st.

Third Attack, November 22d 1874.—She rose in the morning, feeling particularly well; but whilst at breakfast she felt pain in the pelvic region, which rapidly increased, and in half

an hour was very severe. The sponge pessary had been applied just before she rose from bed; and there was then no pain or tenderness whatever in the parts. She was at once placed on her knees and chest, but the pain was so severe she could not long maintain this position. Enemas were given without effect. She was then induced to resume the "knee-chest position," the vagina was completely distended, and the tumor disappeared *without manipulation*. Being induced to maintain this position for some twenty minutes, she soon felt great relief from pain, so that by 10 A.M. she had no pain at all. At this time, digital examination revealed great tenderness in the parts concerned. (It will be remembered that there was no tenderness whatever in the early morning, when the sponge was inserted.) The abdomen had become distended, and the hypogastrium was very sensitive. There was no return of the pain, but the tenderness of the tumor continued until the 25th (three days). During these three days, also, the sponge, when removed, was always found tinged with blood, which was believed to proceed from the uterus.

November 30th, 1874.—The border of the hernial opening could now be well defined for the first time—it being circular, thicker, and more indurated. The tumor when prolapsed, seemed smaller, and at times was scarcely perceptible. She rode out occasionally and seemed to be doing well until the

Fourth Attack, December 9th, 1874.—On rising from breakfast she began to have severe and peculiar pain as in previous attacks. The night before she had complained of uncomfortable distention, and also on rising from bed this day. An examination showed but slight descent of the hernia, and entire absence of pain or tenderness in it. She was at once put on her chest and knees, and the vagina distended by air; but this measure did not afford any relief. Enemas were given without avail, and irrigation of the vagina with hot water was also practised. Her abdomen began to swell in half an hour after the pain began, and she had nausea, cold extremities, and shivering. Morphia was freely given, and, her bowels having meantime moved, the pain at 1 P.M. ceased. 3 P.M. pulse was 94 and she felt sore in the hypogastrium. She speedily recovered from the attack, there being less abdominal tenderness than usual.

From this time to the latter part of January, 1875, she was able to ride out at times, but her general health seemed to retrograde. She continued to nurse her infant, but had barely sufficient milk. At times, on replacing the sponge, the uterus would be found quite sensitive. The hernia seemed to descend in proportion to the amount of exercise practised, but she had become wearied of confinement and somewhat despondent, so that I deemed it advisable not to confine her to the house.

January 31st, 1875.—On examination, the margins of the hernial opening are very distinct—feeling like the rubber rings used for children teething—only of about half the calibre, and and of about one and a fourth inches in diameter; there is one tender point on the outer side of this ring, and the vagina is somewhat more relaxed at this point than usual.

February 1st.—The following is noted under this date. ‘From time to time, during the last three weeks, it has been observed that, when the sponge pessary happened to be omitted, there seemed to be no more descent of the hernia than when it was used—provided she remained quiet. I have therefore concluded to discontinue its use for a time, and as soon as the existing inflammation subsides will use astringent irrigations.’ Its use was now finally abandoned.

February 6th.—On examination, the condition of the parts concerned in the hernia is most noticeable. There is a complete well-marked ring about one and a half inches in diameter, at the bottom and right side of the cul-de-sac, feeling callous to the touch; the mucous membrane covering this ring is much more dense than before, and there is scarcely a perceptible descent of the hernia, the vagina only being somewhat prolapsed, the space within the ring being thicker and more dense, the whole giving, it would seem, unmistakable indication that there had been a consolidation and induration of the parts!

February 8th.—An examination, confirmed that of the 6th just noted. It seems evident that there has been some inflammation with plastic exudation in the parts concerned in the hernia, and most likely this began about January 25th, when the endometritis was established.”

Fifth Attack, February 9th, 1875.—At 8 A.M. of this day, whilst at breakfast, the well-known and peculiar pelvic pain again began, and rapidly increased in severity. Examined at

one, there was found to be some descent of the vaginal mucous membrane, but none of the hernia. She was placed in the knee-chest position, but without relief; tepid enemata were also given, but they returned unchanged. Morphine was given. At 9.30 the pain was extreme; there was nausea, the abdomen became distended, and the extremities were cold. By 10.20 she had taken one grain of morphine and began to feel easier, and by noon she was nearly free from pain; but it recurred at 1.20 P.M. and caused excessive nausea and feeble pulse. By 3 P.M. she was wholly free from pain, but the abdomen was swollen, and tender on pressure, and the last named condition did not disappear until February 11th, two days after the attack. The night previous to this attack she had a free operation from the bowels, with discharge of much flatus.

February 19th.—An examination, elicited no sign of tenderness in the uterus or elsewhere, and the uterus was normally moveable in all directions. She expressed herself as feeling ‘perfectly well.’ She was able to ride out by February 26th, and she went out evenings and took frequent rides with much improvement in strength and spirits. The use of a slightly astringent vaginal irrigation was persisted in. An examination on February 26th showed the edges of the “ring” less marked, and there was no sensitiveness of the uterus or adjacent parts. March 11th the menses reappeared, of normal character.

She stated that on March 15th, and again on the 19th, she had a slight paroxysm of the peculiar pelvic pain, which each time came on whilst dressing in the morning, and lasted ten to fifteen minutes, but she made no mention of it at the time.

She now improved very much in her general health, had a more abundant supply of milk for her infant, and felt quite well until the last week in April, when she complained of uneasiness in her uterus and had frequent headaches.

Sixth Attack.—On May 1st she was feeling very well, but whilst at breakfast she felt a return of the pain, which by 9 A.M. became very severe. The abdomen became very speedily distended and sore, but this time she did not have nausea or cold extremities. A digital examination showed a general relaxation of the vagina, and the uterus was lower than usual, and pain was produced on lifting it with the finger. There was no hernia, and the margins of the “ring” were not so well de-

fined as they were on February 6th and 8th. Morphia was given, and she was placed in the knee-chest position, which she at once said gave her much relief; but some pain continued until noon, and the tenderness of the abdomen continued until the night of the next day.

Mrs. —'s health now improved very rapidly, and early in this month (May, '75) she made a journey to the East, and underwent much fatigue in visiting, etc., without any ill results. She continued to gain flesh and strength, and in September weaned her infant, and thought that she had never been in better health in her life—and her weight had increased in October to 158 pounds.

Seventh Attack, after an interval of nearly six months. October 23d, '75.—She had during the day a bad headache, without ascertainable cause, and at night whilst preparing to retire she was suddenly seized with the usual "peculiar" pelvic pain. Her abdomen speedily became distended, tympanitic, and tender, but the symptoms were not as severe as usual. Morphia was freely given, and the pain subsided in the course of the night. A digital examination showed that there was no hernial protrusion, and the "ring" of hard tissue was as last noted in May of this year (1875). The uterus was sensitive to pressure, on lifting it with the finger. Enemas and vaginal irrigations with warm water had no evident good effect. The abdominal tenderness continued all the next day, but she was well again on the 25th October, 1875.

She remained quite well, but continued to be annoyed by habitual constipation, which required the use of aloetic pills. She also menstruated with regularity.

February 7th, 1876.—She had a slight attack of what she regarded as the usual pain at 11 A.M., but it subsided in about twenty minutes without treatment.

Eighth Attack. February 10th, 1876.—Having complained of a feeling of abdominal distention, she had suddenly a return of the pain similar to all the preceding attacks. The abdomen very speedily became tympanitic and tender to the touch, and as the pain increased she had nausea and cold extremities. She was placed in Sims' position without relief, and enemas of warm water were ineffectually used. Morphia was also given freely, and she became easier in the course of the night, but she

was not free from abdominal tenderness until the 12th. A vaginal examination showed that there was no descent of intestine and that the "ring" and adjacent parts were in the condition last noted.

February 15th, 1876.—A digital examination made whilst she was in the usual obstetric position (*not* in the "knee-chest" position) showed the existence of the indurated "ring" as last described—the included surface of this ring was firm but not wholly unyielding—and there was absolutely no descent of the intestine, nor was there any tenderness anywhere in the uterus or adjacent parts. The patient's general health at this date is excellent.

It is desired to have an opinion on the following points especially :

1. The mechanism of (or, perhaps, more correctly, the pathological anatomy of the parts concerned in) the hernia—especially of the indurated "ring."

2. The cause and nature of the attacks of sudden and acute pain.

3. The probable effect of a future pregnancy on the condition of the parts.

4. What means of prevention, relief, or cure can be adopted?

Now, in relation to this very interesting case, I will first remark that the vaginal hernia, which occurred in the seventh month of pregnancy, does not seem to have interfered at all with the process of parturition, nor was the development of the hernia attended with any symptoms of peritonitis, the first attack of which occurred nine days after delivery, when she rose from her bed to evacuate the bladder. Seven hours after this, a soft tumor, of the size and the shape of the thumb of a glove, was discovered in the vagina, which was readily reduced, after which the symptoms of peritonitis were greatly alleviated. Two days after the attack a free movement of the bowels was followed by great relief to the tender and distended abdomen.

Twenty-five days after the first attack, she had a second, much less severe, which was relieved by placing the patient in the knee-chest position, and the free use of morphia. In five weeks after she had a third attack, which apparently came on without any exciting cause from physical effort on her part.

This seems to have been overcome by keeping her in the knee-chest position for twenty minutes. She had a fourth attack, a severe one, a fortnight later, and two months after this a fifth. There was then an interval of nearly three months before she had a sixth attack. During all this time she had been wearing a sponge pessary, daily introduced, and astringent injections were also used. There was no hernial protrusion with the sixth attack, but yet the patient was relieved by placing her in the knee-chest position. It was nearly six months before she had a seventh attack, and four months after an eighth, but the two last were less severe than the former ones.

Now, on a review of this very interesting case, we find that seven out of eight attacks came on soon after rising in the morning, without premonition, and, in fifteen minutes, developed symptoms of acute peritonitis, viz., intense pain, abdominal tenderness, and tympanites, with a very rapid pulse. In the earlier attacks there existed a vaginal tumor, which an hour previous to the attack was not sensitive to pressure when the sponge pessary was introduced; but this hernial tumor immediately became very painful on pressure.

The severe pain of the attacks were overcome by the prompt, wise, and judicious treatment of the attending physician in a few hours, and the abdominal tenderness and tympanites disappeared in two or three days.

I think the cause of these attacks can only be explained on the theory of suddenly developed constriction or strangulation of some portion of the intestine, a condition now well understood by pathologists. It is a curious fact, which I believe is well authenticated, that in some fatal cases of this kind the constriction is not complete, but is found to admit the passage of a finger, or even a larger body; but it is sufficient to excite *spasmodic* contraction, which renders the intestine impervious.

ON PROLAPSE OF THE ARM IN TRANSVERSE PRESENTATIONS.

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WHILE it is my particular object in this paper to call attention to a certain method of managing the prolapsed arm, which may, as it seems to me, sometimes facilitate combined external and internal version ; I am inclined, in view of the conflicting opinions which have been held in regard to prolapse of the arm, and the brevity with which the subject is treated of in the more familiar text-books, to think that a general review of the doctrines held and taught in regard to this complication of transverse presentations may be of service.

It has been suggested, that the term prolapse cannot properly be applied to the descent of the arm in shoulder presentations, since the member forms an integral portion of the presenting part, and that the word should be restricted to cases of such descent in conjunction with a cephalic or pelvic presentation.¹ But, as the expression is in common use and well understood, I can see no good reason for discarding it.

Prolapse of the arm is of very frequent occurrence in shoulder presentations. According to Fritsch,² it always takes place in protracted cases. The hand alone may be found in the vagina, or the forearm or the whole limb, with the elbow either flexed or extended, may be driven down into the vagina, or even beyond the vulva. As a matter of course, it is generally the lower arm which escapes from the uterus, but, in case the pains are powerful, the upper arm also may be pressed down to a certain extent, and, in presentations of the back or breast, or in cases of twins, we may find two arms lying wholly without the uterus. Indeed, Credé³ states that all four of the extremities may prolapse in a transverse presentation. Finally, according to

¹ See Depaul's *Leçons de Clinique Obstétricale*. Paris, 1872-6, p. 625.

² *Klinik der alltäglichen geburtshülflichen Operationen*. Halle, 1875, p. 162.

³ *Klinische Vorträge über Geburtshülfe*. Berlin, 1853, p. 577.

Fritsch, in cases of pendulous abdomen, with a dorso-anterior position, the presenting shoulder may be arrested by the symphysis pubis, the child's belly turn downwards, and the upper arm slip down posteriorly.

The older authors looked upon prolapse of the arm as a complication which necessarily rendered parturition very difficult, but it is now generally considered as, of itself, exerting little or no influence upon the progress of labor, although in some cases it is recognized as materially complicating the situation. On the other hand, it is held to present certain advantages, and, on the whole, is viewed as a favorable circumstance. Hodge¹ says: "In presentations of the shoulder, it is a matter of minor importance whether the arm be retained within the uterus, or whether it glides out of the os uteri, through the pelvis and os vaginæ; yet, upon the whole, it is more favorable, as will be seen, that it should be retained within the uterus." In this opinion Hodge stands almost alone among modern authors. Many writers, as, for instance, Pinard,² state in general terms that they attach no importance whatever to the mere prolapse of an arm, but subsequently, when they come to speak of details, it is evident that they do regard it as of some consequence under certain circumstances. Denman³ says that "there is always less difficulty if both arms present, than if there should be but one arm." I presume he founds this statement on the fact that both shoulders are not as likely to be impacted in the pelvis as one alone is, and that consequently the child will be more readily movable.

As regards the *advantages* which have been supposed to attach to prolapse of the arm, none are more generally recognized than those which are turned to account in *diagnosis*.

In the first place, as to the *presentation*. Having made sure that the prolapsed limb is really the arm and not the leg (for which purpose, if the prolapse be only partial, it is not only justifiable but advisable to make it complete by drawing down the hand gently—even beyond the vulva, so as to allow of in-

¹ The Principles and Practice of Obstetrics. Philadelphia, 1866, p. 357.

² Des Contre-Indications de la Version dans la Présentation de l'Épaulé (Thèse d'Agrégation). Paris, 1875. p. 26.

³ An Introduction to the Practice of Midwifery. New York, 1802, vol. ii. p. 124.

spection), the question arises, *is the child presenting transversely?* According to Kiwisch¹, any very decided prolapse of the arm establishes the diagnosis of shoulder presentation, but De Soyre² records a case in which the whole arm, with the elbow bent, lay within the vagina, and yet the head presented. Depaul³ observes, that we should not hastily conclude that the shoulder presents, even when the prolapsed arm hangs out from the vulva, but should make a careful examination, to discover if it be not a case of true prolapse by the side of the head or breech. There can be no doubt that such an examination should always be made, upon the general principle that no source of corroborative or corrective information should be neglected, and the examination is doubtless facilitated, as remarked by Smellie,⁴ Burns,⁵ Hohl,⁶ von Siebold⁷, and others, by making gentle traction on the arm, which, being thus made tense, serves as a guide for the examining finger.

Having ascertained that the presentation is really transverse, we have next to inquire as to the *position* of the child. Here, too, we derive valuable information from an examination of the prolapsed member, as is amply set forth in all the text-books; by ascertaining which hand it is that is prolapsed, and in which direction the palm looks, the forearm being in a state of supination. It is generally stated that, in this position of the forearm, the palm corresponds to the child's abdomen and the thumb points in the direction of the head. Blundell⁸ advises that for this purpose the hand should be placed "intermediately between supination and pronation." It seems to me that we should not place such absolute trust in this test as the text-books inculcate. It is doubtless true, that in the usual dorso-anterior and dorso-posterior positions the palm, in a state of supination, looks in the same general direction with the abdomen;

¹ Die Geburtskunde. Erlangen, 1851, I. Abth., p. 403.

² *Archives de Tocologie*, vol. i. 1874, p. 571.

³ *Op. cit.*, p. 552.

⁴ A Treatise on the Theory and Practice of Midwifery. London, 1779, vol. i. p. 304.

⁵ The Principles of Midwifery. London, 1809, p. 228.

⁶ Lehrbuch der Geburtshülfe. Leipzig, 1855, p. 952.

⁷ Lehrbuch der Geburtshülfe. 2te Aufl., Braunschweig, 1854, p. 194.

⁸ The Principles and Practice of Obstetrics. Castle's edition. London, 1834, p. 394.

but, apart from the fact that there may be more or less rotation of the humerus, the prolapsed arm may belong to the upper shoulder, in which case the palm would look towards the child's back, or the presentation may be abdominal or dorsal, and then the palm would look towards the head or the breech. It is therefore manifestly necessary to make a careful digital examination of the shoulder and as much of the adjacent parts as can be reached; supplementing this, too, with external palpation and auscultation.

Of what value is the condition of the arm in assisting to establish a knowledge of the *death of the child*? "In brachial presentations," says Blundell, "the putrescency is known by the state of the arm." Rosshirt¹ states, that, on the death of the child, the swollen arm becomes smaller and softer. Chiari, Braun, and Späth² looked upon maceration of the prolapsed arm as justifying a resort to embryotomy in difficult cases. On the other hand, de La Motte³ gives a case in which a physician had pulled on the arm for more than six hours, until it was so swollen, black, hard, cold and insensible, that an ill-informed person would have thought it right to remove it; and yet de La Motte turned and extracted a living child, and the arm regained its natural condition in the course of two or three days. Oslander,⁴ too, observes, that "we often find the prolapsed arm swollen, hard, livid, and even blackish-blue, with the epidermis easily stripped off, without our being able to regard it as a sure sign that the child is dead." He refers these conditions to injury inflicted by the mother or the midwife, the swelling and dusky color being caused by compression of the veins by the os uteri tightly embracing the shoulder. In view of such statements as these, and of the many instances in which the arm has been amputated in the belief that the child was dead, but in which nevertheless it has been born living, we are not justified in concluding from the condition of the arm, in the absence of corroborative evidence, that the child is dead. But, if we can detect muscular movements of the hand or arm, we have sufficient proof that the child is still living, and, if there be no

¹ Lehrbuch der Geburtshülfe. Erlangen, 1851, p. 555.

² Quoted in Scanzoni's *Beiträge*, I., 1854, p. 319.

³ *Traité Complet des Accouchemens*. Paris, 1765, vol. ii. p. 675.

⁴ *Handbuch der Entbindungskunst*. 2te Aufl. Tübingen, 1833, Bd. iii., p. 330.

spontaneous movements, there can be no objection to endeavoring to provoke them by simple measures calculated to stimulate reflex action, such as the application of ice and the like.

Among the advantages not connected with diagnosis, it is generally agreed that, in case version is to be followed by extraction, the prolapsed arm can be prevented from rising by the side of the head, whereby we avoid, at least as regards that one arm, the delay and difficulty which often occur at a critical juncture from the necessity of liberating the arms. This point will be more fully referred to in a subsequent portion of this paper, together with the advantages to be obtained by making use of the prolapsed arm in various operative proceedings.

The *disadvantages* which have been attributed to prolapse of the arm are, that it impedes version, and that it interferes with the introduction of the hand for other operations.

Cephalic Version.—It may naturally be supposed that the expulsion of an arm from the uterus diminishes the probability of spontaneous version by the head; and it may very likely be the case, that, in those instances of obliquity of the head at the commencement of labor in which an accident may either cause the head to engage or establish a frank transverse presentation, the prolapse of the arm may be the determining factor in producing the latter condition. Thus, Fritsch alludes to a case in which, "on account of obliquity of the head, an arm prolapsed, and a shoulder presentation resulted from the action of a few strong pains following each other in quick succession." But, on the other hand, Barnes,¹ in mentioning the rarity of spontaneous cephalic version, says: "But some cases of head-first deliveries have been observed. Pezerat relates a case that seems free from ambiguity. The child was large, the shoulder presenting. Pezerat tried to push it up, but could not. A violent pain drove the head down. Fichet de Fléchy gives two cases. In both the midwife had pulled upon the arm. Balocchi relates a case. It was an eight-months' child. He says the case is unique rather than rare, but still regards it as a natural mode of delivery in shoulder-presentation. Lazzati thinks the descent of the head in these cases is always the result of traction upon the presenting arm. As the expelling power is

¹ *Obstetric Operations*, 2d Am. ed. New York, 1874, p. 151.

exerted mainly upon the breech, tending to drive the head away from the brim, it is indeed not easy to understand how spontaneous action can restore the head, if the shoulder *is* forced low down in the pelvis. Monteggia held the same opinion. He relates two cases, in both of which tractions had been made. I myself have seen an instance of the kind." In a case recorded by the late Dr. Elliot,¹ a second twin presented the arm, with the head in the left iliac fossa, spontaneous cephalic version occurred. "The head of the child was driven into the pelvis, and the elbow turned down so as to pass with it into the world." There is no mention of traction having been made. Dr. Edmund Arnold,² of Yonkers, gives a case of transverse presentation, with prolapse of arm and funis (the latter being traceable to the umbilicus), in which spontaneous cephalic version occurred, after several fruitless attempts to turn by the feet, apparently without any traction having been made on the arm.

As regards artificial cephalic version, Rosshirt includes prolapse of the arm among the conditions unfavorable to its performance, and Pinard quotes from Dubois³ to the effect that the operation is impracticable under such circumstances. Braxton Hicks⁴ says, however: "If, in these malpresentations, the foetal hand protrude, it is even then quite possible to induce cephalic presentation, provided the thorax has not yet descended."

Podalic Version.—Hodge states that spontaneous version by the feet is more apt to occur when the arm is retained within the uterus. On the contrary, Osiander saw five cases of spontaneous breech version—three of the children being mature, and two immature. In all of them the arm was markedly prolapsed [*ragte der Arm weit hervor*]. In one case the midwife was so astonished to see the breech born, after she had pulled long and hard on the prolapsed arm, as to exclaim, "Man könne an der Geburt die Allmacht Gottes erkennen."

According to Credé, artificial version by the feet may be hindered in various ways by the prolapsed arm; by the space which it takes up in the os uteri and vagina, and by its interfering with the recession of the presenting part. Pinard says

¹ *Obstetric Clinic*, New York, 1873, p. 346.

² *Am. Med. Times*, June 8, 1861, p. 367.

³ *Mém. de l'Acad. de Méd.*, 1833, t. iii. p. 476.

⁴ *Trans. of Obstet. Society of London*, vol. v. 1863, p. 219.

that total emergence of the limb from the vulva "can exist only in case of a profound engagement of nearly the whole trunk in the pelvic cavity. Its occurrence prohibits any attempt at version. Not only would it be very difficult to introduce the hand as far even as the os uteri, but the rotation of the child would probably not take place without rupture of either the vaginal or the uterine portion of the genital canal." It will be seen that these remarks apply rather to impaction of the trunk than to the mere descent of the arm.

Denman, Hohl, Schröder,¹ and others, consider that the prolapsed arm can never materially obstruct version, and Tarnier² and Saboia³ declare that it facilitates the operation. It is probable that the latter authors refer, not so much to version itself, as to its issue in the birth of a living child being aided by our being able to prevent the arm from rising by the side of the head during extraction.

There is great difference of opinion as to whether the arm may prevent the *introduction of the hand* for the purpose of version and other operations. Smellie, while making no positive statement upon this point, says, "if the arm that is come down be so much swelled that it is impracticable to introduce the hand," etc. Hohl declares that "experience has shown beyond a doubt that a prolapsed arm may be so greatly swollen as to preclude the introduction of the obstetrician's hand, as far as the elbow, through the vulva, or through the vagina and os uteri, *with safety*, whether for the purpose of turning, or for that of opening the child's thorax." Further on, he speaks of the impediment to the introduction of the hand through the vulva as causing pain to the woman and endangering the perineum, cramping the operations of the hand in the vagina, and interfering with the introduction of instruments." As impeding the introduction of the hand into the uterus, he attaches importance to the arm being very much swollen, and occupying that side of the pelvis in which the hand is to be introduced.

¹ Manuel d'Accouchements, trad. par Charpentier. Paris, 1875, p. 313.

² Des Cas dans lesquels l'Extraction du Fœtus est nécessaire. Paris, 1860, p. 139.

³ Traité Théorique et Pratique de la Science et de l'Art des Accouchements. Paris, 1873, p. 691.

De La Motte met with a case in which the arm was so swollen, and the vagina so dry and narrow, that, although strongly opposed to the practice, he was obliged to remove the arm.

Baudeloeque¹ maintains that the introduction of the hand is not prevented by the swollen arm, but by rigidity or spasm of the cervix uteri. Scanzoni² denies that the swollen arm can prevent the introduction of the hand, and refers the trouble to impaction of the shoulder. He inculcates the practice of holding the arm close against the pubic arch, in which situation, according to him, it no longer encroaches upon the posterior portion of the pelvic cavity. Rosshirt and Fritsch also recommend this manœuvre. Hohl says, however, "We have slung the arm and held it against the symphysis pubis, as taught by Scanzoni, without being able to overcome the obstacle which, in its swollen condition, it opposed to the introduction of the hand."

The truth of the matter seems to be, that, in a pelvis of normal dimensions, the prolapsed arm, if properly managed, cannot, by virtue of its encroachment upon the capacity of the pelvis, obstruct the introduction of the hand into the uterus; but that impaction of the shoulder or rigidity of the cervix uteri is the cause of the difficulty. But, on the other hand, there can be no doubt, I think, that an arm, whether swollen or not, protruding from the vulva, may seriously embarrass the introduction of the hand *into the vagina*. By holding the arm snug up under the pubic symphysis, we undoubtedly secure room enough, as regards the bony canal, but we do not, thereby, enlarge in any degree the vaginal canal. Remembering, then, that, in many cases of turning, more difficulty is met with in introducing the hand into the vagina than in all the rest of the operation, it seems to me that we must admit; that this difficulty may be materially increased by an arm hanging out from the vulva.

As regards the whole matter of the part played by the prolapsed arm in favoring or impeding the satisfactory progress of cases in which version is indicated, I should say: (1) pro-

¹ A System of Midwifery, Heath's Translation. London, 1790, vol. ii. p. 325.

² Lehrbuch der Geburtshülfe. 4te Aufl., Bd. iii., Wien, 1867, p. 61.

lapse does not, of itself, contra-indicate, or seriously embarrass any form of version, except in so far as it may interfere with the introduction of the hand; (2) neither does it facilitate version *as usually performed*; but (3) it does facilitate extraction, thereby increasing the chances of the child being born alive, as will be more fully alluded to hereafter.

What remains to be said can, I think, be best arranged in connection with the various methods which have been proposed for managing the arm, or for assisting labor by manipulations of the member. Without discussing the different expedients which were formerly resorted to, with a view to provoke the automatic retraction of the prolapsed arm, and which have now fallen wholly into desuetude, I will pass at once to the consideration of the measures which are mentioned by contemporary writers.

Replacement.—The old obstetricians considered it highly necessary that the prolapsed arm should be replaced within the uterus, and carried over the child's breast, in the direction of its belly. So thoroughly were they imbued with the notion that the prolapse was in itself a source of grave difficulty, that Mauriceau,¹ following Paré, recommends amputation in case replacement should prove very difficult. According to Schröder, Portal never replaced the arm, and Deventer, in 1701, first showed that it was not always necessary to do so. Hoorn also considered that replacement was not always necessary, and de La Motte, Puzos, Levret, and others stoutly opposed it. Oslander speaks of it as now seen only in the rude practice of country midwives, and says that the arm when forced back into the uterus, at the risk of lacerating the vagina and os uteri, far from facilitating version, renders it much more difficult by getting in the way of the operator's hand. Siebold says, that unskilful replacement is apt to make the presentation more unfavorable. Burns, Rosshirt, Clay,² and Schröder teach, that it should never be attempted. Scanzoni includes forcible replacement among the "crude and horrible proceedings"

¹ *Traité des Maladies des Femmes Grosses, et de celles qui sont Accouchées.* 6me éd. Paris, 1721, tome I., p, 310.

² *Handbook of Obstetric Surgery.* Philadelphia, 1874, p. 241.

which are now rejected as wholly superfluous. Leishman¹ states, that replacement should not be attempted in cases of impaction of the shoulder. Hodge, quoting from a paper on a method of rectifying shoulder presentations by correcting the obliquity of the uterus, published in 1850, by Dr. M. B. Wright, of Cincinnati, says: "Should the arm be prolapsed, it is advised to re-introduce it, and carry it, as far as practicable, toward the breast of the child." Credé observes, that by replacement the various difficulties in version, which are due to the prolapse, are overcome, but that it is possible only when the presenting part is still, to a certain extent, movable. De La Motte attempted, in a case occurring early in his practice, to carry out the teaching which he had received, but found it impossible to replace the arm.

Hicks, in describing his method of combined external and internal version, says: "Where the arm has not descended so low as I have supposed above, but yet has fairly come into the vagina, it will be advisable always to endeavor to return it on to the chest of the child." In one of Elliot's cases (No. 100) the House Physician (Dr. Mead) replaced the prolapsed arm, and then performed bi-manual cephalic version. In another case (No. 88) Dr. Elliot performed external cephalic version, after "having carried the arm within the uterus above the chin of the child." Mr. J. Henderson,² in a case of double arm presentation, returned both arms and brought down the feet.

As regards the manner in which replacement should be effected, Credé says: "We accomplish it by means of either an immediate or a mediate manœuvre. In the immediate method we bend the elbow, grasp the limb with the fingers, and, pressing on the hand or the elbow, endeavor to carry it over the child's breast, holding it there a short time, and then carrying the hand further into the uterus, for the purpose of performing version. In the mediate method we introduce the hand past the arm, grasp the presenting part with the whole hand, lifting it gently aside in the direction of its dorsal aspect, and then return to the arm, which we treat as in the immediate method. The mediate method should be tried, even if the presenting part has descended low, and it has the advantage

¹ *A System of Midwifery*. Glasgow, 1873, p. 380.

² *British Medical Journal*. Vol. i., 1875, p. 141.

that at the same time it prepares the way for version, by raising the presenting part."

I can imagine that some advantage might be gained by gently replacing the arm in cases which are seen early, and in which there seems to be a prospect of inducing a head presentation. As regards the "mediate method" of Credé, the preliminary step—the elevation of the shoulder—is of such overshadowing importance, that the operation can scarcely be considered as forming a part of the treatment applicable to prolapse of the arm.

Retropulsion.—Depaul employs this term to express the replacement of an arm prolapsed by the side of the head. I have ventured to apply it, as regards transverse presentations, to a sort of manœuvre which I made use of in the following case :

Mrs. F., about 29 years old, was taken in labor with her third child, at term, towards midnight, April 12th, 1875. She had had vague pains for two days, but on the evening in question she had gone to bed and fallen asleep as usual. About midnight she was suddenly awakened by a copious gush of amniotic fluid, and by severe pain in the back. I arrived at about 1 A.M. of the 13th. On making an examination, I at once encountered the child's hand, lying low in the vagina. It was the left hand, the forearm in a state of pronation, the palm looking towards the mother's right hip, with the thumb directed forwards. External examination showed the abdomen to be somewhat smaller than usual, the long axis of the swelling being nearly transverse. The child's head was resting upon the right ilium, and the breech was situated somewhat higher up on the left side, the back being directed forwards. Muscular movements of the arm in the vagina bore witness that the child was living.

The uterus was acting but moderately, and the presenting part was freely movable. I decided to attempt combined external and internal version. Having introduced my right hand into the vagina (the patient lying on her back), I seized the arm, with the intention of effecting replacement by Credé's *immediate* method, having placed my left hand on the abdomen, above and to the right of the child's head, for the purpose of cephalic version. Somewhat to my surprise, this left

hand informed me that every little movement which I impressed upon the child's arm caused a corresponding movement of its head. I soon found that, by gently pushing the arm upwards, and somewhat towards the mother's right side, *taking care that the direction of the force should coincide with the axis of the arm*, I could raise the head sufficiently to enable me to get my left hand fairly under it. I now abandoned all intention of attempting cephalic version, but continued to urge the head upward with my left hand, until the shoulder rose beyond the reach of my right hand, when, upon introducing my right forefinger into the os uteri, I found the child's left tuber ischii within reach. With the point of my finger I gently urged this along towards the mother's right side, and soon encountered the left foot, which, without any difficulty, I hooked down into the vagina. I immediately brought the foot down through the vulva, rotating it slightly, so that the toes looked backwards.

The whole operation was so quickly and easily performed, that the mother (who was not anæsthetized) supposed that I had simply made an examination. The uterus remained almost quiescent for about six hours, at the end of which time efficient pains came on, and the child was soon born. The arms did not require liberation. The child (a good-sized girl) was moderately asphyxiated, but was readily revived. Both mother and child did well.

Now, I have no doubt that in this case it would have been perfectly easy to turn the child without the manipulation of the arm which I have described, and I do not draw any inference from the facility with which the operation *as a whole* was accomplished; but, when I say that the elevation of the cephalic pole, including the presenting shoulder, was by far the easiest and shortest part of the proceeding, it will be conceded, I think, that the retropulsion of the arm was the chief factor in effecting the child's revolution. I believe that it would prove of service in more difficult cases. Doubtless, like any other manipulation, it should be performed gently and carefully; but, when we consider that the force exerted acts in a line with the humerus, pressing the head of that bone directly against the glenoid cavity, it would seem that, with ordinary caution, there should be no danger of injuring the shoulder.

It seems to me that it would be perfectly safe to use as much force in this way as would be justifiable in any other way, in an attempt to raise an impacted shoulder; at all events, it is well known that the greatest difficulty in version is, often, to get the shoulder to rise, and it can scarcely be doubted that cases will occur in which, by making use of some such manipulation as that which I have described, a resort to the old operation of podalic version, by groping with the hand within the uterus, may be avoided. If we can once raise the shoulder, a point will be gained, even if we subsequently have to introduce the hand into the uterus to search for a foot, for, the shoulder being free, it will be a matter of indifference which foot we bring down.

Braxton Hicks,¹ in his first article on combined external and internal version, says: "The motor force is employed almost entirely from the outside, and . . . the fingers within are used more as an indicator than as a propeller." Manifestly, then, the motor force is to a certain extent wasted—the child's pelvic pole is first made to descend, not as a part of any motion of the child *as a whole*, but at the expense of more or less bending of its spinal column; if now, the force continuing to act, the head rises, well and good; but if it do not rise, it is only driven against the ilium and rendered still less obedient to any effort on the part of the internal hand. In this state of things we may, as Hicks suggests in his longer article, supplement the force at work by pushing on the elbow, and in ordinary cases doubtless we shall usually succeed; but it seems to me that, by so directing the force as to raise the cephalic pole *at the outset*, we shall be able to turn without introducing the hand into the uterus in some cases in which we should otherwise fail, thus widening the scope of the exceedingly valuable operation for which we are indebted to Hicks.

Schröder,² in a case of combined external and internal version, did indeed act primarily upon the cephalic pole, making the external part of the operation consist in pushing up the head; but, as the membranes were entire, it is not likely that any use was made of the arm. In one of Elliot's cases (No. 93) a second twin presented a foot, a hand, and the

¹ *The Lancet*, Feb. 9, 1861.

² Schwangerschaft, Geburt, und Wochenbett. Bonn, 1867, p. 148.

funis. He "readily delivered a good-sized, still-born male child by pushing up the arm and drawing on the foot." It is not stated that the cephalic pole was pushed up by means of the arm. "When the shoulder presents," says Smellie, "and the arm lies double in the vagina, let him push them both up." It is evident from the context that Smellie intends this to be done, not to initiate a movement of revolution in the child, for indeed a head-first rotation would be contrary to the genius of the old operation, but simply to enable the hand to be introduced within the uterus.

Although, then, I have been unable to find in literature any account of version having been effected by moving the child through the medium of the arm, I am inclined to think it must occasionally have been done by accident during attempts at replacement.

Possibly, in cases of arrest of the shoulder over the symphysis pubis, with prolapse of the upper arm, such as I have already referred to as having been mentioned by Fritsch and supposed by him to be connected with a pendulous condition of the abdomen, it might be found serviceable to bring retropulsion to bear upon the prolapsed arm, thus maintaining the dorso-anterior position until the dislodgment of the shoulder could be effected and perhaps the other arm brought down, thus converting the case into a frank shoulder presentation of the most favorable kind.

Traction.—From time immemorial midwives seem to have been possessed with a blind impulse to pull upon any part of the child's body upon which they could lay their hands; hence a case of protracted transverse presentation, in which traction was not applied to the prolapsed arm, is something of a rarity in literature. Much damage has doubtless been done in that way, both to the mother and to the child, but, as we have seen, nature is often able to effect a favorable change in the child's position in spite of such rude interference. Nevertheless, traction upon the arm is a recognized expedient, and, under certain circumstances and when properly managed, very serviceable.

As a matter of course, traction properly so called, *i.e.* of such a sort as to produce any effect upon the child's body, should not be thought of until the diagnosis has been made perfectly clear. The mere drawing down of the arm for the

purpose of examination, or simply holding it somewhat tense, to serve as a guide to the examining finger, is an entirely different matter.

Barnes' observations, already quoted, would seem to show, that traction has sometimes aided in bringing about cephalic version, but they are not, in my opinion, sufficient to justify an attempt to turn by the head by means of traction in the ordinary sense of the term; nor do I suppose that Barnes meant to give that impression. It might serve the purpose better, if we were to first replace the arm within the uterus, and then make traction upon it in a lateral direction—away from the head, at the same time pressing upon the latter with the other hand applied externally.

We have seen also, that traction does not necessarily prevent spontaneous version by the pelvic pole, but it must have a tendency to do so under ordinary circumstances, although perhaps in the case of an immature child it might favor it.

Bandelocque's brother easily completed the spontaneous expulsion of a small child (a second twin) by traction on the arm. Schröder teaches, that in case the shoulder is so wedged in the pelvis that version is become impossible, we may, provided the pelvis is capacious, at least in its transverse diameter, the child small, and the mother's condition not such as to call for the speedy termination of labor, await spontaneous evolution, or endeavor to make the child engage by traction on the arm or the pelvic extremity. Under such circumstances, "we may," says Pinard, "as recommended by Velpeau, draw upon the arm after the manner of Fabricius Hildanus or of Fichet de Fléchy, a method which has been several times employed by P. Dubois." On the other hand, Hohl considers the practice in the highest degree objectionable.

Scanzoni, in advocating the application of a fillet to the prolapsed arm, to prevent its rising by the side of the head during extraction, says, "at the same time we have in the arm thus secured with a noose a hold by means of which we may assist those rotations of the trunk upon its long axis which are often necessary." It is to be supposed that traction cannot produce any such result unless the shoulder from which the arm springs is more or less above the superior strait. Mention has already been made of such a proceeding in cases

of prolapse of the upper arm. It is not easy to understand how traction upon an arm attached to a shoulder already deeply engaged in the pelvis can cause the child to turn on its long axis, and yet, in a case recorded by Depaul,¹ in which disarticulation at the shoulder-joint was performed, it is stated that the tractions made upon the arm caused the child to revolve in such a manner that the other arm could be seized. In cases of moderate impaction a manipulation of this sort may render the child movable, and thus allow of version.

Traction upon the arm for the purpose of rendering the neck more accessible, and thus facilitating decapitation, is so universally approved, that it need not be discussed. The same may be said in regard to traction upon the arm after decapitation, for the purpose of extracting the trunk.

Posta² gives the following directions for managing the arm in such a manner as to facilitate evisceration of the chest: a noose is to be attached to the prolapsed hand, and confided to an assistant. If it be the right hand, the assistant should raise the child's arm almost perpendicularly against the mother's pubes, inclining it a little towards her right groin, so as to draw the axilla as far down as possible. Schröder advises, that if, after evisceration, the shoulder is fixed in the pelvis, spontaneous evolution should be imitated, by drawing the shoulder well down and towards the side opposite to that in which the breech is found, extracting by the pelvic extremity, which we cause to pass in advance of the chest. Veit³ lays stress upon the value of traction upon the arm after evisceration, in his method of delivery *conduplicato corpore*.

In a case of spondylotomy, reported by Affleck and Macdonald,⁴ the operation seems to have been facilitated by traction on the arm.

Application of a Fillet.—As a matter of course, the arm may be noosed to facilitate traction for any of the purposes

¹ *Archives de Tocologie*, III., Fév., 1876, p. 111.

² *Il Filatre Sebazio*, March and April, 1857, quoted by Pinard, op. cit., p. 84.

³ *Monatsschrift für Geburtskunde*, 1861, p. 457, quoted by Pinard, op. cit., p. 85.

⁴ *Edinburgh Medical Journal*, July, 1872, p. 39.

which have already been considered, but the fillet is thought essential by so many authors, for the special purpose of enabling us to make traction after podalic version has been performed and the arm has retreated within the uterus, with a view to prevent the arm from rising by the side of the head during the process of extraction, thus imperilling the child's life at the critical moment when the placenta has nearly or quite ceased its function as a respiratory organ, and yet there is no opportunity for air to enter the child's lungs—that it demands separate consideration.

Those authors who mention the matter at all (including Osiander, Rosshirt, Hohl, Scanzoni, Hatin, Hodge,¹ Schröder, Tarnier, Fritsch, Pinard and Saboia), almost without exception, urge its importance with more or less enthusiasm. Credé advises that the application of the fillet should be followed by replacement. Scanzoni advocates its use if only the elbow be prolapsed. Depaul would have it applied to a hand brought down by mistake for a foot. Levret² says that the arm should be gently drawn out from the vulva, in order that the noose may be applied easily and without pain to the mother.

Osiander and Saboia even recommend that the arm should be noosed within the uterus, the latter employing a *porte-luc*s for the purpose. Fritsch, however, while bemoaning the frequent neglect of the fillet, advises against its application within the uterus, particularly by inexperienced practitioners, and Chiari, Braun, and Späth,³ who (in a series of 21 cases of arm presentation and another of 45 cases of "secondary" transverse presentation [the membranes having ruptured and the liquor amnii escaped]) did not noose the arm in a single instance, consider Trefurt's *porte-luc*s as dangerous and difficult to use, and all such instruments as superfluous.

Credé advises that both arms should be noosed, in case they can both be readily reached before turning; while, curiously enough, Osiander, who is said to have noosed every arm he could feel, expressly declares that both arms should never be slung.

The fillet is easily applied, and, if the hand can readily be

¹ Cours complet d'Accouchement. Paris, 1832, p. 180.

² Quoted by Scanzoni, op. cit.

³ Quoted in Scanzoni's *Beiträge*, i. 1854, pp. 309 and 319.

brought outside the vulva, can do no harm, except, perhaps, by introducing some deleterious substance within the uterus. It should be applied to the wrist. Its use would oftenest be of service in the hands of those who are in the habit of performing the old operation of podalic version, following it with extraction. As extraction may become necessary in any case of that sort, doubtless the more frequent use of the fillet would be of advantage. It can rarely if ever be required in a case of combined external and internal version.

Scarification.—Hohl scarified the swollen arm in one instance, on the recommendation of Plenck and Scanzoni, for the purpose of so reducing its bulk as to allow of the introduction of the hand, but found it useless.

Brachiotomy.—This term is applied to the removal of a portion or the whole of the arm, with or without the clavicle and scapula. Nearly all systematic writers, even those who admit the occasional necessity of the operation, speak of it with a feeling of horror—and yet it is frequently done.

Denman, the younger Ramsbotham,¹ Tarnier, Clay, Fritsch, and, according to Hohl, von Siebold, condemn the operation in unqualified terms. Scanzoni opposes it in his *Lehrbuch*, but seems afterwards to have met with a case which Hohl considers as supporting the views of those who advise removal of the arm under some circumstances.² Among those who speak less positively, but who still oppose the operation, except in the very rarest cases, are Rosshirt, Credé, Pinard, Schröder, and many others.

All admit, that it is unjustifiable if the child be living, and that it should be done only in case of necessity. In several instances it has been done in the belief that the child was dead, and yet it has been born living. De La Motte gives a case in which, after both this operation and craniotomy, the child was born alive.

Among the specific objections to amputation of the arm, in addition to the repugnance which it excites, are the facts that it

¹ The Principles and Practice of Obstetric Medicine and Surgery, Keating's Am. ed., Philadelphia, 1861, p. 363.

² Geburtshülflichen Operationen, quoted by Hohl, op. cit.

deprives us of a valuable means of traction, and that it tends to produce a confusion of the foetal and the maternal parts.

According to Hohl, the younger Nägele regarded the removal of the arm as sometimes necessary, and Oehler and Michaelis were decidedly in favor of the proceeding. Hohl himself, in speaking of the obstacle which the swollen arm may offer to the introduction of the hand, uses the following forcible language: "We have no dynamic measure against this state of things; expectant treatment adds more and more to the difficulties of subsequent operative procedures, and is trifling with the mother's life, while the material advantage and necessity of brachiotomy are before us. Empty phrases, to the effect that such a state of things cannot occur, lose their force in the presence of a case."

Pinard considers that brachiotomy is admissible only when the arm is the sole accessible part of the child, and there is urgent need of delivery. Hodge advocates the removal of the arm, together with the clavicle and scapula, to favor version by the pelvic pole, by facilitating the ascent of the head and thorax, especially in cases of deformity of the pelvis or great rigidity of the os uteri; in cases of cephalic version, to prevent the simultaneous engagement of the head and arm; in cases of evolution; and, as recommended by Robert Lee,¹ as a preliminary to evisceration.

The various views which have been held in regard to the propriety of brachiotomy for the purpose of facilitating the introduction of the hand rest upon the discrepant opinions which have prevailed as to whether or not the arm itself ever constitutes a serious impediment. This question has already been discussed, and no further reference will be made to it at present.

Pinard, who adduces the experience of Oehler, Depaul, Blot, Pajot, and Stoltz, in rendering version possible in difficult cases by means of brachiotomy, is of the opinion, that both this proceeding and evisceration, allowing of *version forcée*, should remain rare operations—operations of necessity. "Why," he says, "does this simple operation suffice in these cases? Is it because the vaginal canal is relieved of the prolapsed member? Assuredly not. It is because, the shoulder being cut, we thus obtain

¹ *Ed. Med. and Surg. Jour.* Apr., 1828, p. 239.

the result which Burton vainly sought to effect with his crutch. *The shoulder, or rather the axillary region, rises, reascends to the superior strait,* and the lower extremity of the child's trunk is then enabled to descend; whereas, if the scapular prominence be entire, the upper border of the shoulder becomes fixed at the lower edge or posterior face of the symphysis."

Pajot¹ concludes that amputation of the arm facilitates version, not only by favoring the introduction of the hand through a contracted pelvis, or into a constricted uterus, but also by enabling the head to rise and the pelvic pole to descend; but he expressly limits the application of this statement to cases in which the pelvis is so considerably contracted that the shoulder cannot engage.

Osiander (who, it will be remembered, saw five cases of spontaneous version after traction on the prolapsed arm) mentions amputation as among the resources left when all hope of spontaneous version is at an end. Pinard quotes cases in which amputation enabled Blot and Depaul to turn and extract by the feet in cases in which it was otherwise impracticable to do so. In Milroy's case of impaction with prolapse of both arms, in which two practitioners had failed in their attempts to turn, he gave chloroform, and then tried to turn, "but it was quite impossible to introduce the hand, and still impossible to put back the arms. There was no alternative now but to amputate one of the arms, which we did. After this was done, it was with the greatest difficulty I managed to turn." In De La Motte's case, in which he removed the arm, "*en deux coups de main,*" to enable him to introduce his hand, he finally succeeded in turning and extracting, but the amount of assistance which he derived from the removal of the arm may be inferred from the following words, with which he concludes his account of the case: "*Je crus très-certainement que je mourrois après cet accouchement, où j'épuisai et ma science et mes forces, et après lequel je restai sans respiration; en sorte qu'il me fallut mettre sur un matelas devant un grand feu, et me frotter avec des linges chauds pendant plus d'une heure, de même que si je fusse sorti de jouer à la paume.*"

¹ *Arch. Gén. de Méd.*, Sept. 1865, p. 257.

Pajot, in speaking of impaction in a contracted pelvis, says: "The child being mature, and version recognized as impossible, amputation of the arm will certainly favor the evolution of the fœtus." In a case of this sort Pajot and Danyau unsuccessfully attempted to turn by the feet, after which the arm was amputated, and Danyau undertook to decapitate, but could not finish the operation. He then succeeded in bringing down the pelvic extremity. Delivery was completed by perforation through the vault of the palate.

Boëns¹ removed the prolapsed arm, together with the scapula, in order to reach the neck, for the purpose of decapitation. In one case he removed both arms, but, failing in decapitation, eviscerated the thorax. Rosshirt states, that in cases of decapitation, "doubtless it often happens that the clavicle becomes separated from the sternum in the course of the operation, so that the arm, the shoulder-blade, and the clavicle, especially if the muscles chance to be ruptured, remain attached to the trunk by the skin only; but this is not intentional, and cannot be called a necessary removal of the arm."

Hohl thinks, that in cases requiring embryotomy it is sometimes necessary to violate the rule against disarticulating at the shoulder. Hodge, in speaking of cases in which evisceration becomes necessary, says: "The first step in the operation of embryotomy, in these cases, should be the complete removal of the arm and shoulder, including the whole scapula, and perhaps even the clavicle . . . there will be then no obstacle to the ascent of the side of the thorax behind the pubes." Again, he quotes Robert Lee as "always recommending that the shoulder be removed before evisceration and the crotchet be resorted to." Schröder says, that, in these cases, if the prolapsed arm were so swollen as to render the operation specially difficult, it would have to be removed, but that it should be preserved if possible, since its removal would deprive us of a valuable means of extraction. Oehler,² considers that the arm should not be removed for the purpose of facilitating evisceration, but only to make room for the introduction of the hand in turning.

¹ *Jour. de Méd. et de Chir. de Bruxelles*, t. xxxi. 1860, p. 259, quoted by Pinard.

² *Gemeinsame deutsche Zeitschr. f. Geburtsk.*, Bd. vii., 1832, p. 105; and *Neue Zeitschr. f. Geburtsk.*, Bd. iii., 1835, p. 161.

Pinard quotes a case of Pétrens', in which, after version had been attempted in vain, the arm and scapula were removed, and evisceration performed. Boëns also teaches that this should be done. Pinard mentions a case in which Dubois found brachiotomy necessary, as the first step in evisceration of the thorax. Doleris¹ reports a case in which Depaul removed the limb, and then eviscerated the thorax. Depaul², in a case of labor occurring apparently after the completion of the tenth month of gestation, with cancer of the cervix and lower portion of the body of the uterus, and physometra, the right shoulder presenting, and the arm protruding beyond the vulva, first disarticulated the prolapsed arm with large scissors, and withdrew it by means of a fillet. The tractions made upon the arm caused the child to revolve in such a manner that the other arm could be seized. This, in turn, was partly disarticulated, and then an attempt was made to sever the vertebral column. Failing to extract by these manipulations, the operator finally performed forced version by the feet.

Pinard cites a case in which Depaul amputated, first the prolapsed arm, and then the other one, and afterwards performed craniotomy and cephalotripsy. Hohl says, that if it should be thought necessary to remove both arms, as Dubois³ was obliged to do, on account of an unskilful operator having brought them down with a foot, we should remove that one first which offers the greatest impediment, and thus perhaps do away with the necessity of removing the other one.

As to the way in which the arm is to be removed, Mauriceau recommends that it be done by torsion. Smellie says: "If the limb be much mortified, it may be twisted off; otherwise, it may be snipt and separated with the scissors." Hohl says: "If the head lie to the mother's left side, the right hand is to be introduced into the vagina, and if it lie to her right side, the left hand is to be introduced. If the forearm should impede the entrance of the hand through the vulva, we may disarticulate at the elbow-joint. We seize the upper arm with

¹ *Arch. de Tocologie*, t. i. 1874, p. 632.

² *Arch. de Tocologie*, t. iii. 1876, p. 111.

³ Quoted by Hohl, who refers to an article by Dubois in the *Gaz. Méd. de Paris*, 1845, pp. 337, 346. That article, however, although by Dubois, is on the subject of hare-lip. I have not been able to find the original account of Dubois' case.

the thumb and the ring and little fingers, and draw it down as far as we can, keeping the index and middle fingers free for the purpose of encircling the shoulder-joint and guarding the points of the scissors. We slip the latter between our hand and the child's arm up to the axilla, enter their points, and cut through the soft parts of the joint, keeping up the traction on the upper arm, and not allowing the points of the scissors to come out through the skin. Having cut through the joint, we draw the disarticulated arm downwards, grasp the joint with the hand, and cut the remaining attachments with the scissors in the hollow of the hand."

I have thus taken pains to record, with some detail, the teachings and practice of various authors in regard to brachiotomy, in order to show clearly the contrast between the doctrine which is held by most of them, that the operation should rarely if ever be resorted to; and, on the other hand, the frequency with which, in clinical reports, particularly, as it seems to me, those of the French, we find it stated that the arm was removed, and that too, without any special reason being given for its having been done.

It is not certain that a prolapsed arm ever prevents cephalic version. If it did so in any case, I should think that replacement would be possible, unless there were other conditions present which would of themselves obstruct the version. I cannot see that the prolapse constitutes in itself any obstacle to podalic version, except by interfering with the introduction of the hand. Under such circumstances amputation may sometimes be necessary, but I should consider that the cases requiring it must be very rare. I do not understand how the removal of the arm can facilitate decapitation, evisceration, or spondylotomy, except by getting rid of an impediment to the introduction of the hand. As a matter of course, it should never be done until it is certain, either that the child is dead, or that some mode of delivery necessarily involving its sacrifice is demanded.

In conclusion, I should thus sum up the doctrines which I consider to be tenable in regard to prolapse of the arm in transverse presentations:

1. Mere prolapse of an arm does not of itself indicate, with certainty, that the presentation is transverse.

2. Inspection of the prolapsed hand will, in ordinary cases, roughly indicate the child's position, but should be supplemented by other means of diagnosis, and may, in presentations of the back or abdomen, or if the upper arm alone be prolapsed, give faulty indications, if conclusions be drawn in accordance with the rules commonly laid down.

3. Death of the child cannot be positively known from the condition of the arm, but muscular movements in the limb show that the child is still living.

4. Prolapse of the arm may cause the presentation to become transverse in cases which might otherwise end in a head presentation.

5. A prolapsed arm constitutes more or less of an impediment to cephalic version, spontaneous or artificial ; but yet

6. Traction on the prolapsed arm may, in certain cases, favor cephalic version, but is not to be recommended for that purpose, unless preceded by replacement.

7. The prolapsed arm does not always, even when pulled upon, materially hinder spontaneous version by the feet.

8. Prolapse of an arm does not facilitate the old operation of podalic version ; but

9. It offers the means of avoiding the necessity of liberation of the arms, or at least of one arm, during extraction, and thus, by hastening delivery, increases the chances of the child being born alive.

10. The arm may materially interfere with the introduction of the hand into the vagina and through the os uteri.

11. On the whole, prolapse of the arm is of advantage in transverse presentations.

12. The arm should be replaced within the uterus, by the "immediate" method of Credé, provided it can be done easily, in cases in which cephalic version is to be attempted, or in which there is reason to believe that a transverse position of the child is not yet thoroughly established.

13. Replacement is not called for under other circumstances, and should never be done by force.

14. Retropulsion, as described in this paper, may prove to be of service in combined external and internal podalic version.

15. By retropulsion, in cases in which the upper arm alone

is prolapsed, we may be able to prevent the child from assuming a dorso-posterior position.

16. Traction should not be made upon the arm until the diagnosis is established.

17. Lateral traction *in utero*, after the arm has been replaced, may assist in performing cephalic version.

18. Traction may be used to complete spontaneous evolution.

19. Traction, particularly if the shoulder be not deeply engaged, or if the force be applied to the upper arm, may cause the child to rotate to a certain extent on its long axis, and perhaps render it sufficiently movable to allow of version.

20. Traction is of manifest advantage in rendering the neck or trunk accessible, and thus facilitating decapitation, evisceration, or spondylotomy; and, after either of these operations, it materially assists in delivery.

21. A fillet should be applied to the wrist, provided it can be done easily, in case the old operation of podalic version is determined upon. This affords a means of preventing the arm from rising by the side of the head during extraction, by making traction on the arm after its retreat within the uterus.

22. The fillet should not be applied within the uterus.

23. Amputation should never be performed, unless the child is clearly dead, or embryotomy be necessary; and even then it should be avoided if possible, as it deprives us of a valuable means of traction, and may lead to confusion of the foetal and the maternal parts.

ON MASTURBATION AND "HYSTERIA" IN YOUNG CHILDREN.

BY

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(Concluded from p. 606, vol. viii., No. 4.)

NEUROSES of the joints have come under my observation a number of times in children of from five to twelve years, particularly in girls. I allude to the affection first described in Brodie's second edition, 1822, since by Stromeyer, in 1844, and by Esmarch. The majority of cases have been observed about the knee-joint, but the ankle-joint and the hip-joint are by no means free therefrom. The internal condyle of the os femoris, the styloid process of the ulna, and the vertebral column, are also pet places. The same affection, however, has been observed in the sciatic, obturator, crural, peroneal, saphenous and tibial nerves, in the cervical, lumbar, hypogastric and sacral plexuses, also in many peripheric nerve branches distributed about the integuments, joints, periosteum and bones. In both of the latter, according to Luschka, sensitive nerves are very numerous indeed ; thus they ought not to be lost sight of in the attempt at localizing the diagnosis.

A girl of eleven years, a patient of the German Dispensary some twelve years ago, applied to the surgical department for coxalgia. The pain was intense, the relative posture of the parts that of coxitis in the second stage, no change during day or night. The knocking of the knee against the hip-joint, said to be painless in "hysterical" coxalgia of the adult, was painful ; thus the diagnosis was uncertain for weeks. A striking feature in the case, however, was the fact that the child was not emaciated, and no fever could be detected. Anæsthesia was required to solve the problem. Under chloroform the spasmodic contraction relaxed, no crepitation was felt, mobility complete. A general roborant treatment, with an occasional sedative, and compulsory exercise, restored the patient gradually,

after months of patient treatment, and a year's useless suffering.

A little girl, nine years old, small and delicate for her age, but never subject to any form of serious disease, has for half a year suffered from intense pain, occasionally of the knee-joints, more frequently of the ankle-joints, but mostly of the tarsus and metatarsus. The attacks were mostly of short duration, and but rarely brought to my notice until lately, when I was requested to attend to her rheumatism. She had complained for days from severe pain about both ankle-joints, and the metatarsus. Conversation and play moderated her complaint considerably; the thermometer did not rise above the normal standard in the rectum, slight pressure resulted in expressions of severe pain, continued pressure appeared rather to relieve than to increase the sensitiveness. Again, the diagnosis of a neurosis appeared more indicated than that of a rheumatic affection; again compulsory exercise, roborant treatment, both general and local, were resorted to, with success.

A similar case was that of a girl of fourteen years, who after a long walk up and down hill, through ice and snow, was taken with severe pain about the left hip-joint, which soon incapacitated her for the use of the left lower extremity. Her case appeared particularly difficult. She had been a sufferer from early childhood. From her third year she suffered from osteitis, resulting finally in cicatrices of the malar and superior maxillary bones of the left side, disfiguring her orbit; of the right radius, and the left os femoris below the trochanter. She was eleven years before she was permitted to walk. Since that time she thrived, grew strong and hearty, and regularly menstruated before she completed her thirteenth year. In that respect she was never irregular afterwards. When the above symptoms of pain and immobility showed themselves, the diagnosis of coxitis, or pericoxitis, was made. Accordingly she was treated, principally, by rest, and her severe pain somewhat relieved, sometimes, by subcutaneous injections of morphia. In the course of time a variety of treatments were resorted to, every one without success. In this condition she had remained for a year and eight months, when I saw her. She was in the same bed in which she had been a year and a half before, was in constant pain, bore no touching about the hip-joint, or the

anterior aspect of the thigh, from the ramus horizontalis downwards to the extent of three inches, tolerated no moving, was hardly ever relieved for any length of time by subcutaneous injections of morphia, showed no swelling of the trochanteric region, but a slight but well-marked elevation of the central portion of the ramus horizontalis ossis pubis, which was also met with on pressing the finger down into the pelvic cavity, and a few inches down the femur, in the median line. Some explorative punctures, in different depths, revealed nothing. The diagnosis appeared positive enough, in my mind; an osteitis of very slow progress explained all the symptoms. The absence of fever could be explained by the very slowness of the whole process. Still, it appeared somewhat peculiar that the general condition of the patient, then sixteen years old, was far from unsatisfactory. Her general health, in fact, was good, and she was fleshy and rosy.

In connection with the case I may be permitted to state that the patient had been seen by a number of prominent medical men. She lived in a German University town. The professor of theory and practice, a gentleman of world-wide fame, agreed with me in the diagnosis of an osteitis. The professor of surgery, and a New York friend who left an enviable surgical reputation and practice in New York City to enjoy a scientific "otium cum dignitate" in Europe, pronounced in favor of a neurosis. Between the two opposite diagnoses the patient had the questionable benefit of doubt and wavering treatment. No change took place while she was under my observation; but I had scarcely returned home, when other symptoms developed, which turned the scale in favor of my surgical friends, such as tenesmus, irritation of the bladder, of irregular severity, duration, and alternation. Henceforth her recovery was a matter of time, endurance and patience. Internal remedies, external remedies, electricity, were exhausted. Compulsory exercise restored her. When she was to be removed from her place of sickness, she had to be carried on a stretcher for many miles to get to a railroad depot, reached her home hundreds of miles away, under excruciating pains, was compelled finally to sit, to stand, to walk, to take exercise; and improved gradually, but so slowly, that when, a year afterwards, she was sent to a foreign country for an entire change,

she still walked with pain, limped, and got exhausted for many a long, weary month.

A girl of about eight years,¹ was under my treatment for a neuralgia of the right ulnar nerve, without fever or spinal complication. After some time a moderate swelling of the subcutaneous tissue of the carpus, and in the neighborhood of the shoulder-joint, was added to her difficulties. They disappeared, only to be replaced by a very severe pain of the toes of her right foot. Her sufferings were intense for a long time; they appeared to be mitigated, when an oedematous swelling of her right foot made its appearance. A protracted tonic and galvanic treatment was required to restore her.

Another girl of five years was presented for acute rheumatism of the right shoulder-joint, which was said to have lasted several weeks, and to be very painful. There was excessive sensitiveness to the slightest touch, and some swelling. But it struck me that, since the commencement of the attack, neither the heart nor another joint were affected, that there was no fever, that deep pressure produced no more pain than superficial touch; that the pain extended as well over the thoracicus longus as the shoulder and upper arm; and finally, that the swelling was not exactly in the shoulder-joint, but above; nearer to, and to the rear of, the acromial end of the scapula. Thus my diagnosis was secured. I had to deal with a neuralgia of the cervical plexus, and not with "rheumatism."

In the same lecture from which I have just quoted, I alluded to the case of a boy of eight years, who had contracted a slight mitral incompetency while suffering from chorea, years previously. A year and a half ago he was attacked with rheumatism of both wrists, knees and ankle joints. A number of joints of the feet also took part in the process. There was moderate fever and distinct swelling of wrists, knees, and ankles. After some weeks his fever was gone, and the swelling very moderate indeed. Still, his complaints grew no less. He was taken with sudden attacks of excessive pain, which gave rise to screams and yells commencing about dark and continuing all night; was very sensitive, even in day-time, to the slightest

¹ A. Jacobi, "Acute Rheumatism in Infancy and Childhood," p. 23. A series of American Clinical Lectures, edited by E. C. Seguin, M.D., Vol. I., No. ii. p. 52.

touch, and exhibited such a disproportion between his objective and subjective symptoms that my suspicion was directed to other quarters than before. Then I recollected, that in periods of great mental anxiety, his father, many years ago, suffered from very severe and well-pronounced attacks of hysterical convulsions, and that his mother, a refined, intellectual, and neurotic woman, while subject to chronic oophoritis, had been disturbed by neuroses of both peripheric (mostly neuralgic) and cerebral character. My young patient had no more fever for some time, there was hardly any articular swelling left; he was quite comfortable at certain times, screamed fearfully—without tears—on the slightest touch on certain points, and became quiet, often, under protracted and deep pressure, particularly when his attention was diverted. The pain was not confined to the points most sensitive in sciatica, in fact there was no pain about the leg, joint, or the sciatic notch. A number of cutaneous branches of the crural nerves were affected, as also the ramifications spreading to the synovial membrane. At the same time, neither heart nor spinal cord participated in the process. If the original inflammation of the joint had anything to do with it, it had but been the source of irritation (in this case, perhaps, by a contiguous neuritic process?) in the sensitive nerves of both synovial membranes and skin. When, in accordance with the diagnosis of neurosis (neuralgia only, no vaso-motor complication being present) the treatment was changed to iron, galvanism, roborant diet, and warm bathing, the condition improved. But it took several months' absence from the city, and persistent exercise in a more genial climate, before recovery was complete.

The PNEUMOGASTRIC NERVE is frequently the seat of both irritative and paralytic neuroses in the adult. Of these, aphonia, so common in the "hysteria" of advanced years, I have never seen in the child. But the "hysterical" cough has often become a subject of observation. The first case I remember to have noticed sixteen years ago in dispensary practice. The patient was a boy of six years. A loud, hoarse, abrupt cough announced his entrance into the building; the absence of any change upon the mucous membrane of both the pharynx and larynx pointed to a neurotic origin of the affection. He never coughed in his sleep, but incessantly during the day, with but very short inter-

missions. Frequently his shoulders and the muscles of his face would participate in the convulsive process, with twitchings and contortions. He had suffered for two months, and two more months of permanent treatment with arsenic and atropia—the galvanic current forming no part in my therapeutical resources at that early time—were required before a gradual improvement took place. Some years ago I observed the same neurotic cough in two children of the same family, one a boy of six, the other one of four years. It was most developed in the former. First, I was inclined to accuse the hyperæmic condition of the pharynx as the cause of the cough, the more so as the tonsils were congenitally enlarged, and had repeatedly been the seat of catarrhal and erysipelatous inflammation. But a successful local treatment of these affections had but a partial effect upon the cough. Protracted attacks would follow each other, and were but little influenced by sedative and roborant treatment. Not before the child was detected as being given to excessive masturbation, and the habit finally suppressed, did the treatment directed against the neurosis prove effective. In the younger patient masturbation was not detected; his cough, however, was less pronounced and obstinate. Evidently nervous irritability and imitation had a great deal to do with the affection in his case. It has struck me as singular, in my experience, that the large majority of these pneumogastric neuroses took place in male children, while the same affection when found in adolescent or adult age, was mostly found in the female.

PARALYTIC and PARETIC conditions of simply neurotic character are not very frequent in the young. The large majority of cases require the searching for a local, mostly central, origin. Still, neurotic paralysis will occur. A girl of nine years, coming home from school some afternoon, fell in front of her residence with an attack of general clonic convulsions. She was carried up stairs, the convulsion subsided, and a local paralysis took its place. Nearly the whole of the right motor oculi was paralyzed, the musc. obliquus superior alone being intact. Ptosis was complete, the right eye deviated outwards, the pupil was somewhat dilated, but responded to the light slowly. On the left side the rectus internus muscle was intact, and the pupillar branch was a little affected. The tongue de-

viated to the right. In this condition the child remained for weeks, until she gradually improved. After having been considered well for some time, another attack of general convulsions took place, and was again followed by the same symptoms. A number of weeks afterward she was presented to me with the above symptoms, exhibiting in addition the expression of general anæmia, and complaining of occasional headaches. After convincing myself of the absence of a central cause, and excluding everything except a merely functional disturbance, I promised to relieve her at once in such a manner that she was firmly impressed with the certainty of success. I pressed my thumb firmly upon her supraorbital nerve, and commanded her to open her eye. Her ptosis was instantly removed. She remained well for several weeks, when again she exhibited the same symptoms (this time without previous convulsions), and was relieved in the same manner. Meanwhile, a general roborant treatment was resorted to, with the final result of a favorable report concerning her permanent condition.

Another form of paralysis, of evidently neurotic character, resembles very much the spinal paralysis of the young, the so-called essential or infantile paralysis. It is apt to appear quite suddenly, and although not differing much from the latter in its symptoms, is more promising in its termination. It appears to result from a change in circulation of the spinal cord only; and such cases as have been observed, appear to point to the anterior horns as the principal seat of the affection. For sensation is generally less affected than motion. Nor is the effect of the affection so great as in myelitis or meningo-myelitis. For neither rectum nor bladder are affected as in general degenerative processes of the nerve centre, or in affections of the posterior horns only. Such affections bear a great similarity to what is known to take place sometimes after dysentery, typhoid fever, variola, or other serious infectious diseases. Subsequent paralyses have been claimed to be "functional," because, frequently, a number of symptoms which would result from universal disease of the spinal cord, are absent, with what justice, is hardly our province here to go into. Still we can point to the fact that, with the same reason, essential paralysis might be claimed as functional. In fact, so we did, before

we knew better; the name of "dental paralysis" given to the "essential," speaks volumes for medical naïveté. A very few cases of paraplegia in the adult, depending upon blood-vessel dilatation only, have been reported in the journals in the course of the last few years. They all refer to adults. Two cases, which first drew my attention to this class of paralysis, also occurred in adults. One was a young man of twenty odd years, who came with a recently and suddenly developed paraplegia to the German Hospital. His upper extremities were normal, his lower extremities heavy, paretic. Walking was almost impossible, the patient not being able to raise his feet from the ground. Sensation was but little affected, the sphincters active. Masturbation was freely admitted. No previous disease accused. Temperature was slightly raised, pressure on the lumbar portions of vertebral column but little painful. In his case I excluded a deep lesion of the spinal cord, and thought I ought to trace the affection to a momentary change in the circulation of the spinal cord or the meninges, anteriorly. In regard to that, it would make no difference in the facts (but perhaps in the indications for treatment) whether the cause of the dilatation of blood-vessels is sought in a paralysis of sympathetic fibres; or whether, as Goltz has it, the dilatation takes place through the action of spinal, vessel-dilating nerves, which come from the spinal cord and can be called into action by reflex, from other centripetal fibres. According to the diagnosis, galvanism and ergotin were freely used. The effect was remarkable, inasmuch as improvement took place soon, and recovery within a few months.

A second case was in the same hospital for some time when the other was admitted. The patient had been paralyzed some months before he was admitted. He had been in the ward before his condition was recognized as similar to the other patient, who came in with the affection recently started. The same treatment was resorted to, with marked benefit, though slower result.

The following is a case of neurosis, with its centre about the lower portion of the spinal column, in which the symptoms of both paresis and increased irritability were well-marked and coexisting. The former predominated, however. I look upon it as a case of vaso-motor neurosis (paralysis) with blood-vessel

dilatation, the frequent changes being due to the very fact that the affection of the sympathetic nerve did not produce an anatomic lesion, but a change in the hæmostatic and nutritive conditions. In point of fact, neuroses of the vaso-motor nerves are frequent in the young. It is but just that it should be so; for the physiological and pathological action of the sympathetic nervous system ought to go hand in hand with their early development. In embryonic life, the cells of the sympathetic nervous system develop prior to those of the cerebro-spinal system. Those enclosed in the nerve centres are earlier than those in the outlying organs. Those in the spinal cord precede those in the cerebrum. Those in the anterior horns are earlier than those in the other parts of the cord. In other words, the centres for circulation (and motion) precede the development of other centres. Let us add to these statements the fact that the nervous system is heavier, more extensive in proportion to the weight of the whole body, and the physiological necessity of greater irritability and nervous vulnerability in the young requires no further illustration.

Mary L. M. was 10½ years old in February, 1874, when her mother wrote as follows: "She began complaining some three or four years ago of stomach-ache, which at first we treated with home remedies, thinking it of little importance. When the pain increased, so as to torture her constantly, Dr. M. treated her a long time, but without success; and, at his advice, Dr. C. used electricity (electro-magnetism), very successfully. For about six or eight months she was quite well. Then the old difficulty returned, in the form of nausea, from which she has suffered almost constantly for a year and a half at least. During this time various remedies were used; she was removed from school, her diet and bowels were carefully watched. During the summer months, (1872) she was always in the country; and, last winter, (1872-1873), she was also sent off for a change. Electricity was again used, then galvanism, but unsuccessfully. In the middle of August, (1873) while in the country, she was taken with typhoid fever. During the fever she had no nausea; but as she convalesced it returned. Still, after some time, say for two or three weeks, she was apparently well. She was out again in mild weather during December,

(1873) and January, (1874), taking exercise on a velocipede. Some over-fatigue caused its return, and some ten days ago she was, in the night, taken with a general convulsion. Since then she has kept her bed, being constantly nauseated and very weak."

In February, 1874, I saw her. Her "weakness" was almost complete paraplegia. In fact, she had not left her bed for a fortnight, and her condition was not appreciated at all. She could move her whole extremities with a sudden swing, by an effort of her trunk, but neither the muscles of the thighs nor legs responded to the will, if will she had. There was some slight movement, however, of the toes. Reflex motility was but inconsiderable. Sensibility was not always disturbed. Sometimes both extremities, sometimes one, were hyperæsthetic; sometimes only certain, not always the same, territories. Now and then anæsthesia set in, but never over a large surface. Such anæsthetic surfaces, sometimes the size of a hand, sometimes larger or smaller, felt cold to the touch, and were frequently pale. Not always, however, would coldness of the surface and anæsthesia correspond. Sometimes, side by side with the cold or pale surface there was a circumscribed red spot, of the size of some square inches, to that of the hand, or larger, thoroughly hyperæmic. Such hyperæmia could, besides, be easily produced by gentle friction. When so produced, it would remain six or eight minutes, and gradually disappear. Frequently the whole limbs were bathed in perspiration, alternating with dry coolness. The perspiring surfaces felt usually cold, not always with a change in the natural color. Pressure over the limbs resulted in but moderate pain. Pressure over the spinous processes was but little painful, somewhat more so about half an inch each side of the median line, about the lower dorsal and upper lumbar vertebræ. Even this symptom, however, was not constant. There was moderate constipation, never a difficulty in emptying her bladder, the urine pale, sometimes copious and limpid. Temperature always normal, with the exception of the times of excessive perspiration, when it would fall below the normal. Other morbid symptoms were but rare. There was an occasional slight cough. The internal viscera were all in a normal condition. The child looked pale but not emaciate, was cheerful though whimsical, and appeared to

enjoy her rest and the trouble she gave her attendants to a moderate degree.

Of diagnostic importance were the small number of tangible changes in proportion to the large number of symptoms, frequent alternations amongst the latter, the trifling and but occasional pain near the median vertebral line, the intact condition of the sphincter, and the absence of temperature elevations. The latter is particularly important. Changes in temperature are, in diseases of the nerve centres, mostly not in proportion to the dignity of the case. Destructive processes may run their full course without much fever. Therefore, the most careful observation is required to secure a differential diagnosis. And still, in many cases, the height of temperature, a difference of one or two degrees, is the only diagnostic sign between an "organic" (inflammatory, nutritive) and a functional or peripheric disease. Many a time have I secured a diagnosis by repeated measurements only, having no other guide. For obtaining correctness, the rectum alone, with its uniform temperature (at least in children, fœcal accumulations in the adult rectum permitting of possible mistakes) yields a positive result, which, as the difference to be found is probably but trifling in figure, though important in meaning, is urgently needed. For that reason I always measure the temperatures in the rectum, in questionable diseases of the nerve centres. Many a case of encephalitis or myelitis owed its diagnosis to the persistent elevation of perhaps one-half to one or two degrees.

Thus I fastened my diagnosis on a changed circulation rather than a nutritive alteration in the spinal cord, and took all of the symptoms for the result of a vaso-motor neurosis, on which partly temporary, partly obstinate dilatation of the blood-vessels were thought to be dependent. In consequence, the only treatment consisted, with trifling additions of occasional stimulants and nervina, in the administration of ergot, and the use of the galvanic, either ascending and descending spinal, or the peripheric current. Improvement began soon after the commencement of this course of treatment; it progressed steadily for months, until the child was out of bed and about. She was afterwards, without any further medical treatment, taken to Europe, and reports of a complete recovery were soon sent. She has been well since.

A boy of fourteen years was seen by me, two years ago, with Dr. Arcularius. His upper extremities were in a normal condition, his lower ones paretic, and moved with a swing by an effort of his trunk, but he could stand when supported, both with open and closed eyes. His skin felt dry, was slightly anæsthetic, no neuralgia anywhere. His urine was normal, contained no albumen. His sphincters were active. No pain on pressure over his vertebral column. No increase of temperature. The history yielded a report of many years of assiduous masturbation. The diagnosis was the same as above, and a similar treatment adopted. Of the result I am not informed, as I did not see the patient afterwards, and the attending physician has since died.

A case of GRAVES' (Basedow's) DISEASE has lately been published by Fr. Chvostek (*Oester. Jahrb. f. Pädiatrik*, vi. 1875, p. 51.) It occurred in a girl of twelve years. This affection is but rare in children. Beside his own, Chvostek collected only four additional cases, one of Trousseau in a boy of fourteen, Solbrig in one of eight, Rosenberg in a girl of seven, Dusch in a child of between the third and fourth year. Thus, I have every reason to feel very much satisfied at having seen three cases in children myself.

Mary S., Bolton, Lake George, came under my observation in the summer of 1874, during my residence in the neighborhood of that village. She was then ten years old, and had suffered from her symptoms "for years." She was of average size, well developed, pale, intellectual; a good scholar, a poor eater. Now and then her face or her feet would swell. The functions of her rectum and bladder were normal. She complained of great weakness, could not walk without an effort, the heart would beat violently, and dyspnœa set in on slight exertion. The dulness over her heart was too extensive by one half, the shock perceptible to both hand and eye from the third to the sixth intercostal spaces. A loud systolic murmur was audible all over the chest, covering the diastole. It was of equal strength over mitral and aortic regions, and extended into the carotids. The radial pulse was feebler than normal. There was no history of any disease except malaria; no rheumatism. No swelling of the neck, no affection of the eye was noticed at that one visit she paid me; at all events, it must

have been very slight, if it was present at all. Lungs were normal; nor could anything abnormal be discovered in her abdominal viscera. Under the circumstances my diagnosis was that of a general chronic endocarditis, and the prognosis a very grave one. For a long time, during the following autumn and winter, she took iron and digitaline, and followed such dietetic rules as I saw fit to give. She called upon me again the day I left that part of the country, in the early part of September, 1875. To my astonishment she was better to all appearances in her general health than the previous year. She could walk better, and her appetite had improved. The local symptoms were the same everywhere. The size of the heart was perhaps a little less; the murmur as loud and extensive as last year. Besides, I noticed at once a slight protrusion and immobility of her eyeballs, a staring look, and somewhat swelled lower eyelids, and a considerable goître. The diagnosis was changed into that of Graves' disease, and the prognosis corrected accordingly. Three months she took tinct. ferri and digitalis, regularly, and when in January, 1876, I received her news, they were favorable.

Dr. Moller, of West 37th street, introduced to me a little patient of nine years, Louisa W., whom he had attended for several months, about six months ago. When presented to me she was greatly improved already. Still she was anæmic, and of small stature; smart, and a good scholar. The contractions of her heart were both visible and palpable over three intercostal spaces, the pulsations of both carotids unusually distinct. The systolic murmur was strong, and audible over the whole chest, both anteriorly and posteriorly. Her eyes did *not* protrude. The thyroid gland was slightly swollen on the left side; very much so, and protruding, on the right. It was not the first time, however, that I noticed a unilateral swelling of the thyroid gland in Graves' disease. She had steadily improved under the use of iron and digitaline (gr. 1-10th daily) and quinia, and roborant diet, and thus the treatment was continued. When I saw her four months afterwards, her general appearance was about normal—still the child was small—her eyes did not protrude, her goître was not visible to the left of the median line, and less marked than formerly, on the right; her heart's action was less impetuous, and the

murmur less loud and less extensive. Of her complete recovery I entertained no doubt.

The third case is an exact counterpart of the one just mentioned. She was a patient of the clinic, and its records contain the few notes which were taken at her two visits. She was nine years, of average size, not remarkably anæmic. Heart loud and impetuous, systolic murmurs very strong and extensive, eyes not protruding, the right lobe of the thyroid considerably, the left but slightly swelled. Nothing was known about a previous disease, nor was any discoverable at the time of our examination. A similar treatment was resorted to, with what effect we had no means to ascertain since.

The neurotic origin of many SKIN DISEASES is established beyond doubt. Amongst them we count herpes zoster and urticaria. To the same class belongs pemphigus, in its rare acute form, with its hundreds of vesicles or bullæ filled with a thin alkaline fluid and surrounded by a red circle. I do not here allude to that form which is frequently seen upon the surface of the new-born; viz., a few bullæ spread over the whole body, and then of an innocent character; or on the palms of the hands or the soles of the feet, and then mostly of syphilitic origin. I allude only to that form, the existence of which is doubted by such an experienced observer as Hebra, who asserts never to have seen a case amongst a million of patients.

More than a year ago I was called to see a patient of Dr. Chabert's, in Hoboken. The little girl, four years old, was taken sick with a moderate fever, and exhibited a general redness of the whole surface the next day. On the third day the redness appeared in circumscribed territories, which were elevated, leaving between them portions of the skin of a more normal character. Although no symptoms belonging to the respiratory organs made their appearance, measles were suspected. On the fourth day a large number of the elevated territories exhibited a serous effusion raising the epidermis, the vesicles ranging between the sizes of a pin's head and that of a coffee bean. Accordingly, the diagnosis was changed into that of pemphigus. On the next day, the number of small vesicles was very limited, every one of them increased in size, and a number of them broke, either spontaneously, or in consequence of moving or scratching. On the evening of the sixth

day I saw the child. She then had a temperature (rectal) of 105°, a pulse of 160, and a proportionate number of respirations (44 to 46). She was on her back, moaning, delirious, wrapped in cotton, not an inch of the surface was normal. The epidermis was partly lost, in consequence of the effusions underneath, which were very copious, and the thrown-off integuments were washed or scratched off. In some places a number of bullæ were still intact, thus on the scalp; on others, square inches of surface were entirely denuded of epidermis; even hands and feet were raw and sore. Not before the following day did the sufferings of the child terminate with her death.

I could not but consider the sudden outbreaks of this peculiar form of dermatitis as the result of a neurosis. Was it a sudden attack, brought on by some poisonous influence, malaria, diphtheria, variola? There was neither a history of any of them in the child, nor in the immediate neighborhood. Was it but the final attack of a neurotic disposition in the patient, which had exhibited kin, but apparently unlike outbreaks before? I learned, on questioning, that the patient, now four years old, had suffered from extensive urticaria, which had tortured the little sufferer from birth. More than half the days of all her life she had been afflicted with this scourge. No dietetic care, no medical treatment had ever relieved or benefited her. Other sickness she never had, however. Urticaria, and frequent perspiration, such was the history of the infant and child, until she finally succumbed under the violent outbreak of the neurosis, which, while formerly confining its efforts to the production of local dermatitis of the lower strata, wound up with extensive superficial effusion. Where did it take its origin? There was a younger baby, two years old, in the family. He had "hives," but by no means so much, or so often, as the girl. The mother was free. The father was a sufferer from urticaria, almost constantly, for the last 1½ years, and found no relief. A younger brother of his had been affected with frequent and severe attacks of urticaria all his life.

A similar case, not so severe, because not fatal, has come to my notice, through the kindness of Dr. Assenheimer. The little girl is in her third year, had her first outbreak of acute pemphigus before she was a year old. The eruption was mostly

spread over back, chest and abdomen, also over the thighs. Frequently, large portions of the surface were denuded of its epidermis, the effusion being so copious, that the epidermis was washed off before scabs could form. When I saw her, the affection had lasted several months. Looking upon it as a vasomotor neurosis, with dilatation of the blood-vessels and consecutive effusion, I advised—beside general attendance to the health of the child—the constant administration of quinine and ergotin. This treatment was followed by a speedy success. Not only did the bullæ dry up rapidly, but their reappearance ceased, and the patient did well for some time. After the treatment had been discontinued, a return took place. A few weeks ago I have again seen the patient, covered with pemphigus, less violent and extensive, but still with its old character and local severity.

The following is a case in which the neurotic symptoms were less localized:

Alice K. (patient of Dr. Guleke's), thirteen years old, not menstruated, was a bright and vigorous child, ambitious and studious. On the fourth of January, 1876, after a good night, she was taken with nausea and delirium, and two hours afterwards with general and bilateral convulsions, and loss of consciousness. During the attack the pupils were equal, somewhat dilated, but little responding to light. The tongue was bitten. In short intervals the convulsions returned for three or four hours, when finally they ceased, after the free inhalation of chloroform and the administration of turpentine injections. Soon after the convulsions, the urine contained albumen in small quantities; it disappeared next day, never to return. Temperature 102. Pulse not very frequent, could be estimated only during the attack. On the evening of the same day consciousness was somewhat restored. Pulse and temperature as above. On the next day the temperature remained the same; pulse became slower, sank gradually to 60, and was once observed to be but 48. Some (3d to 5th) cervical vertebræ became painful, after a while, on pressure. The child grew irritable, insisted on getting out of bed, to school, to the piano. Application of ice to the head, and the administration of chloral hydrate with bromide of potassium brought some relief. Temperature in rectum sank to 100°, and remained abso-

lutely normal after a short time. Bromide of potassium was ordered, but irregularly taken. About the end of February vomiting set in, not preceded by nausea or retching. Sleep was not good. The child grew more and more pale and irritable, disobedient, unmanageable, obstinate, with frequent local flushes about the face. Such was the condition in the middle of March, when I saw her. The slight elevation of temperature in the beginning, the vomiting, and perhaps also the slow, (not irregular) pulse at a certain period of the affection gave us the impression that possibly the process was one of irritation of the brain, perhaps produced by the presence of a tumor. The administration of bromide and iodide of potassium was advised. Three weeks afterwards the report was tolerably favorable. No new brain symptoms had developed. Pulse about 80, regular. Respiration normal. No sighing. No more vomiting. Temperature normal, with a difference between morning and evening of three-fifths of a degree. The emotional disturbances the same, perhaps worse. The child irritable, peevish, disobedient, with occasional unexplained or unprovoked circumscribed flushes about face and forehead. The sphincters acted normally through the whole time. I allow that the case admits of doubts concerning the diagnosis. But the principal reason for the diagnosis of a functional disturbance is certainly present, viz., the absence of symptoms necessitating the diagnosis of an anatomical lesion. Besides, the functional anomalies attending a general neurosis are so manifold and various, that I believe the case is not very doubtful. The very slight increase in temperature, in the beginning of the whole process, may be the accidental result only of a complication; or it may, like the slight albuminuria, depend on the circulatory stagnation attending and following the convulsion. If I am not greatly mistaken, the case is one of bonâ fide "hysteria." Moreover, momentaneous increases in temperature may be found when circulation is disturbed. Local hyperæmia from a neurotic cause will raise the temperature. Even chronic nutritive diseases of originally the same character will admit changes of heat. Thus even in a few cases of unilateral hypertrophy (mostly described by Friedberg, Trelat, and Monod) an increase of from half a degree to two degrees has been observed from time to time.

After all, then, the neurotic impressibility known by the general term of "hysteria," which is still considered by many as an attribute of the adolescent or adult female, is not confined to either the adult or the female. It is true that, in regard to the male sex, the term of hypochondriasis is preferred to that of hysteria, in those cases in which we have to deal with certain general, either moral or emotional, disturbances; but the recognized forms of "hysteria," such as laughing and crying spells, and convulsions, under the ordinary circumstances believed to form or cause, and usher in, hysterical attacks, are by no means unheard of amongst men. I have above alluded to the case of a gentleman, about thirty years old, who was always of a rather nervous temperament. While under constant mental strain, brought on by a chronic ailment of his eyes, which he was told would necessitate the removal of one of his eyes, he suffered from very frequent and intense hysterical convulsions, which returned for years, until the operation was performed; and after the moral shock attending the prospect of the operation and the operation itself was overcome, his attacks became more and more rare. Another male patient of mine, fifty-four years old, has frequent attacks of "nervousness," trembling, weakness about the "stomach," globus, and dizziness, which are always followed by the frequent and copious elimination of a limpid urine, for hours. The disease is more frequent in females, it is true. In them it has long been attributed—as its name shows—to some affection of the sexual organs. But it is an acknowledged fact, that it does not result from their acute affections, which ought to be expected to work more serious changes than chronic ones. On the other hand, many a case of "hysteria" is certainly not complicated with a demonstrable disease of the genital organs. In thirty-four cases of hysteria reported by Bernutz, nineteen were free of such complications; and finally, such cases as have been reported by Grisolle, and Castanx, (*Gaz. hôp.* 1853, 1873,) prove that hysteria occurred with entire absence of both uterine and ovaries; not to speak of Charcot's case, in which a sclerosis of the lateral columns of the spinal cord formed the only etiological element.

Statistical reports prove further, that "hysteria" is found before either adolescence or adult age. Briquet states that twenty per cent. of his cases occurred in "children;" Amann collected

sixteen of from eight to fifteen years in a total of two hundred and sixty-eight; Althaus, seventy-one out of eight hundred and twenty below ten; Landouzy, forty-eight from the tenth to the fifteenth year, out of three hundred and fifty-one; Scanzoni, out of two hundred and seventeen, four below ten, and thirteen between ten and fifteen.

If the preceding statements prove anything, they demonstrate that the symptoms of "hysteria"—no matter whether they belong to motor or sensitive, sensorial or vaso-motor nerves—are found in all ages. This does not mean, however, that children will exhibit, as a rule, a number or all of the disturbances met with in the adult, or female, at the same time. In the majority of cases the neurosis is but local or circumscribed; but, when closely studied, of the well-known neurotic character. Now, if my readers should, after perusing these pages, come to the conclusion that the term of "hysteria" is an improper one, I shall be far from dissatisfied. It is but just that our nomenclature should gradually get rid of a number of terms which have but a clinical meaning of moderate convenience, and that we should substitute in their places anatomical and physiological diagnoses.

APPENDIX.

The following case of MASTURBATION IN AN INFANT WAS communicated to me by J. B. A., M.D.

April 1st, 1876.

DR. A. JACOBI.

MY DEAR DOCTOR:—I have delayed writing, that I might give you the result of treatment in the case of our little one, and also the history of the case, as promised. This I inclose, and hope it may prove of interest. The case is virtually ended, as she has not had a complete attack since we saw you, and adopted the measures suggested. Occasionally, while being held, she stiffens the limbs, but a separation of the thighs at once puts a stop to it all. She seems perfectly well, is eating and sleeping well, has gained in strength, and flesh, and color, and I do not see how she could be better. The constipation is entirely overcome by the change in diet. There is no evidence of any trouble with the kidneys or bladder. For a few weeks

we gave the cod-oil emulsion with the hypophosphites, but now she is not taking anything.

I am very truly yours,

J. B. A.

CASE.

During the first nine months the child was perfectly healthy, and did not suffer from any of the ailments common to infancy. At this age teething commenced. It was not accompanied by pain or any constitutional disturbance, and has not been during the cutting of the nine teeth she now has at fourteen months. The first indications of nervous trouble were noticed about this time. They were very slight and occurred while the child was lying in its mother's lap. She suddenly became pale, had a peculiar dazed expression, and her attention could not readily be attracted. On being raised up and moved, she immediately became natural in looks and action. This was repeated a few times only, when the attacks changed in character. In addition to the appearance of the countenance already described, there was much muscular rigidity; the arms became quite stiff and strongly resisted being flexed, and the hands were clenched and the little fists firmly pressed into the iliac region on either side. At the same time the legs were strongly extended at right angles to the body, and there was a strong contraction of the abdominal muscles, and a straining as if at stool. If the child was held against one's breast she made strong pressure with the knees, and up and down movements of the body. After a short period, a moment or two, the respirations were quickened to a rapid panting, and perspiration started freely from the head and stood in drops about the mouth. The attacks often terminated in sleep. There was at no time any spasmodic or convulsive movement, or unconsciousness, or mental disturbance beyond an apparent abstraction.

The attacks came on irregularly; at times with intervals of some days, and, again, they were repeated many times a day, for several days in succession, and sometimes for two or three hours with but slight intermission. *They never came on during sleep*, but usually when the child was sitting on the lap, and occasionally when on the bed or floor. If she was placed on the floor early in the attack, and amused with her play-

things, it would frequently be broken up; if, however, she was held till it was fully developed, and then put down, she would lie upon her side, and the attack would progress as described.

During the whole period she suffered much from constipation and from successive attacks of bronchitis, which reduced her flesh and strength. For some weeks the bowels were moved only by injections or medicines. During this period, the nervous attacks were more frequent and severe. The condition of the child attracted the attention of all who saw her while suffering thus; and, of course, each one had a theory concerning the nature and cause of the attacks. By some they were attributed to the state of the bowels, but by far the greater number to the presence of worms. To exclude the possibility of this theory, *santonin* was given, without, however, producing any result. Some physicians considered the attacks of serious import, and urged the use of the bromides, and, though none expressed the opinion that they were of an epileptiform character, the inference was plainly to be deduced.

On watching the child closely, the conviction was forced upon my own mind that the power of habit was a strong element in the case, and I entertained the belief that, by improving the general health, and correcting the tendency to constipation, recovery would ensue. To this end she was given cod-oil emulsion and laxatives.¹

It was at this time, and when the child was thirteen months old, that you saw her. Upon the adoption of your advice there was a wonderful change. The mother fully carried out your suggestions to separate the thighs, to remove the hands, and to amuse and attract the attention of the child. The habit was to be broken up at all hazards. Care and watchfulness have been crowned with complete success, and there is now scarcely a tendency toward the habit perceptible. A change of diet, in substituting for milk, a variety of food and of fruit, has corrected the condition of the bowels, and cod-oil emulsion, with the hypophosphites, aided by proper hygienic care, has improved the general health, and the child is now in good flesh and strength, well and happy.

¹ At the time of the infant's visit in New York, there was also a marked amount of mucus in the urine. If it continued, the administration of alkalies was recommended.

THE SURGICAL TREATMENT OF PRIMARY RETROFLEXION
OF THE UTERUS.

BY
WM. LENNECKER, M.D.,
Chicago.

(With six Woodcuts.)

PRIMARY retroflexions are found in the virginal state. During this period, the uterus, having no functional existence, lies small, undeveloped and passive, and gives rise to no subjective symptoms; but, when the organ becomes subject to the periodical hyperæmia of menstruation, the obstacles set by this malformation give rise to dysmenorrhœa, and the body of the uterus, being enlarged by the development incident to its entrance upon functional activity, and also abnormally by the congestion to which it is exposed, and the impediment offered to its circulation, becomes the source of other troubles, which cannot be overlooked.

When women, having a retroflected uterus, marry, their suffering becomes aggravated, and a new source of congestion is added to the menstrual flux; the organ fails to get the interval of rest it needs, a state of persistent hyperæmia is introduced, which borders on inflammation, and this leads to hypertrophy of the body of the uterus. Its functions are performed with increased difficulty, the dysmenorrhœa is more severe, menorrhagia, uterine hemorrhage, leucorrhœa, dyspareunia and sterility will be the result. It produces also distress by pressure on surrounding parts; the stools become flattened; the pain felt in defecation induces the sufferer to postpone evacuations, and thus will gradually cause an accumulation of feces. A habit of constipation and a loss of peristaltic power, and the imperfect performance of the functions of the smaller intestines and stomach, will cause nausea, flatulence and dyspepsia.

Imperfect indigestion, and consequently imperfect nutrition, will cause anæmia; the nervous system will exhibit the most

marked disturbance; hysteria breaks out in all its manifold eccentricities; sciatica, lumbago, tic-douloureux, rheumatism, headaches, and vertigo, frequently occur; despondency, melancholy, loss of command over feeling and thought are developed, and constantly exhaust, in a morbid direction, the nervous force, which is wanted for the performance of healthy functions. Many of these phenomena may be thus traced to bad nutrition; but there is good reason to believe that some, especially the nervous phenomena, are more directly induced, or at any rate aggravated, by the influence of the displaced uterus upon the nervous centres. The congested and displaced organ is a constant source of nervous irritation and exhaustion, and is constantly pressing upon the sacral plexus, and sending painful impressions to the nervous centres.

A not uncommon form of nervous disorder produced by retroflexion, is a severe, almost persistent, pain in the lower part of the spine, sometimes most intense in one particular spot, which increases under the least pressure. Many such cases have been treated for spinal disease; and the patients have been forced to wear spinal instruments for years, under the erroneous belief that the spinal suffering was primary, its mere symptomatic character not being suspected. With or without this marked pain, a sense of numbness, a want of power, especially an inability to walk, are often complained of, and tend to confirm the belief of spinal disease.

Endometritis and uterine catarrh are a frequent consequence of primary retroflexion. In the advanced state, the valvular closure of the fixed cervix, impeding the discharge of the menstrual fluid and ordinary uterine mucosities, leads to congestion, irritation and inflammation of the lining membrane of the body, as well as the cervix. The cavity enlarges under the distending influence of accumulation, the retained accumulation undergoes decomposition, resulting in irritating matter. The discharge occasionally becomes exceedingly offensive; causing redness of the vaginal canal and vulva, and in some cases vaginitis. The fundus of the uterus, being more sensitive than any of its other parts, may become very irritable, if the sound be passed per anum or per vesicam, and the point be turned upon the fundus. If passed into the uterus, there may be no pain until the point has passed the os internum and struck the

fundus, where, if pressed at all forcibly, absolute agony may result, and will probably produce vomiting, hysterical paroxysms, and sometimes a regular epileptic form of attack.

Vaginismus is another distressing result of primary retroflexion. The friction of the inflamed mucous surfaces, and the chafing of the inflamed and enlarged vaginal portion of the uterus, excites spasmodic contraction of the muscular coat, and especially of the vulvar sphincter, and is the immediate source of distressing pain.

Serious danger attends primary retroflexion, should conception take place. The unfavorable shape of the uterus, and its retention in the pelvis, oppose the due development of the organ, hence abortion frequently ensues; when this does not take place, there is a greater danger arising from the impaction of the enlarged uterus about the third or fourth month.

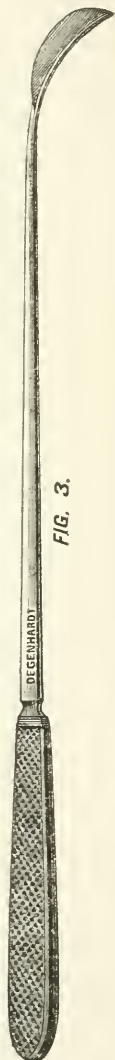
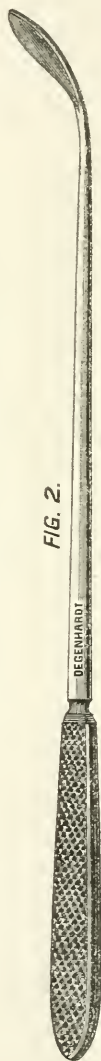
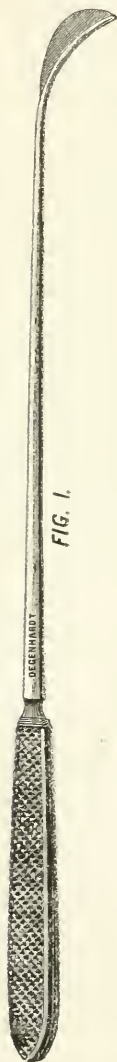
Uterine diseases, being surgical rather than medical, exact, for their successful treatment, more rigorous measures than are necessary in the diseases of most other organs. It is superfluous to say that palliative treatment will not suffice in uterine surgery, and that we cannot trust to, and have any success with, salines, sedatives, or tonics, when there is a dislocation of the womb needing rectification.

To straighten the axis of the uterus, and to restore the fundus to its normal position, are objects produced concurrently by the same means.

These means are mostly mechanical; when this is attained, the distress due to retention of blood clots and mucus will subside; by lifting up the fundus, the engorgement of the body of the uterus will diminish; and, as the bulk lessens, there will be a smaller degree of vicious leverage to counteract. The combined progress of rectification and of diminution brings immense relief to the organs which have been pressed upon, and it is almost needless to say that general amelioration soon follows; the local irritation being lessened, all nervous symptoms will subside; and, with renewed capacity for exercise, nutrition will greatly improve. The last indication is to treat the local complication of the retroflexion. It is true that the engorgement, endometritis, uterine catarrh, and other changes in the uterus, may subside under the means employed to rectify the malposi-

tion, but their cure may be accelerated, and be more permanent, if treated by local remedies.

Having decided that the patient is a fit subject for treatment

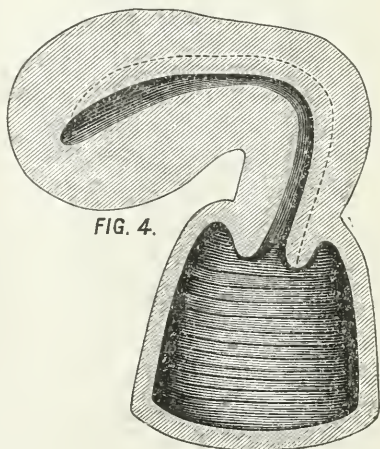


by ascertaining the cause and nature of the disease, the operation is performed as follows:—

The instruments necessary are, Simon's speculum and elevator (resembling Sims' duck-bill speculum, with a straight handle attached; the elevator, to hold up the anterior vaginal wall, is flat instead of concave); two uterine sounds, one lead and one rubber; one pair of long, straight sharp pointed scissors, one double tenaculum hook, one sponge-holder, one set of retroflexion knives (Figs. 1, 2, 3, Fig. 1 for cutting the anterior wall of the uterus, Fig. 2 for the right side, Fig. 3 for the left side).

The knives, with the exception of the blades, are made of flexible metal, so that they may be bent in any curve to suit the flexure of the uterus.

The patient is placed on a table, in the position employed by Simon, of Heidelberg, while operating for vesico-vaginal fistula (see figure on p. 201 of Thomas's "Diseases of Women," 4th edition) with the nates drawn well up to the edge of the table, and the thighs flexed. The operation not being painful, it is not necessary to make use of anæsthesia, unless the patient is very nervous. The speculum is then introduced and held by an assistant, the vaginal portion brought well into view, seized in the anterior lip by the double tenaculum, and drawn well downward. An exact survey of the curve of the uterus is taken by the lead or rubber sound. One blade of the scissors is passed into the cervix until the outside blade reaches the angle of reflection of the vagina, and the part intervening between the blades divided by a single stroke; the opposite side of the cervix is similarly treated. Having been bent to the exact curve of the uterus, as ascertained by the lead sound, the knife (Fig. 1) is taken, and the anterior wall of the uterus up to the fundus slit open. I now take the knife shown in Fig. 2, after being bent to the exact curve of the uterus, and slit open the right side, and repeat the operation, in the same manner, with Fig. 3 on the left side (see Fig. 4). The parts are now swabbed with a



sponge and ice water, until the bleeding is fairly stopped, which is generally the case in a short time. I now take a small silver wire uterine sound, well wrapped with a strip of cotton to the depth of three or four inches, thoroughly soaked in carbolic acid; this is rapidly introduced into the uterus to the fundus, and left there for a few moments to allow the carbolic acid to cauterize the wound thoroughly. After all parts have again been well washed, the patient is removed to bed, where she remains for ten days. After forty-eight hours, the patient is again placed in the same position as during the operation, the speculum introduced, and the uterus brought well forward. A two-bladed dilator (Palmer's, Miller's or Ellinger's) is introduced up to the fundus, and all parts well expanded, and then the sound, wrapped in cotton, and soaked in carbolic acid, is again used. This treatment is repeated every third day until the twelfth day, and then once a week for six weeks. Ten days after the operation, I introduce one of Noeggerath's pessaries (Fig. 5) the proper size having been carefully selected; this

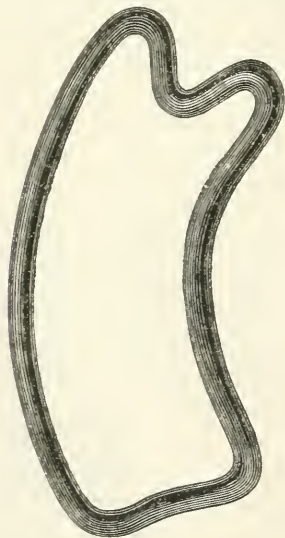


Fig. 5.

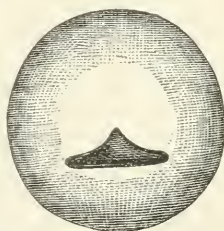


Fig. 6. Os three months after operation.

will elevate the uterus, and keep it in an erect position while the cicatrix is forming, which generally takes from eight to ten weeks, but if the cicatrix is not strong enough at the end of this time, the pessary may remain longer.

To illustrate the principles in the foregoing treatment, I give the history of a few cases.

Mrs. A., consulted me in May, 1874. She was twenty-four years old, and had been married three years. Previous to marriage she had suffered greatly during menstruation, and also from habitual constipation. After marriage these difficulties increased, and were combined with nervous headache, loss of appetite, leucorrhœa, and vaginismus. Before this time she had consulted many physicians, who had introduced different pessaries, none of which she had been able to wear longer than twelve hours. Owing to the lady's nervousness, it was impossible to perform an operation without administering chloroform. I found primary retroflexion of the uterus; the cervix enlarged and in a chronic inflammatory state, the os greatly contracted, not being larger than the head of a pin; examining through the rectum I found the body of the uterus very much increased in size. I at once slit open the uterus in the manner already stated, and wiped it out with pure carbolic acid, ordered a vaginal injection of an infusion of tobacco and hyoseyamus, (folior. nicot. 3 i, folior. hyoseyami ℥i, aquæ fervidæ, Oj,) three times daily. Three days later, the patient being placed under the influence of chloroform, the uterus was wiped out with a saturated solution of chloride of zinc; to allay the pain following this application, suppositories were ordered, each containing morphine and extract of belladonna, half a grain of each, every three hours, until the pain ceased. Six days later I again administered chloroform, and found the chronic induration of the cervix subsided, the uterus greatly diminished in size and secreting a watery discharge; the wounds were also healing rapidly.

Six days later, the vaginismus having entirely disappeared, after expanding the uterus and wiping it out with pure carbolic acid, I elevated it to its proper position, and introduced Noeggerath's pessary. The pessary was removed once a week for four weeks, the uterus dilated and wiped out with pure carbolic acid each time. Five months after the operation I removed the pessary, leaving the uterus in an erect position, which it still retains. All suffering has disappeared, and the lady is now in the enjoyment of perfect health.

Miss M., 20 years, had always menstruated with great difficulty, but of late years the pain had become so intense as to

| No. | Cases operated. | Size and position of Uterus before operation. | Other diseases of the Uterus complicated with the Flexion. | General health. | Remarks. |
|------|---|--|--|--|---|
| I. | Mrs. A—, age 24 years, married 3 years, operated in May, 1874. | Uterus very much enlarged, size four and one-half inches. Flexion at right angles. | Chronic inflammation of the cervix, endometritis, vaginismus and leucorrhœa. | Dysmenorrhœa, habitual constipation, nervous headache, loss of appetite, despondency, and inability to walk. | This lady had been suffering from dysmenorrhœa since menstruation began, which gradually increased after marriage. Endometritis. I found it necessary to mop out the uterus with a strong solution of chloride of zinc. The cutting of the cervix was also followed by the application of acids and zinc, which subdued the chronic inflammation, and, as a secondary result, the vaginismus. |
| II. | Miss M—, age 20 years, operated May, 1874. | One inch over normal size. Accompanied with the retroflexion; towards the left side. | Chronic endometritis, leucorrhœa, hypertrophy of os and cervix. | Dysmenorrhœa, neuralgia, lumbago, headache, terrible pain in left ovary and left thigh during menstruation, constipation, and melancholy, sometimes bordering on insanity. | The patient has completely regained her health. Endometritis was removed by the application of carbolic acid. |
| III. | Mrs. M—, age 24 years, married 4 years, operated June 16th, 1874. | Length of uterus one and three-fourth inches. Atrophy of the uterus. | Chronic endometritis, amenorrhœa. | Dysmenorrhœa, slight constipation. | After the uterus became erect, the amenorrhœa still continued; the menstruation, though painless, still remained scanty, but by the application of the constant galvanic current this was overcome. |
| IV. | Mrs. K—, age 38 years, married 15 years, operated Aug. 24, 1874. | Uterus much enlarged, though slightly flexed. | Contraction of os external, and chronic hyperplasia. | Hysteria, melancholy, hysterical dysmenorrhœa, dyspareunia, difficulty in urinating during menstruation, constipation. | This lady had been suffering from hysterical dysmenorrhœa for fifteen years. It was more severe immediately before menstruation, but would sometimes abate during that period and the week following. Since the operation this has entirely disappeared, and the lady is enjoying the best health. |
| V. | Mrs. S—, age 28 years, married 9 years, operated Aug. 28th, 1874. | Much enlarged; flexed so as to completely obstruct the rectum. | Endometritis, leucorrhœa. | Constipation, extreme nervousness, melancholy to such an extent that frequent attempts at suicide were made. | This lady had been suffering so severely from endometritis that she was confined to bed for a year. This was overcome by mopping out the uterus with a strong solution of chloride of zinc. |
| VI. | Mrs. F—, age 19 years, married 1 year, operated Oct. 20th, 1874. | Normal size; flexed at right angles. | Chronic inflammation of the cervix, leucorrhœa. | Dysmenorrhœa, dyspareunia. | This lady became pregnant two months after the operation. |

| | | | | | |
|-------|--|--|---|---|--|
| VII. | Mrs. F—, 25 years old, married 5 years, operated Dec. 28th, 1874. | Size normal; flexed to such an extent as to completely obstruct the rectum. | Leucorrhœa, slight chronic endometritis. | Constipation, dysmenorrhœa, dyspepsia, neuralgia, nervous system much depressed. | Health completely restored after operation. |
| VIII. | Mrs. S—, age 30 years, married 10 years, operated Feb. 4th, 1875. | Slightly flexed; much enlarged. | Chronic inflammation of the cervix, chronic endometritis, leucorrhœa. | Dysmenorrhœa, lumbrago, constipation, severe pains in small of the back, neuralgia, dyspepsia, pain in back of head. | Had been suffering greatly from endometritis, and I found it necessary to mop out the uterus twice with fuming nitric acid, which overcame the difficulty. |
| IX. | Miss R—, age 21 years, operated April 2d, 1875. | Normal, but badly flexed. | | Anæmia, dysmenorrhœa, hysteria. | Has since married and enjoys splendid health. |
| X. | Mrs. B—, age 22 years, married 2 years, operated May 3d, 1875. | Normal; flexed so as to obstruct the rectum. | Chronic inflammation of the cervix, chronic endometritis, leucorrhœa. | Dyspareunia, dysmenorrhœa, constipation. | Became pregnant three months after the operation. |
| XI. | Mrs. W—, age 22 years, married 6 months, operated June 18th, 1875. | Much enlarged; flexed at right angles. | Chronic endometritis. | Constant backache, severe lumbrago, dysmenorrhœa, and constipation. | Became pregnant four months after the operation. |
| XII. | Mrs. F—, age 22 years, married 3 years, operated July 3d, 1875. | Normal; flexed at right angles. | Leucorrhœa, chronic endometritis. | Dysmenorrhœa, constipation, hysteria, dyspepsia, and anæmia. | Completely restored to health. |
| XIII. | Mrs. G—, age 38 years, married 15 years, operated July 16th, 1875. | Much enlarged; besides the retroflexion, was lateral flexion towards the left. | Chronic endometritis, leucorrhœa. | Dysmenorrhœa, constipation, lumbrago, neuralgia, pains in the left leg, severe backache, nervous headache, and dyspepsia. | Besides the retroflexion, the uterus was inclined very much to the left, thereby causing very severe pains in the left leg during menstruation. To overcome the endometritis I was obliged to mop out the uterus with fuming nitric acid. After the operation the lady's health was completely restored. |

prostrate her for weeks, and cause her to dread extremely her regular monthly suffering. Her nervous system was very much disordered, and she was also suffering from neuralgia, back-ache, lumbago, melancholy bordering at times on insanity, constipation and leucorrhoea.

The lady had taken many different medicines, but would not consent to an operation, until her health became so much impaired as to render active interference imperative. On the 26th of May, 1874, I performed the operation, as before described ; May 30th I stretched the os and cervix thoroughly, and mopped out the uterus with carbolic acid ; June 8th I repeated the operation and introduced a pessary. Congestion of the uterus having entirely disappeared, I stretched the cervix once more on June 20th. Eleven weeks after the operation I removed the pessary, leaving the uterus in an erect position, all other symptoms before mentioned having entirely disappeared.

Including these two cases, I have now operated on 13 patients in this manner, (see Table, pp. 246, 247) in all of which a complete and permanent relief from the symptoms occasioned by the displacement was obtained. In three cases pregnancy speedily followed the operation.

REMARKS UPON THE NATURE OF THE DIPHTHERITIC POISON, AND ITS TREATMENT BY SO-CALLED DISINFECTANTS.

BY

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THE belief which prevails with respect to the true nature of diphtheria will naturally influence to a great degree the treatment employed. Bretonneau and his condisciples, we all know, put faith in the decided inflammatory and specific character of this disease. At the commencement of his brilliant career, Trousseau adopted and proclaimed his great master's teachings.

Hence their use of antiphlogistic and caustic remedies. In our day, while the specific infectious type of diphtheria is still described by almost all writers, the acute inflammatory condition is brought to our attention less prominently. The affection is spoken of as being one in which the tendency to general and local asthenia is ever present in a more or less marked degree. And, for this reason, tonics and stimulants are given internally at an early stage of the morbid invasion; and different detergent and astringent agents are applied to the mucous membranes in which the local signs of disease are apparent. With respect to the specific character of the disease there is little variance of opinion. The obscure questions—By what is it caused?—In what does its contagious, infectious nature consist? still remain unanswered. Amongst those who admit the parasitic nature of the diphtheritic poison, there is a difference of conviction in regard to the way in which monads penetrate the economy. According to some authorities, they are taken up immediately by the circulatory system, and occasion the outbreak of the disease, under the form of constitutional phenomena which become the result of blood changes, possibly of a chemical sort. By others, it is believed that general manifestations of grave import only follow after a shorter or longer period, the primary local signs which are evident upon the mucous membrane of the fauces and tonsils.

These distinct theories of the processes by which the blood and viscera of the body become infected by the diphtheritic poison are important, because they also lead to variances of treatment. Medical men who believe in the primary local nature of the disease, and who acknowledge, also, the direct agency of lower forms of organized life in occasioning it, attribute the greatest importance to the prompt action of antiseptic drugs applied topically in weaker or stronger combinations. Those who admit the existence of blood poisoning by similar organisms from the very beginning, and who understand the throat manifestations to be but a consequence of the blood changes already produced, and the bacteria present upon the false membranes and mucous linings to be only a concomitant fact of very secondary value, are extremely anxious to start remedial measures with frequent and relatively large doses of disinfectants given internally. And, in their judgment, these latter,

when taken up by the absorbents of the economy, will more than prove a match, by antagonistic properties, for the specific noxa which is manifesting its deleterious effects.

Predicating, too, the nature and mode of development of a redoubtable affection from their own stand-point, they are but little disposed to attribute importance to local applications. Estimated by the results, which therapeutic creed is the best? It is difficult to answer; for, with one method of treatment, as with the other, we have many deaths to deplore; and diphtheria still prevails to an alarming extent. The question naturally presents itself, therefore, is diphtheria in reality of a parasitic nature? and if it were proved *not* to be so, is it rational to continue the use, either externally or internally, of the so-called disinfectant remedies?

The contagious character of diphtheria is to-day accepted by all. Numerous well-known facts incontestably prove it; viz: the deaths of Valleix, of Blache, of Gillette—the cases referred to by Guersant, Trousseau, Roger, Lorain and Lepine, &c.

The inoculability of the disease is not so universally admitted. Bretonneau, it is true, fully believed in the inoculable power of diphtheria, and many facts narrated in his famous work on this disease bear evidence to the justice of his convictions. Still Bretonneau never performed any experiments with the local products of diphtheria, so as to carry conviction to the mind of septsics. Amongst later French authors, several deny absolutely that the diphtheritic poison is capable of transmission after this manner. Michel Peter reports in his inaugural thesis, published in Paris in 1859, three attempts that he made upon himself, to test whether or not the diphtheritic poison was inoculable. They all proved unsuccessful, as did one auto-inoculation made by Trousseau.¹

Inoculations of false membranes taken from patients suffering from diphtheria, have been made by Raynal,² not from man to man, but from man to the chicken, and the results were wholly negative.

Lorain et Lepine³ consider that Peter⁴ advanced too much,

¹ Dict. de Méd. en 30, vol. x. p. 393.

² Dict. des sc. Vétérinaires, par Boulay & Raynal, vol. i. p. 605.

³ Dict de Méd. et de Chir. Prat. vol. xi. p. 600.

⁴ Op. cit.

when supported by the negative inoculations made upon himself, and by similar facts described by Trousseau, he denies the possibility of their success. "Facts, according to them where inoculation has been negative, prove simply that it is not necessary;" and they summarize what they wish to say with the statement that negative facts do not destroy the value of positive ones of direct contagion, of which several have been reported by reliable authors.

These prudent and sensible remarks appear to be completely ignored in the article on diphtheritic sore throat, in the *Encyclopedic Dictionary*,¹ where we still find the inoculability of this specific disease flatly denied.

Very different results have been reached in Germany. Trendelenberg² speaks of 68 inoculations made by himself upon birds and rabbits with fragments of false membranes taken from children attacked with diphtheria. The implantations were made upon the inner surface of the trachea. The experiment was followed by success on eleven occasions. And further, the products of the first inoculations were found also to be capable of transmitting the disease a second time.

Certel, when "privat-docent" at the University of Munich, first made known the results of his experimental researches in this same direction. His inoculations were usually successful, and the results reported by him,³ are such as to convince the reader that like ones may be readily achieved when the inoculations are properly conducted. Certel considers the almost indefinite transmissible power of diphtheria as proved by repeated inoculations, from one previously infected animal to another, through many degrees removed from the first, to be complete proof of its noxa being due to an organism of parasitic nature.

His investigations have been corroborated by those since made by Nassiloff, of St. Petersburg. The authors first cited found that, although the false membranes taken from diphtheritic patients were easily transmissible, such was not the case with the false membranes of true membranous croup. And it is affirmed by them, as it has been before and afterwards by

¹ Dict. Encycl. des Sc. Méd. vol. v. p. 21.

² Archiv f. Clinische Chirurg., 1869.

³ Archiv für Klinische Medicin, Mai 1871, p. 242.

others, that the lesions of this last-mentioned disease may be artificially produced by pouring a few drops of liquid ammonia, or other local irritant, into the windpipe.

With respect, then, to the inoculability of diphtheria, due regard being had to the preceding quotations, we believe there can be little doubt in any candid mind that diphtheria is really an inoculable disease ; and nearly, if not quite as certain to be transmitted in this manner as variola or syphilis. And, consequently, it is true that when there is immediate contact between the product of diphtheritic inflammation and a softened mucous membrane or denuded skin, contagion will almost inevitably take place. It is essential, therefore, for practitioners of medicine to be forewarned of this fact, in order to shield themselves from danger. And we would here, in parenthesis, insist upon two practical bearings ; viz., that in tracheotomy of diphtheritic patients special attention should be paid to the condition of the cutis covering the fingers and hands, for if there be even a slight abrasion of these parts, diphtheritic toxæmia may follow an inoculation, as it were, from immediate contact.

Again, when incision is made into the trachea, or the canula is being introduced, care should be taken, so far as possible, to avoid placing one's face too close to, or immediately in front of, the neck of the patient, for during the first effort of expiration which follows the opening made into the windpipe, and also insertion of the tube, contagious products are frequently thrown to some distance, and may alight equally upon the lips, conjunctivæ, or even enter the buccal cavity of the operator, and thus cause direct transmission of the disease. True it is, many operators have sucked out the liquids contained in the trachea of a child ; and have not had reason, fortunately enough, to regret their temerity. In like manner, a few bold scientific observers have had the fool-hardy courage to make attempts to inoculate themselves with false membranes, and their attempts have happily failed.

As we have previously shown, however, such negative facts are by no means conclusive, and do not take away from the importance of many positive ones, some of which have already been quoted. Now, although the proofs of the inoculability of diphtheria may be admitted, differences of opinion may still

prevail in regard to the pathological characters of the false membranes.

While some authors unreservedly accept the existence of a vegetable organism in the structure of diphtheritic false membranes, and bring proof to support that it is this mycrozyma of special nature which is the necessary or essential factor in the propagation of the disease, others remain in doubt, or even deny absolutely the existence of any such monad.

Does this organism exist?

Has it any special appearance or properties?

Tigri was the first who attempted to describe the special characters of the bacterium of diphtheria. This he did in a memoir, presented to the French Academy of Medicine in 1867, where he calls it a "branching parasite," and says that he has found it to enter into the structure of the false membranes, and considers it to be the direct cause of diphtheritic infection.

Later, Letzerich¹ has studied the different appearances, changes and developments of the cryptogamous organisms in diphtheria. The most perfect of these he designates by the name of *Tilletia Diphtherica*.

For him there are two forms of local diphtheria. The active principle of one, or diphtheritic fungus, may be obtained from the filter through which the urine of a child suffering from the *infectious* disease has been passed.

If portions of this fungus be placed on the mucous membrane of a rabbit, a local diphtheria of the first kind is produced. The phenomena of general disease only make their appearance afterwards, ordinarily in about two or three days from the period of inoculation. According to the investigations of Tommasi and Hueter,² granulations of undetermined nature were found in the blood of several patients attacked with diphtheria. These granulations were introduced by inoculation into the cellular tissue of rabbits. Everyone of the rabbits died, but of disease which was of septic nature, without being recognisable as diphtheritic. The blood, however, of the rabbits, when examined after death contained granulations similar to those found in patients suffering from diphtheria. They

¹ Virchow's Archiv, 1869, and *Berliner Klin. Wochens.*, 1874.

² Centralblatt f. die. Med. Wissench, 1868.

remain doubtful, therefore, as to the specific nature of the noxious granulation which they describe.

In Certe's last and most famous article on Diphtheria, in *Ziemssen's Cyclopædia*, we find the following statement, which allows of no uncertainty in regard to this learned author's belief of the rôle played by bacteria in this infectious disease: "There can no longer be a doubt," he writes, "that these vegetable organisms are not of accidental occurrence, but are inseparable from the diphtheritic process, just as the bacteria of decomposition are necessarily connected with decay, and act as a ferment of it." With respect to the first part of this assertion, we would remark that if micrococcus is inseparable from diphtheria, so also is it inseparable from the product of ordinary, non-specific inflammatory conditions of the fauces and tonsils; and that it would therefore no more appear to be necessary to diphtheria than it is to certain conditions of a manifestly different character. Dr. J. Lewis Smith, in an interesting contribution to the *Virginia Medical Monthly* (February, 1875) affirms that he has lately, in conjunction with Dr. Heitzmann, formerly of Vienna, examined the secretions and exudations upon the fauces in various cases of pharyngitis, both diphtheritic and non-diphtheritic, and they have always found the micrococcus in abundance in the inflammatory product, whether diphtheritic or non-diphtheritic, a secretion or exudation, if it had remained for some time upon the surface of the fauces.

It is plausible, therefore, to infer that a few hours or a few days will cause that condition of things in an inflamed mucous membrane, which becomes the efficient cause of the generation, development and propagation of micrococci. The nidus, influenced by the special blood dyscrasia present in diphtheria, may be, it is true, somewhat more favorable to the ready and excessive production of this vegetable organism than the purely inflammatory state, without concomitant constitutional disorder of a grave sort. For, inasmuch as we are well aware that in all virulent fluids or solids, bacteria of special forms are generated and grow with marvelous rapidity, we can also easily comprehend, and be prone to admit, that conjoined to the septic blood changes so markedly present in toxic diphtheria, the mucous membrane lining the air passages may present a very favorable

ground for their rapid pullulation. Nothing is truer, at all events, than the existence of putridity in the inflammatory products of, and exhalations from the throats of patients attacked with this disease—whenever it is of malignant type—and for this reason alone, if for no other we can assume that the micrococci developed in diphtheria, may possibly be in greater abundance, more rapidly generated, and of greater vitality than in common erythematous sore throat. And a like admission will in nowise conflict with the position taken by Dr. J. Lewis Smith, that the micrococcus in the inflammatory product upon the fauces certainly does not indicate disease of a specific nature. In antagonism with the investigations of Letzerich, of Certe!, of Tommasi and Hueter—all of whom recognize more or less the parasitic nature of diphtheria, we find many equally famous observers who wholly deny its reality. First amongst the latter we should mention Trendelenberg, who flatly denies the existence of both micrococcus and granulation. Then come Wagner, Senator¹, Robin, Peter, Laboulbène,² Chauveau, Duchamp³—who, after having made repeated examinations of diphtheritic false membranes, have never been able to discover the vegetable organism which corresponds with the one described by Letzerich, under the name of *Tilletia Diphtheria*—as the full cryptogamous development present in diphtheria. Duchamp has indeed been able to point out numerous granulations of yellowish coloration and of high refractive power, in the false membranes taken from the larynx of children who died of diphtheria. These granulations appeared to him closely to resemble those already described by Tommasi and Hueter, and also the *less perfect* form of parasite made mention of by Letzerich. How far, however, these granulations are to be considered as bearers of the contagion of diphtheria it is yet impossible for him to declare positively. When they were inoculated to animals, they were again found reproduced in the blood of these animals, who likewise developed symptoms of general septic infection. Still there is nothing characteristic about these granulations; and entirely similar ones, according to Chauveau, may be frequently found

¹ Archiv. f. Path. Anatomie und Physiol. 1872.

² Société de Biologie, 2ème serie, t. ii. p. 881.

³ Du Rôle des Parasites dans la Diphthérie—Paris, 1875.

in the blood of patients who have died of different forms of virulent disease—such as vaccinia, variola, &c. And further, these granulations, according to Duchamp, when inoculated, have not, in his experiments, produced on a single occasion the more characteristic anatomical features of diphtheria. To this he adds, as the first conclusion of his interesting memoir, that he has never been able to recognize under the microscope the parasite, which according to Letzerich, is pathognomonic of the diphtheritic process. Inasmuch as the other conclusions, arrived at by Dr. Duchamp, are also very important, with respect to the part played by the noxa peculiar to diphtheria, we give them in full :—

1. The false membranes of croup transferred from the larynx of a man into the larynx and trachea of the rabbit can give origin to the diphtheritic process.

2. In the absence of false membranes, the products (bacteria, vibriones, &c.) collected in the larynx of a man suffering from croup, and transferred to that of a rabbit, appear to lose that peculiarity, whilst they are very noxious.

3. The injection of false membranes from the larynx of a man into the jugular vein or cellular tissue of the rabbit, appears to constitute at least, an unfavorable means of reproducing the diphtheritic process. One of the subjects of experiment died from phlebitis, and the other from a form of septicæmia.

4. Reinoculations appear to weaken the noxious properties of the virulent elements. In the experiments that were made from rabbit to rabbit, the results were constantly negative.

In the absence of false membranes, the products collected in the larynx of man and put under the epidermis of the rabbit, did not provoke the development of the diphtheritic process in the latter animal; it survived without accident.

6. The cutaneous inoculations of false membranes taken from the larynx of man have given equally negative results in the rabbit and the horse.

In other words *it is proven*, if we may accept the above conclusions as final, that *in order to reproduce the diphtheritic process by inoculations, it is necessary to make use of fragments of false membrane taken from the air passages of man and placed in contact with, or under the epithelial covering of, the mucous membrane of animals*; and from this it may be fairly

inferred that a similar method of inoculation would result in communicating diphtheria to individuals previously in good health. Further, that in the *false membranes themselves* of diphtheria, and *not* in the so-called bacteria, vibriones, &c., present in this disease, the peculiar poison of diphtheria is to be found. We say so-called, because Duchamp,¹ Hiller,² and Jacobi present arguments which, to our mind, make it very questionable whether the bacteria described by so many scientists are in reality vegetable organisms with well-defined corporeal features. In a very learned and highly suggestive paper, published in the *American Journal of Obstetrics* for February, 1875, on the pathology and therapeutics of diphtheria, Dr. A. Jacobi regards the false membrane in diphtheria as essentially an epithelial product; and the peculiar amorphous material which according to Certei and others, represents the essential element in the disease, and consists of bacteria, is believed by him to consist of detritus and fat molecules, which are the products of degeneration of the epithelial cells.

Granting, however, that there may yet remain considerable doubts as to whether this last-mentioned opinion should be accepted—we believe there should be none at present, when we refuse to accept the statements of Dr. Letzerich in regard to the specific parasite of diphtheria. Many personal investigations, carried on by experienced searchers, and with the closest attention to accuracy and to avoidance of error, suffice to make the belief widely accepted, that in the blood and false membranes of diphtheritic patients there exist corpuscles resembling those already found in many virulent or zymotic affections—but to admit that they are peculiar in nature, appearance, or properties when visible in connection with diphtheria—there is no proof which to us is at all worthy of credence.

The following experiment of Onimus³ appeared for a time to establish the fact, that the noxa of putrefaction is at least of organic nature. By analogy as much might perhaps be admitted for the diphtheritic poison. The author cited “placed putrefying blood in a bag made of dialytic membrane, and immersed the whole in distilled water, which

¹ Op. cit.

² Berliner Medicin. Wochenschrift, November 30th, 1874.

³ *The Popular Science Monthly*, February, 1875.

after a few hours, was found to be filled with bacteria. Inoculations with the blood produced the usual results; but inoculations with the water caused no septic symptoms whatever; on the other hand, the same blood, when subjected to various processes which removed or destroyed the bacteria, retained its virulence, and from these experiments he drew the conclusion that the virus of putrid infection is not an organized ferment, not bacteria, but an albuminoid substance."

The experiments of Sanderson¹ in the instance of the cattle plague appeared likewise to prove that the substance occasioning septic phenomena in that malady, was probably of albuminoid nature, and should be ranked amongst the colloids. Since that time, it having been found, however, that albumen will, under certain circumstances, and more especially in presence of the phosphates and carbonates, pass across organic membrane in small quantity; the investigations of the two last observers have been invalidated.

Finally, then, it had become an accepted belief of many, previous to the late learned investigations of Curtis and Satterthwaite, that repeated filtering would not always separate *matter* capable of producing putrid or septic changes, from solids or liquids which primarily contain it. To the scientists just mentioned, however, we are indebted for the proof that—

1. The virulent principle of infectious disease does not reside in the perfectly clear fluid which passes through porous clay.

2. The virulent principle is soluble, or at least suspended in water, and the liquid which is rendered poisonous by its presence, may be clear to the eye, but contains granules under the microscope.

3. These granules have not produced bacteria in a number of instances when they have been placed in a suitable condition to do so.

We are thus almost forcibly led to conclude, that in many diseases of an infectious type, the rôle played by bacteria has been misinterpreted. They, in themselves, are not the causative agent of these maladies; but are rather, as Hiller has already asserted, a frequent accompaniment of septic action, and may act as the carriers of contagion. It is not difficult to understand, therefore, that when once the septic process is begun

¹ Twelfth Report of Medical Officers of Privy Council, 1869.

by the presence and influence of the poisonous principle of the contagious or infectious disease (diphtheria, small-pox, typhoid fever, &c.,) such change may take place in the liquids and tissues of the body as to promote the rapid production and development of micrococci, or other forms of bacteria, with more or less characteristic features.

And after this manner may be satisfactorily explained the almost indefinite production or reproduction of the septic poison in any one of the diseases mentioned above. Let us beware, however, of confounding the really pernicious substance with an agent whose influence, to say the least, is but indirect and secondary. Once more—*the bacterium is the abode of the poison, not the poison itself*. This dwelling-place, as it were, may remain the same for quite a period of time, and through an uninterrupted succession of many different individuals.

It may also be destroyed or changed by the use of methods which occasion mechanical separation of the poison with the bacterium, or else which destroy the virulent principle by decomposing it, or simply neutralising its pathological action.

Reasoning by analogy, however, we now feel justified in asserting that such local or general methods of treatment as merely preserve the liquids or solids of the economy from becoming a favorable medium for the reproduction of vegetable organisms, are not on this account to be trusted *absolutely* for their beneficial action as prophylactic or curative agents of diphtheria.

In order to substantiate this opinion, let us consider, for example, the experiments of Dr. Dougall¹ with respect to the destruction of contagia. This author proved that the infecting property of vaccine lymph is unaffected after being buried for thirty-six hours in an atmosphere of concentrated carbolic vapor; and Drs. Curtis and Satterthwaite went still further—for they have established, that even if salicylic acid be mixed in concentrated form with tongue scrapings, it had no effect upon their poisonous matter; and that, when inoculated, septic infection was produced, and the lesions, though without the bacteria, appeared as usual.

Now we are all familiar with the remarkable property of carbolic and salicylic acids—proved by so many observers—by

¹ Glasgow Med. Journ., July, 1875.

which they are able to prevent the formation of bacteria in putrescible substances during considerable lapses of time. And this they will accomplish effectually in relatively weak solutions. Hence their almost constant employment by many eminent practitioners to destroy all organized germs. And yet the experiments of Dougall and of Satterthwaite, already mentioned, suggest, as these authors state, that salicylic acid and carbolic acid, while excellent preservatives and anti-microphytics, are perhaps not disinfectants at all in the strict sense of the term. In other words, the true poison of virulent disease will remain unaltered by their action, even though lower organisms are destroyed and the power taken away by which their reproduction is effected. If this be true of the noxa of virulent disease, it is probably true also of the poisonous principles of zymotic affections in general, including of course the one peculiar to diphtheria. In these diseases, therefore, when by the most perfect and thorough employment of anti-septics we have taken it for granted that we have destroyed their special cause of transmissibility, we have probably tended rather to preserve it intact than to annihilate it.

And what applies to the acids mentioned above, as types of a large class of so-called disinfectants, may apply equally well to many other substances of different chemical and physical properties, but which are co-ordinated by many physicians with the former, on account of a similar destructive action which they exercise over lower forms of life.

Is it not, then, a very doubtful matter whether the beneficial effects of simple preservative agents can be such as have been so frequently described, when they are injected, or sprayed, in a very moderate degree of concentration, into the throats of patients suffering from diphtheria? If virulence cannot be destroyed by the continuous local action of a given substance in its natural state, is it at all likely to be so by the temporary contact of a solution, very considerably weakened, of the same? And if a substance of well-known anti-microphytic power be given internally, shall we therefore obtain an undoubted antiseptic influence throughout the solids and liquids of the economy?

Sufficient has already been written to prove, we believe, with tolerable certainty, that "the germ theory," as applied to diphtheria, is, in part, at least, a manifest error. And yet we find

many eminent practitioners who have been lately so much seduced by its plausibility as to vaunt, with enthusiasm, the prophylactic and curative effects of the anti-microphytics mentioned above, when assimilated by the economy, in the treatment of this dread malady. In our opinion, they have been unduly influenced by the crudest analogies of disease, and in a manner unworthy of scientific observation. For where is to be found the proof of the utility of *preservative* remedies, for those who deny, as we do, that bacteria and their like are proximate and mighty agents of infectious disease? Evidently there is none, and there is, in reality, no good reason for believing that any one of these drugs will affect, in the smallest degree, the poisonous entities, either of diphtheria or of allied affections.

And for those who still hold fast to the germ theory of diphtheria, as of other zymotic affections, how can they willingly accept that the small doses of them which, almost of necessity, are administered through the alimentary canal, have been able to arrest the production of bacteria in every tissue and liquid of the body? We do not believe they can; and thus far, at least, no publication has appeared which has at all substantiated, in our opinion, a different conviction. Let us consider this problem. The weight of the blood, in proportion to the entire weight of the body is, we are aware, as 1 to 8; and the body of a healthy man, weighing 145 pounds, will therefore contain, on the average, about eighteen pounds of blood.¹

Now, of all the so-called disinfectants, there are few, if any, that we can give in anything like the proportion necessary to interfere notably with the vitality of microzymes, without producing injurious effects.

And certainly we may eliminate, for this reason, such medicaments as permanganate of potash, sulphurous acid, carbolic and salicylic acids; and though employed in considerably larger doses, the same is true of the sulphites, hyposulphites, or sulphocarbolates of soda and potash. For, with these latter, as with quinine, also, of which the quantities given at one dose are increasing rapidly, in order to carry out practically the indications which result from Prof. Binz's experiments, we should be

¹ Dalton's Physiology, p. 224.

forced to cause our patients to incur risk. In diphtheria there is, perhaps, a greater tendency than in almost any other amongst zymotic affections towards failure of heart power. Now the alkaline salts, whatever else may be their action in this disease, will certainly, when administered for a time in really active doses, increase notably this cardiac weakness. We should, therefore, be chary of the use of the sulphites and sulpho-carbolates—and doubly so, because we are more than skeptical in regard to the advantages derived from their preservative power. Quinine, too, in other than small doses, is a powerful hyposthenisant; it lowers the arterial tension and reduces the systolic force of the heart. This has already been stated by Giacomini, Binz, &c., and very lately the truth of their opinion has been fully confirmed by the experiments of Dr. Vincent Cherone¹ with respect to the action of quinine upon the circulation, at the Museum d'Histoire Naturelle of Paris.

It behooves us, moreover, in this, as in other difficult or problematical subjects of medicine, to liberate ourselves from preconceived or too absolute theories, which impede our progressive march in the path of exact knowledge. Whilst, then, we are in quest of a substance which shall be a *true antiseptic* in the disease under consideration, let us beware, meanwhile, of narrowing too closely our field of vision. And above all, may we be willing to accept data which result from repeated and accurate scientific research. Now, on the one hand, the specific noxa of diphtheria is as yet unknown, in all its features; and this fact, it seems to us, should prevent too implicit confidence in the *specific* remedial action of any particular medicament. On the other hand, we are persuaded that our views would be broader and our treatment more correct, were we to utilize, so far as practicable, the knowledge already acquired for us. The following conclusions taken from the address² of Dr. Charlton Bastian, in which he attempts to refute views of the upholders of the germ theory of zymotic diseases are, when applied to diphtheria, a most exact expression of our own views of this disease:—

1. The virus, or contagion of some of these diseases, what-

¹ Bull. Gén. de Thérap., August 15th, 1875.

² Delivered before the London Pathological Society, April 6th, 1875.

ever it may be, does not exhibit the properties of living matter.

2. There is the extreme improbability of the supposition that this whole class of diseases should be caused by organisms known only by their effects.

3. The facts of the sudden cessation, periodical visitation, and many of the other phenomena of epidemics, however difficult they may be to explain upon any hypothesis, seem to oppose almost insuperable obstacles to the belief that living organisms are the causes of such epidemics of specific contagious diseases.

ON THE CAUSES AND TREATMENT OF EXCESSIVE OR UNUSUAL UTERINE HEMORRHAGES, WITH ILLUSTRATIVE CASES.¹

BY

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I do not propose in this paper to present a bibliothecal study of menorrhagia or metrorrhagia, or to advance many new ideas upon this subject. I shall endeavor, however, to give a clinical *résumé* of the cause and treatment of these symptoms, with such practical suggestions, based on my own observation and experience, as will best help the busy general practitioner, under whose care this class of practice usually falls at first. The judicious physician of to-day, who has kept pace with the advances made in all that pertains to the diseases peculiar to women, is far better able than was the physician of thirty years ago, to diagnosticate and properly to treat a case, which has for its chief symptom some form of uterine hemorrhage. To-day we are carefully taught, in college and in text-books, the different methods of making a correct diagnosis. Then our eyes and fingers were uneducated; the speculum was but poorly adapted to assist, and was so seldom called into requisition as to be almost useless. Consequently the treatment

¹ Being the substance of a paper read before the New York Academy of Medicine, January 20, 1876.

was empirical, and uncertain as to the result; and the physician would often approach with dread a patient suffering from unusual loss of blood from the vagina.

A knowledge of the best methods of making a correct diagnosis of the cause of all uterine hemorrhages, and of instituting judicious treatment based on such knowledge, should be more general and complete than it is, among a large class of the profession who are usually called upon to attend such cases, especially during their incipency, and often through the whole course of the disease.

I propose to consider the best methods at our command for making a correct diagnosis, and to carefully group the several diseases and pathological conditions which give rise to this one symptom. Possessing such knowledge, we may commence treatment, when summoned to a patient suffering from excessive uterine hemorrhage, with the sure expectation of checking the unpleasant or alarming loss of blood, if not of entirely removing the cause, or curing the disease which gave rise to this symptom. In commencing the study of the pathological conditions of which hemorrhage is an accompanying symptom, we are at once instinctively led to see that these conditions can easily be, and are, properly divided into two general classes, viz.,

PUERPERAL and NON-PUERPERAL.

Among the varieties of the former class are many which fall to the care of the obstetrician. Yet a careful study of the nature, cause, and treatment of the hemorrhages, so annoying to the accoucheur, in immediate connection and comparison with those pathological conditions having hemorrhage for a common symptom, will help to mirror more vividly in our minds the great underlying principles of treatment. There is also a very natural division to be made in our study of the *non-puerperal* variety of hemorrhages. Remembering that, during a certain period of every woman's life, there is a normal flow of blood from the uterus every four weeks, we must decide whether the particular flow which we are called upon to treat, is the regular menses increased in quantity, or whether it is independent of, and occurs between, the menstrual epochs. The former condition is called Menorrhagia, the latter Metrorrhagia.

Menorrhagia can affect the woman only during menstrual life, while Metrorrhagia may occur at any time, and frequently after the climacteric period. The same condition which gives rise to Metrorrhagia will also often be the cause of an increased menstrual flow. Thus an intra-mural fibroid or carcinoma, when occurring during menstrual life, always causes an increased menstrual flux, as well as hemorrhage between the menstrual periods. It is well for us to always have these divisions fresh in our minds, and ascertain, first of all, whether the patient is suffering from a puerperal or non-puerperal hemorrhage. If from the former, learn to what stage of utero-gestation the patient has attained. If from the latter, whether it is an increased menstrual flow. To determine this, the usual type of the menses for the past few months, while in health, must be ascertained; that by comparison the present deviation from the normal can be comprehended, and a reasonable prognosis afforded. The following questions will elicit such information as is necessary before making a physical examination: 1. How long ago did your last menstrual period occur? 2. How frequently in health? 3. Was it always regular as to time? 4. Was it always regular as to quantity? 5. Was it scanty or profuse? 6. Was it usually dark or bright red? 7. Was it thin or in clots of blood? 8. Is the flow usually accompanied by much or little pain? 9. Tell the difference as you have noticed it. With the facts derived from the answers to these questions, we at once proceed to study the pathological condition producing this symptom.

For the best methods of making a physical examination, I might refer to numerous works on Gynæcology, but I wish to emphasize a few points, as I am sure they are so often ignored, or overlooked, by those who are but seldom called upon to make a careful uterine examination. A brief reference to the many aids to diagnosis, some of which are available in each case, may help the general practitioner. I am convinced, from my experience in consultations, and in teaching pupils this branch of practice, that but very few physicians, who are not specialists, are capable of using to the best advantage the knowledge within their reach, simply because they have not been taught how to gain it.

(1.) For a thorough examination of the uterus, it is absolutely necessary to provide a good light. I have seen a uterine examination made in a chamber, by a learned physician, with no good resulting, because the room was dark. A Sims or Barnes could have done but little better under similar circumstances.

(2.) A hard table should be used if possible. If your patient objects to this, let her remain on the bed. The hard table is always preferable, however, and should be placed directly opposite the light. Let the patient lie on the back, head resting on a low pillow, thighs drawn up, legs flexed on thighs, feet resting on the table or bed.

(3.) If the patient is a young girl, or a nervous unmarried woman, it is best to administer ether.

(4.) Now proceed at once with the physical examination of the abdomen with the right or left hand, whichever is the best educated, or both.

(5.) If not satisfied, with the eye also.

(6.) Next question, with the best educated finger, the condition of the vulva and perineum.

(7.) Then the vagina. Examine its size, shape, heat, and moisture. Then, passing upwards until the finger touches the cervix uteri, consider its position, size, shape, density, and mobility. Passing around the cervix uteri, try and ascertain the position of the body of the uterus; if it is movable, and to what extent.

(8.) If you are not able to determine, bring to bear another aid, by using the other hand on the abdomen at the same time--the bi-manual method. You may now be able to grasp the uterus, if the patient is a thin woman. If she is stout, you must resort to the uterine sound. If you are an expert, *before* introducing the speculum, otherwise not until afterward. The sound should indicate the position of the uterus, and its shape and size. If the sound is passed with great difficulty and causes great pain, a stricture or flexion undoubtedly exists.

(9.) If you are not an expert, you must not be sure of either of these conditions until you have used the speculum. This, if a glass Fergusson, will only reveal the condition of the mucous membrane of the vagina and cervix uteri, and the size and position of the external os. Some form of the short-bladed bi-valve specula will serve best, if the patient is a lean woman;

if she be stout, a long-bladed speculum will be necessary. If you place the patient on the side, in the Sims' position, you can use his speculum, if you have an assistant; or Thomas' improved speculum, if an assistant is not at hand. Whichever speculum is used, observe this rule in its introduction: *Know positively the exact location of the cervix uteri before attempting to introduce the speculum.* The uterine sound ought now to pass up readily. If an ante-flexion is suspected, press the fundus backward with the depressor, or a loop of wire; if a retro-flexion, press it upwards and forward with the same means.

(10.) If still uncertain of the conditions, an examination per rectum will be necessary. The finger will detect any disease of that organ, which so often simulates uterine complications. In this way you can judge if a retroversion exists, and of the degree of fixation, and of the irregularity of the posterior wall of the cervix and body of the uterus.

(11.) Another method remains, if this prove unsatisfactory, that of passing the sound through the urethra; or by dilating it, and using the finger.

Before discussing the several diseases which have this symptom in common, I wish to call attention to the comparative frequency of each. I herewith append a table of statistics for the past year, from my own private practice, and also in Demilt Dispensary, in the Department for Diseases of Women. This table will show about the usual proportion in which the several diseases occur; with this exception, that, in private practice, of course there is a greater proportion of hemorrhages from puerperal causes, and acute displacements, than in the Dispensary class, as the latter consists of so-called out-door patients. Of course there are a few diseases that may be attended with excessive uterine hemorrhages that I did not see in the short space of twelve months. There were 620 cases treated by me at Demilt Dispensary in 1875; 37 came because of an unusual uterine hemorrhage. In my private practice I treated 26 patients suffering from this symptom. The following table I have compiled to show the comparative frequency of this affection; also the collateral influences of age and marriage, number of children, and miscarriages.

Hemorrhages from *placenta prævia* are not uncommon. As the cause is a detachment of some portion of the placenta from

the cervix near the internal os, the blood will at first appear very suddenly, and generally will be of a bright red color. The discharge will be attended with little or no pain. If it does not appear before the third month, it may be very profuse, and be followed by the death of the foetus. When it appears earlier, it will be less profuse, but quite frequent; and the patient will present the peculiar anæmic appearance which accompanies a constant loss of blood. The diagnosis can be made from the evidence of pregnancy; from the absence of any disease of the cervix or body, which would occasion such hemorrhage; from the suddenness of the first appearance of blood, and from its frequent occurrence, if it is not soon followed by abortion. Generally the placenta can be felt, soft and elastic, at the internal os. The prognosis is good for the mother, but grave for the foetus. *Treatment.*—If the flooding is abundant, the ovum should be delivered at once. Squibb's or Cooper's fl. ext. ergot should be given hypodermically in xxv. gtt. doses, followed by brandy and strong beef tea or extract, in copious draughts, and ergot in such quantities as will insure constant uterine contraction. The labor should be hastened by dilating the cervical canal with the finger, or Barnes' or Molseworth's dilators. If the hemorrhage still continues profuse, efforts should be made to entirely detach the placenta. This may be done by means of the finger or a No. 16 (American scale) bougie. If this is unsuccessful, a pledget of cotton, saturated with solution of per-sulphate of iron, must be crowded up the cervical canal, and held in position by cotton tightly packed in the vagina. If a tampon is used at all, it should be *well* applied, or its use is a source of more annoyance than benefit. When the hemorrhage from this cause is slight, the patient, by keeping the recumbent posture, for the greater portion of the time, may be enabled to carry the child to full term.

The following case will illustrate:

Case No. 2. Table A.—Mrs. M., 30 yrs., three children; third month of pregnancy; sudden attacks of hemorrhage; the first, three weeks previous to summoning me. Blood bright red; no pain. Has lost blood freely every two or three days since first attack. Blood comes very fast for a few minutes, then ceases. Pulse small and weak. Patient quite exsanguinated. Uterus, normal size for three months' pregnancy. Cervix large, patulous; os opened to size of index finger; a soft elastic substance felt near internal os. On removal of finger, fresh

blood follows. Diagnosis: Placenta prævia. Treatment: Hypodermic injection of twenty-five drops fl. ext. ergot, and brandy by mouth. The foetus was expelled in twenty hours with but little more loss of blood.

Case No. 1. Table A. Patient of Dr. Comstock; exhibited symptoms similar to the above, excepting that the pregnancy was seven months before an alarming loss of blood occurred. Labor was brought on by dilating the cervix, and by internal use of ergot. Child dead. Patient did well.

Hemorrhage from retention of a part of the placenta, or membranes, are quite common, as indicated in the table. The cause of hemorrhage when a part of the placenta remains in utero, may be quickly understood. The uterus cannot fully contract, the bleeding vessels remain open, and there is more or less irritation and congestion from the offending substances. The diagnosis is never difficult, and in most cases may be quickly and readily made. Pregnancy having existed, and a foetus having been expelled, we would naturally suspect this condition. The uterus will be found larger than normal. If the labor, or abortion, has been recent, fresh blood will be oozing from the os externum. If at an earlier date, the blood will be darker, and perhaps fetid, or the flow may not be constant. The cervical canal will be found more open than normal. If a large piece of the placenta remains unexpelled, and the abortion is of a recent date, two fingers can be easily crowded in, and past the internal os, so that the membranes may be felt. If but a small portion is retained, and the abortion took place several weeks, or months previous, the os will be about one-quarter to one-third of an inch in diameter, and must be dilated with a cervical dilator. A small loop of wire can then be introduced, if necessary, and any remaining membrane may be removed, thus making clear the diagnosis. The treatment must always have for its object the removal of the cause. It consists of the proper administration of medicines that excite uterine contraction, and such manual, or instrumental assistance as is needed to remove the offending tuft of placenta, or membrane. I always give fl. ext. of ergot hypodermically, when there has been a great loss of blood and an immediate action is required; this to be followed at once with brandy in the same manner (xxv. gtt.) if the stomach does not retain it well. The foot of the bed should be raised, lowering the shoulders, etc. The patient must be sustained with brandy,

and copious draughts of beef essence. The finger thoroughly cleaned and oiled, should then be introduced into, and through the cervical canal. Constant pressure, sometimes quite firm, with opposite hand upon the abdomen over fundus uteri, forces the organ low down and steadily fixes it. One or even two fingers can be introduced, and by a rotatory motion, will succeed in tearing up and completely removing, the now foreign body. If the patient is stout, it will be well to resort to a wire loop or a blade of the placenta forceps, or the forceps themselves. A wire loop can be readily improvised if the forceps are not at hand, by bending a No. 13 wire, twelve or fourteen inches long, upon itself, twisting it once at a distance of one and one-half to two inches from the bent end, thus making an elliptical loop, and a handle about five inches in length. The rotary motion with this loop, will quickly and safely dislodge any loosely attached membranes, so that with ergot, hypodermically, the cause of the hemorrhage should be entirely removed before the physician leaves the patient. If the placenta has remained within the uterine cavity for one or more months after the expulsion of the fœtus, and there has been, as is common in such cases, a constant slight metrorrhagia, with severe menorrhagia, the treatment varies somewhat. I dilate the cervix to No. 18 (Hanks' dilators), and with Thomas' loop curette break up the now much compressed tuft of placenta. With ergot and quinine in full doses by the mouth, I expect all to be expelled within six hours, if not in a few minutes. In rare cases it may be necessary to tampon the vagina. It should never be resorted to except when the patient exhibits symptoms of exhaustion, and needs a brief rest to gain strength and courage for the manual or instrumental assistance. Even in such cases, I believe the brandy and ergot hypodermically, will immediately give the patient strength, and that the time spent in tamponing the vagina would be sufficient for the complete removal of the contents of the uterus, if the course here marked out is followed.

Case No. 5. Table A.—Dr. A. McL. Hamilton requested me to take charge of Mrs. O., as her case was not in his specialty. Found patient nearly pulseless and fainting. Had expelled a small fœtus with some membranes, about two hours previous. Gave brandy and ergot hypodermically at once; introduced index finger of left hand into vagina and

through the cervical canal into uterine cavity. Pressure at the same time, with the right hand upon fundus, enabled me to detect and quickly remove the remaining membrane. Patient rallied at once, and made a good recovery.

The following case was from retention for one week of a three and one-half months' placenta. It required intelligent and quick action, and moral courage, or the patient must have died within a few hours.

Case No. 7. Table A.—Mrs. C., 34 years, five children: had flooding one week previous; physician summoned pronounced it a miscarriage, and thought placenta had been expelled. Severe hemorrhage continuing, another physician was called on third day. Tincture of iron was given internally and ice placed on abdomen; hemorrhage continued. I was called in great haste early on the seventh day. Found patient almost completely exsanguinated; bed and linen saturated with blood. Cataleptic convulsions had occurred during the previous night. Pulse indistinct at wrist; heart sounds very feeble; respiration almost imperceptible. Compressed fundus at once, and gave xxx m. of Cooper & Co's fl. ext. ergot hypodermically—followed by same amount of brandy. Patient rallied; was then able to make physical examination and quickly remove about one-half of an adherent placenta. Placed patient on dry bed, with head and shoulders lowered; ordered frequent draughts of strong beef-tea and brandy. Patient made a quick and good recovery.

There are three facts which ought always to be borne in mind when treating these cases: 1. The retained membranes are the cause of the hemorrhage. 2. The patient can gain but little or no strength while the cause remains in utero. 3. The safety of the patient is to be considered before the *convenience* of the attending physician. The following case will illustrate:

Hemorrhage from Inertia of the Uterus is the most appalling of all complications of the puerperal state, and, as in the treatment of the last case, demands immediate action. This accident usually occurs when there has been some previous ill-health, or uterine disease, causing a deficiency of nerve power. The womb refuses to contract, or to remain contracted; the vessels remain open, and blood is freely poured out into the easily distensible uterine cavity. The treatment, to be effective, must be prompt and persevered in, until the danger is passed. With one hand firmly pressing upon the fundus, through the abdomen, the other hand, or two fingers, within the uterine cavity, can remove all the clots of blood, and excite the uterus to

contraction by irritation and pressure upon its walls. Ergot given at once, hypodermically, and quinine and brandy internally, strengthens the flagging powers of the uterus and sustains the patient. An intelligent nurse should be required to continue constant pressure over the fundus for several hours. The patient should not be moved. Electricity may be used, if desired. Ice, crowded up the vagina and into the uterine cavity, or upon the abdominal wall over the fundus, is often of advantage. If all these means fail, which I doubt, if they are persevered in, the uterine cavity must be filled by means of a syringe, with some powerfully astringent medicament. Solution of persulphate of iron is probably the most effective and reliable.

When a not very strong woman, after a hard, tedious, labor, with complete delivery of the placenta, has sudden hemorrhage, it is because the uterus is too weak to perform its duties, and requires assistance, or death will soon carry the patient beyond all aid.

The following case will illustrate:—

Mrs. M., aged 29, had been successfully treated for retroflexion, by the late Dr. J. L. Brown. Taken in labor with first child at four A.M. March 20th, 1870, went through the three stages with no untoward symptoms, except, vomiting during the last hour. Labor was completed at ten A.M. March 21st. Patient quite exhausted, but uterus contracted well after the expulsion of placenta. I was about to leave, when the patient suddenly became faint from hemorrhage. Found uterus filled, and rapidly expanding with fresh blood. For the first time in my practice, I administered ergot hypodermically (xxv. gtt. Squibb's fl. ext). The nurse was ordered to compress the uterus, while I removed the clots from the womb, which I was pleased to find firmly contracting. Ordered brandy, ergot and laudanum. On the following morning, there was a slight hemorrhage, which was quickly checked with ergot hypodermically. In this case, one hypodermic injection of ergot accomplished far more than had several doses by mouth, during the previous twenty-four hours. Patient made good recovery.

The puerperal hemorrhages, occurring as a result of laceration of the cervix, are far more common than is usually supposed. They are less severe, and consequently attract less attention, and cause little or no apprehension to the patient, or physician. The diagnosis is often not made at all, although the uterus may be found, at the time, to be imperfectly contracted. This sub-involution will remain for a long time, and only at a later

date will the cause of the too abundant flow be ascertained, when, perhaps, the patient may consult a physician for some so-called weakness. An examination will then reveal the open os and gaping cervix; unmistakable evidence of laceration during the previous labor. Hemorrhage from this cause comes directly from the laceration and the open vessels in the cavity of the uterus, which cannot contract properly, because of the defective contraction of the cervical fibres. The diagnosis of this cause of hemorrhage is not easy; we can only form a probable opinion by excluding all other causes. If the laceration is severe, a careful digital examination will reveal it, but if it is slight, it is almost impossible for any one but an expert to certainly discover it. The method of treatment usually followed in these cases of hemorrhage, is consequently the same as for a too profuse lochial discharge. When a laceration is surely diagnosticated, however, in addition to the usual treatment, frequent and copious vaginal injections of warm water, with alum in solution, should be ordered, with special attention to nourishing diet and posture. The position of the body has a powerful and certain influence in checking or increasing a profuse hemorrhage, according as the hips are higher or lower than the other portions of the body.

In preventing the frequently alarming loss of blood during abortions, from whatever cause, the three principles mentioned in connection with hemorrhage from retained placenta, will apply: 1. The retained membranes (the foetus) cause the hemorrhage. 2. The patient can gain but little or no strength while the cause (the foetus) remains in utero. 3. The safety of the patient is to be considered before the convenience of the attending physician. We should remember that there is no *absolute safety* for the patient, until the foetus and membranes are entirely expelled, and the uterus firmly contracted. The diagnosis of the cause of hemorrhage from abortion, can generally be positively made out. There will have been the previous cessation of the menses, the presence of gastric disturbance, and all, or some of the well-defined symptoms of early pregnancy. The cause of the abortion may often be learned, and, in exceptional cases, suspected. The uterus will be found enlarged, the cervix soft and patulous, and the os externum open. If these symptoms exist, and are attended with profuse hemorrhage,

and we find unmistakable evidences of pregnancy from the past symptoms, and apparent labor pains present, we should encourage the expulsion of the ovum. Such medicines should be given internally as will ease pain and cause uterine contraction. I am accustomed to prescribe morphine and chloral with ergot, and find it effectual in expelling the fœtus and membranes within fourteen hours. If the hemorrhage is severe, dilatation of the cervical canal will be necessary with the fingers, if a Barnes' or Molseworth's dilator is not at hand. The uterus will then make powerful efforts to expel the offending body. If the patient becomes exsanguinated before the ovum is delivered, ergot and brandy should be administered hypodermically, with tinct. opium and beef essence internally. The placenta forceps, carefully handled, will do no injury, and in such cases will hasten, what is so absolutely necessary, the delivery of the fœtus. This method of procedure will surely take less time, if saving of time be an object to the physician, than it would to place a sponge tent in the cervical canal and tampon the vagina; besides being, as I believe, the only safe method for the patient.

A very common cause of excessive non-puerperal hemorrhages is a fibroid tumor in the uterine cavity. Time and space will allow only a general mention of these tumors, but for the physician unaccustomed to the diagnosis of the three varieties, a few suggestions will be helpful. The sub-mucous fibroid, or intra-uterine variety, is attended with the most frequent, persistent and severe hemorrhages. As the tumor grows directly beneath the mucous lining of the uterus, it is a cause of constant irritation by its growth, and produces congestion of this always easily bleeding membrane. The mucous membrane, too, is developed, and the surface increases as the constantly enlarging uterine tumor demands.

The sub-peritoneal fibroid, growing as it does, towards the peritoneal cavity, is a source of less irritation to the mucous lining of the uterus, from which blood is so easily poured out, but is attended with more pain, because of its constant pressure upon the peritoneum. In some instances, however, it causes considerable menorrhagia. The pain, instead of the loss of blood, will be the symptom which first attracts the patient's attention, and for which the physician will be consulted.

The interstitial, or intra-mural variety of fibroid, is likely to

cause less pain than the sub-peritoneal, with more hemorrhage ; yet not so excessive as attends the sub-mucous variety. The fibroid polypus, within the uterine cavity, is also attended with more or less menorrhagia and metrorrhagia. Generally, the diagnosis can be made quickly, and with much certainty in all cases of uterine hemorrhage from fibroid tumors. After excluding all puerperal causes, the size and density of the uterus should be determined, remembering that, when these tumors exist, the uterus will enlarge irregularly from within or without. If the uterine cavity is large, and the hemorrhage profuse, a sub-mucous or intra-uterine fibroid may be suspected. If the pain is severe, and the flow but little more than normal, there is probably an extra-uterine fibroid. While the uterine cavity is always enlarged in the latter variety, a thickening of the walls in undue proportion to the cavity will be noticed. The density and hardness of the walls will be increased, and sometimes the uterine cavity will be found at one side of the median line. The treatment must have for its object : 1. Prevention of the severe loss of blood. 2. Removal of the cause. The former, I believe, can be accomplished in nearly all cases with proper medicaments. The latter is feasible by means of the knife or *écraseur*, or by the persistent use of the same medicaments given to control the hemorrhage. During the last three years, in all my cases of fibroids, I have been able to prevent all metrorrhagia and to reduce the monthly flow to nearly normal quantity. Fluid ext. of ergot, in combination with some form of Peruvian bark by the mouth, persistently used, has been my chief remedy. The necessity of good food and exercise is enjoined upon the patient in order to keep the general health in the best possible condition. At the usual time for the appearance of the menses, an extra amount of ergot in the form of hypodermic injections has apparently been effectual. In the great majority of these cases this course will be crowned with success ; the smaller tumors will entirely disappear in time, and the larger ones be made much smaller. When this treatment fails, which is rarely, and the hemorrhage is still profuse, the tumor, if a sub-mucous, may be removed by enucleation ; and if a polypoid variety, by the *écraseur*. If an interstitial tumor exist, the foregoing medical treatment must be followed. Even in the sub-peritoneal variety, which causes but little

hemorrhage, ergot will serve better than any other remedy. If the patient is nauseated by a constant use of the drug by the mouth, ergot suppositories per rectum may be used.

Case in Illustration (No. 15, in Table B). June 23, 1875.—Miss R. B., Ireland; sent me by Dr. L. D. Buckley; æt. 40 years, single. Had lost blood excessively for three years; flowing between regular menstrual epochs; menses painful and profuse (previous to the first hemorrhage they had been regular and moderate in quantity); flow, bright red color, sometimes a little darker; often in clots; enlargement of abdomen perceptible below the navel. Patient pale, thin, nervous. Physical examination revealed the cervix uteri low down, merging quickly into body; os open, size of ten cent piece; body extending upward, and of the size of a three months' pregnant uterus; on pressing finger up cervical canal, a resisting body was felt, one-half inch from external os; the finger could be swept around this body in dilated internal os: uterine sound passed with difficulty, but measured $4\frac{1}{2}$ inches. Diagnosis, submucous fibroid. Prognosis good for the removal of the bad symptoms. Treatment.—As an operation would have been refused, if proposed, the patient was ordered fl. ext. ergot and fl. ext. cinchona; one teaspoonful of mixture three times daily; good food, sunlight, and proper exercise. Improvement was marked; no hemorrhage occurred after commencement of treatment—except at menstrual periods, which were nearly normal.

Carcinoma and Epithelioma of the uterus are almost sure to occasion more or less hemorrhage. Frequently this symptom will be the first indication to the patient of any uterine trouble. If the disease occur at the climacteric period, as is often the case, the pain and hemorrhage will be borne for a longer time without exciting suspicion, than at any other time of life, as most women are taught to expect more or less hemorrhages at this period. In cancer of the uterus, there is a tendency to the development of a spongy, slightly gritty, easily broken-down growth, composed of congeries of blood-vessels, which bled freely when bruised or torn in the least. In the cauliflower variety of cancer, this growth is very abundant. In the true scirrhus variety it hardly exists at all. In the early stage of cancer, we may expect hemorrhage in proportion to the amount of this fungous growth. The diagnosis is easy after the disease has made considerable progress. In its incipiency it is comparatively but seldom seen; nevertheless it may be detected. Either loss of blood, or severe pain in the pelvic region, or both combined, will call for an examination.

The general appearance will, in nearly every case, denote a

serious constitutional disease. The peculiar paleness of a straw-colored tint, the frequent metrorrhagia, or hydorrhœa, the fetid discharge, the pain, the fungous growth, indicate a malignant disease in vagina or uterus. The prognosis is always grave, but varied as to time, according to the amount of tissue involved, and the type of the disease. In a case of cauliflower cancer, the bleeding can be controlled for a time and the progress of the disease greatly retarded, and sometimes apparently cured. If the ulceration has involved much of the uterus, there can be none but palliative treatment, with medicaments for controlling the hemorrhage. In the earlier stages, in order to prevent the hemorrhage and stay the disease, the fungus must be removed. This can best be done by scraping it away with a Sims' curette, and afterwards applying, locally, the weak solution of per-chlorate or per-sulphate of iron. When a sharp curette is not used, a strong solution of per-chlorate of iron should be applied by means of a small uterine syringe. By placing the nozzle of the syringe in as many different spots as possible of the soft fungous growth, and throwing in five or eight drops in each place, we may expect, in a few days, to find it has entirely sloughed away, leaving bare the hard scirrhus base. This treatment is simple, causes no pain, no hemorrhage, and is as effectual as the gas-jet or galvanic cauterization.

In discussing the subject before the New York Academy of Medicine, Prof. E. R. Peaslee remarked that if the patient had lost considerable blood, and had become anæmic, "it is best to resort to this procedure (the use of the curette) at once, for a fatal hemorrhage may attend any delay, and if the parts are well exposed, and the fungus removed quickly, as a rule, severe hemorrhage does not attend the operation. If the scirrhus tissue below is cut through, a profuse hemorrhage will follow, but if only the fungous growth above the scirrhus base is swept away, not much hemorrhage will be produced. This operation will arrest the growth of these little fungous projections for a time, and, if regularly repeated, perhaps every two or three weeks, it will check the progress of the disease, and the life of the patient may be prolonged for a year or more." Dr. Peaslee mentioned one case in which the patient's life was prolonged for a year, and when death did occur, it was

not from hemorrhage or septicæmia, but from a gradual failure of vital power.

To illustrate, I cite the following case :

Case, No. 31, Table B.—M. M., æt. 35 years, married 14 years, 5 children, 2 miscarriages, last miscarriage 5 months previous of 3 months fœtus. Cause unknown. Came to me because of "alarming loss of blood from front passage." Well and strong until 18 months previous, when she experienced uncomfortable lame sensation in region of womb. During pregnancy, discomfort increased; and, since abortion, pain has become sharp and lancinating. Menstrual flow greatly increased. Hemorrhage, at frequent intervals, between monthly periods; also constant discharge of dark, fetid, watery fluid. Had taken much medicine to check hemorrhage, but had never used local remedies. Patient pale, straw-colored tinge, and with shrivelled appearance, which indicates rapid wasting of flesh. Pulse, small and frequent. Physical examination revealed blood oozing from between labia, some fetid clots in vagina, cervix uteri low down, with excavations in some parts, and a few soft outgrowths. Hard pressure caused the outgrowth to break down, and bleed profusely. Deeper in the cervix was much induration. This extended upwards, involving the bladder. Diagnosis: carcinoma uteri of encephaloid variety. Diagnosis confirmed by Drs. H. F. Walker and W. H. Vermilye. Prognosis unfavorable. Treatment.—Solution of per-chlorate of iron was thoroughly applied with uterine syringe to the fungous outgrowth. The patient ordered to take iron, quinine, and arsenic, with nourishing food. Local treatment every week. For five months the disease caused no hemorrhage, and made but little progress, at the end of which time the patient was compelled to leave town, and consequently passed from under my further treatment and observation.

Hemorrhages caused by *displacements* and *flexions* are very common, and often quite profuse, occasioning the patient much alarm, and, if not well understood, much anxiety to the physician; these hemorrhages are but little under the influence of medicaments, and unless the treatment is first directed to the local cause, the physician will be disappointed in his efforts. Flexions and displacements, as a rule, cause menorrhagia rather than metrorrhagia, and the menstrual flow will not only be profuse, but of long duration. The cause of this symptom can be easily explained and understood. Retroversions and retro- and ante-flexions are the most common displacements which induce excessive hemorrhages. In studying the former, we find the uterus, at the time of the menstrual flow, with fundus low down in the hollow of the sacrum; the cervix high up, and pointing towards the pubes. The broad ligaments are weakened from a con-

tinnal state of tension. The arteries which should, in normal condition, convey the blood upwards to the fundus, perform their function more easily still, in its retroverted condition, while the veins, which are more easily compressed, are thus compelled to convey the returning blood upwards to the cervix.

Whether a retroversion may be due to a congestion of the endometrium, is still a disputed point. But there is no doubt of the fact that venous congestion is exhibited immediately after an acute retroversion. The treatment for hemorrhage from any of these causes must have for its object the replacement of the uterus in its normal axis. The best method of procedure to accomplish this end must, in each case, be left to the judgment of the physician. In some cases of recent date, the organ can be easily replaced by means of a properly bent uterine sound, while the index finger presses against the fundus. Cases of longer standing will require considerable patience and perseverance on the part of the physician. While the patient is in the knee-elbow position, the fundus must be crowded back slowly with cotton soaked in glycerine. Each day a little progress may be made, and as the congestion yields to the pressure and the glycerine. In a few days the desired result will be nearly attained. An Albert Smith or Thomas improved retroversion pessary will then complete the cure. Case in illustration :

Case No. 35. Table B.—Mrs. N. S., Ireland, 40 years, married two years. Never pregnant. Placed under my care by Dr. E. J. Darkin, on account of profuse and dangerous menorrhagia ; never rugged ; previous to marriage was obliged to sit much at her work. Since marriage has had more active life. The menses previous to this was excessive for the first time, followed by profuse leucorrhœa. Much pain in pelvis. Had flow every week, since last menses, for several days at a time. This flow so exhausting as to confine her in bed.

Found patient pale, pulse quick and weak. Respiration, sighing ; physical examination revealed a markedly retroverted uterus with blood oozing from os externum. Gave brandy and ergot, hypodermically, and partially replaced uterus with sound and finger. Then crowded up cotton and glycerine in Douglas' cul-de-sac and packed vagina with fresh cotton. Placed patient with head and shoulders lowered. Ordered ergot, opium, and quinine with large quantities of beef tea. The following day found patient much improved ; had lost no blood. Same treatment was continued, with the exception of the hypodermic injections. On the third day an Albert Smith pessary was applied, and the patient made a good recovery.

Hemorrhages from flexions are to be treated in the same manner, as the cause is similar, viz.: congestion from impeded venous circulation. This condition must be removed before the menorrhagia will be lessened. It may not be cured as easily as a retroversion, but it can be helped at once. The loss of blood is seldom so alarming as in retroversion. The uterus must be straightened by means of the sound, or small dilators, and the organ *held* in this condition for twenty or thirty minutes. Afterwards a suitable pessary, according to the character of the flexion, must be applied. This will relieve the menorrhagia, but the treatment must be judiciously pursued before another menstrual epoch. We cannot hope for a complete cure in one or two months; but if the proper course be continued, the patient will surely, if slowly, improve.

Hemorrhages from so-called granular erosion are seldom alarming, are easily diagnosticated, and with such treatment as is within the power of every physician, can be surely prevented. Remove the cause by such local application as will best meet the indication in each case. When only a small portion of the epithelium of the cervix is destroyed, either a weak solution of per-chlorate of iron, or dry iodoform applied on cotton directly to the parts, or sol. of nitrate of silver may be used. The latter remedy I never employ, except in the case of patients who have passed the menopause, as its use is so often followed with cicatricial contraction, thereby producing sterility. When the destruction of the epithelium is great, and has existed for a long time, and we find a large, red and bleeding cervix, the curette or strong nitric acid will be required.

Hemorrhages from a fungoid growth upon the mucous membrane lining the uterus, are very common. The diagnosis would be difficult for a physician who has never dared introduce a sound or curette within the uterine cavity. The history of such cases will be that of a profuse menstrual flow, with more or less backache, and considerable uterine leucorrhœa. The disease consists of a number of exuberant outgrowths, which bleed easily, as do similar fungi of the mucous membrane in other portions of the body. These little tufts are composed of congeries of blood-vessels and connective tissue; are always easily congested, and at the menstrual epoch bleed freely. The diagnosis can be made by excluding all other causes, and

confirmed by a Thomas' curette, the use of which instrument, after dilating the cervix to No. 12 or 14 (Hanks' dilators) constitutes the sole treatment. The cure is speedy and certain.

The following case illustrates :

Case No 46. Table B.—Mrs. M. C., widow eighteen years, never pregnant, had profuse menorrhagia for nineteen days; always well until time of attack, with the exception of increased menses of late, called me on the nineteenth day. Patient apparently well nourished, but very pale; extremely weak. Pulse small and feeble. Respiration shallow and sighing. Physical examination revealed nothing abnormal, except open os. No. 10 dilator passed easily. With the curette some fungoid tufts were detected and removed. The next menstrual epoch was normal. The second was too profuse, and another operation was evidently required, which resulted in a complete cure.

Any disease of the broad ligaments, or displacement or disease of the ovaries, will produce congestion, and will generally be attended with an increased menstrual flow. The diagnosis is not always easy for the general practitioner.

There will be more or less pain and tenderness over both iliac regions, with severe back-ache; and on physical examination, a severe pain will be occasioned by pressure, posterior to, and on the right and left of the fundus uteri. If an ovary is displaced, it may be felt in a thin subject. The treatment consists in a reduction of the congestion of these parts between the menstrual epochs. Hot-water injections per vaginam, perseveringly used; local irritation on abdomen over region of ovaries; leeches in the same locality before menstrual flow, and abstinence from all sexual indulgence, will be followed by gradual improvement.

Pelvic or sub-peritoneal hæmatocoele is generally attended in its earliest stage with a quite constant but slight flow of blood from the uterus. Yet the sudden onset of the disease—usually during a regular menstrual flow—the alarming prostration, local pain and tenderness (which only yield to the most heroic doses of opium), the pallor and feeble pulse, for which the uterine flow will not account; all, or any one of these symptoms, will completely mask the minor fact of a small quantity of blood oozing from the cervical canal.

Acute pelvic cellulitis is often, and indeed generally, attended with slight bloody show, because of the congestion re-

sulting from the contiguity of the uterus to the seat of inflammation. The symptoms which attract the attention of the physician, and cause suffering and anxiety in the patient, are local pain and induration, fixation of the uterus on the side where the cellulitis exists, and the marked febrile reaction.

There are some other conditions of the pelvic organs which have Menorrhagia or Metrorrhagia as one of their symptoms; but the loss of blood is of minor account, and treatment is instituted without special regard to this symptom.

Hemorrhages from constitutional causes are occasionally met with. Scarlatina, Rubeola, Variola, and Diphtheria, may each be attended with a profuse loss of blood. One patient attacked with Scarlatina Maligna had lost a large quantity of blood before I saw her, and all remedies failed to check the hemorrhage or stay the disease. Two cases of Diphtheria came under my care when accompanied by this alarming symptom. In one, intra-uterine astringents of a solution of tannin in glycerine, and attention to posture, with the proper treatment for the disease itself, resulted in recovery. The other patient died on the third or fourth day. Death in the second case, though almost certain from the first attack, was doubtless hastened by the great prostration resulting from the menorrhagia.

Besides these blood diseases, uterine hemorrhage sometimes occurs during the course of Phthisis Pulmonalis, and often in functional disease of the heart. The treatment must be varied with all these cases, as each may require. Intra-uterine medication is frequently of avail. After the flow has ceased, a passive congestion usually exists, which must be reduced. Professor F. Barker, in discussing this portion of the subject before the New York Academy of Medicine, made the following remarks:

“Menorrhagia may be caused by the toxic influence of various exanthemata. A most remarkable case of menorrhagia once came under my observation, and occurred in the person of a girl, æt. 11 years, the daughter of a member of this Academy, who was suffering from measles. The exhaustion present in that case was equal to anything I ever saw in the adult female, having profuse uterine hemorrhage from any other cause. It was finally arrested, after a great variety of measures adopted had failed, by the introduction of an alum

cone into the cervix. This is the only case I have ever seen in connection with measles."

"There is another class of cases in which excessive uterine hemorrhage occurs, dependent upon a *constitutional cause somewhat peculiar*. Perhaps I am wrong in classing these cases under a constitutional head; but the class of cases to which I refer may be described as follows:

"A woman who has been free from any uterine disease, perhaps has borne one or two children, suddenly begins to grow stout; exhibits a remarkable tendency to obesity; so that from being a thin, spare woman, she increases in weight from fifteen to fifty pounds, or even more, and with this remarkable tendency to obesity there is in some cases a rapid diminution in the amount of blood lost at each menstrual period.

"But there are other cases belonging to the same class, where, instead of diminution in the menstrual discharge, this tendency to obesity is accompanied by an excessive uterine hemorrhage.

"These are troublesome cases, for the reason that you will suspect, from the severity of the flow, some serious uterine lesion; but when you come to make the most careful examination you will be rewarded by a failure to detect any condition which will satisfactorily explain the occurrence of the hemorrhage.

"The only thing which does explain the hemorrhage in these cases, is the increased size and consequent plethora of the uterus.

"I think this class of cases is not very rare. The appearance of these patients is usually healthy, and the countenance is commonly flushed, but at the same time they are anæmic. The blood is impoverished, and the patient has a flushed face, simply because the capillaries upon the surface, which are not large enough to allow blood of a normal character to pass through them, now carry red anæmic blood, and the slightest emotion or exercise causes them to be surcharged and the face to become flushed. They are cases of anæmia, and when placed upon the use of tonics, quinine, iron, chlorate of potassa, etc., instead of getting stouter and increasing in weight, they will diminish in size and weight, while the patient is greatly improved in health and strength."

Climacteric hemorrhages are nearly all to be explained on some well-defined pathological basis. It is well for us to re-

member this fact, and make a careful examination in each case. Many a severe disease has been allowed to go on from its mild incipency to its disastrous end, without the knowledge of the physician, simply because so many women are educated to the belief that they must expect to lose great quantities of blood at the change of life. Physicians, too, are accustomed to put off these cases by informing the patient that she is passing the climacteric period, and must expect relief only from time. The menopause, in a healthy woman, ought not to cause any disturbance. If the general condition is good, with no disease or displacement in or about the genito-urinary organs, this period will be passed with no abnormal symptoms.

Let each patient suffering from menorrhagia or metrorrhagia, at the climacteric period, receive special attention and careful examination. We shall then be able to treat each case on its pathological merits, and perhaps save the patient from many unpleasant and dangerous complications.

TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

Reported by PAUL F. MUNDÉ, M.D., Secretary.

Stated Meeting, January 4th, 1876.

The President, DR. T. G. THOMAS, in the Chair.

The conclusion of DR. WM. T. LUSK'S

CASE OF PREGNANCY WITH FIBRO-CYSTIC TUMOR OF THE UTERUS;
OCCLUSION OF THE CERVIX, ECLAMPSIA, VERSION, POST-
PARTUM HEMORRHAGE—DEATH FROM PERITONITIS AND COL-
LAPSE—RESUSCITATION OF STILL-BORN CHILD,

is as follows:—

After the resuscitation of the child, Dr. Van Vorst, who had mean time taken my patient in charge, called my attention to her pulseless condition. The hypodermic syringe was at once filled with brandy, and its contents injected into the subcutaneous tissue of the leg, which at once restored the patient's circulation.

The next three days found the patient cheerful, comfortable, and, save a rather quick pulse, without any threatening symptom. On the fourth day, she began to complain of pain in the left side, increased by lying upon the right. Her temperature rose to 102°; vomiting then came on, and the pulse became very rapid. Toward the end of the fifth day, the temperature rose to 115°, and death ensued.

The family consented to a post-mortem examination. I expected to find imperfect contraction of the uterus, and thrombosis of the veins, with peritonitis. The result, however, showed that the cavity of the uterus was in a perfect healthy condition, without any trace of traumatism. It occupied only a small portion of the growth on the right side, and was well contracted. The tumor was of the fibro-cystic variety. It was

¹ This conclusion, without which Dr. Lusk's case might create a false impression, was accidentally omitted from the "Transactions" in the last number.—ED.

honeycombed with cells containing a clear fluid. Everywhere on the left side it was bound by old adhesions to the intestines, omentum, and the peritoneal covering of the abdominal walls. Organized bands, some of them the size of a goose-quill, connected the growth with contiguous organs. A small amount of fresh lymph covered the adherent portion of the tumor, but none were found upon that portion in which was situated the uterine cavity. The abdomen contained a small amount of reddish serum. The tumor weighed $6\frac{1}{2}$ pounds. The examination, therefore, showed fresh peritonitis, taking its origin from old peritoneal products, the result of by-gone trouble. Its origin seemed to be due to the dragging of the uterus upon its attachments, after its abdominal supports were weakened by the emptying of the uterus. This adds one more case to a number, which I have already witnessed, in which latent peritonitis, or the products of previous inflammation became the source of acute inflammation of the peritoneum either during labor, or shortly subsequent to child-birth.

DR. NOEGGERATH stated that old cases of chronic inflammation are very apt to become acute after labor; even chronic metritis might then assume the acute form. He had published a paper in support of this view nine years ago. The practical result of this was, that a case, in which previous chronic inflammation had existed, should be carefully watched during puerperality.

DR. JOHN G. PERRY said that he always looked for acute endocervicitis in cases where that affection had existed previously, although perhaps latent during pregnancy. He had fourteen cases in which it had returned in an aggravated form after delivery.

DR. NOEGGERATH suggested that this point possessed some practical importance; as, for instance, in deciding the question whether to advise marriage and impregnation in areolar hyperplasia of the uterus. The benefit seemed to him questionable. He had followed up several cases of areolar hyperplasia, and had not found them at all benefited by the marital relation. Areolar hyperplasia is apt to increase after pregnancy. He was not prepared to state the reason, unless the uterus happened to be bound down by adhesions, by which the organ was prevented from contracting, air was admitted, the lochia became decomposed and a source of irritation. He had seen one case after a miscarriage at the sixth month, where the uterus was thus bound down and the cavity was found dilated to the capacity of six fl. oz. The woman died from air admitted into the system, where it appeared under the skin of the thorax, between the second and third ribs on the left side, in the form of emphysema.

DR. JACOBI asked how the air could get into the system through the uterine. It generally requires a pressure from behind, and enters very slowly, spreading from point to point.

DR. NOEGGERATH said that air might enter by the uterine sinuses, and thus get into the circulation. On Dr. Jacobi's remarking that it would probably kill the patient before passing into the general circulation, Dr. Noeggerath said that a small quantity might pass the heart, rupture the lung-tissue, and so get into the cellular tissue.

DR. JACOBI then said that it could not get under the skin, unless there were pleuritic adhesions.

DR. NOEGGERATH mentioned experiments by a French author, in which rabbits were allowed to bleed to death soon after parturition; and, as the result of the extreme anæmia, air was sucked into the veins.

DR. J. W. S. ARNOLD, who was present as a guest, was requested by the President to state the result of his experiments of injecting air into dogs.

DR. ARNOLD said that the effect on the dogs depends on the quantity of air injected. A dog of medium size recovered after 40 to 50 cubic centimetres of air had been injected. The air is sometimes found in the lungs as interstitial emphysema. He had never seen it appear under the skin.

DR. JACOBI then said that the explanation of Dr. Noeggerath's case seemed to him to be the following: Since Dr. Arnold had shown that the heart could bear so much air, and that rupture of the vessel in the lungs takes place, vessels might be ruptured in other places as well, especially in the liver, subcutaneous connective tissue, etc. This would necessitate the air being forced through into the arteries.

DRS. LUSK and ARNOLD both stated that they had never seen air in the arteries after experiments on dogs.

THE PRESIDENT invited Dr. Arnold to continue his experiments on the passage of air into the arteries, and to report the results to the Society.

DR. JAMES B. REYNOLDS showed a

SPECIMEN OF EPISPADIAS

from a child two months old. The peculiarity of the case was, that the child could not retain water at all when sitting up, but could hold it perfectly when lying down, even to the production of a protruberance over the region of the bladder. It would then cry, being evidently in pain, but could not relieve itself until raised. A catheter passed about $\frac{3}{4}$ of an inch into the bladder. The child died of gastro-intestinal disease. These

cases are very rare, and an operation for their relief is very unsatisfactory.

DR. JACOBI explained the peculiarity in micturition, by supposing that the accumulation of urine in the bladder pushed forward the soft tissues, forming a valve, which obstructed the flow in certain conditions.

Stated Meeting, Feb. 15, 1876.

The First Vice-President, DR. A. JACOBI, in the Chair.

CASE OF FIBROMA OF THE OVARY—OVARIOTOMY—RECOVERY.

DR. JAS. B. HUNTER presented a solid tumor of the ovary, of the size of an adult head, which he had removed three days previously.

A microscopical examination of the specimen was made by Dr. E. A. Maxwell, who made the following report:

DR. JAMES B. HUNTER:—As per request, I have carefully and repeatedly examined the specimens taken from the solid tumor removed by you at the Woman's Hospital, February 12th.

1. As to origin: The piece of pedicle removed bears along one border a portion of the Fallopian tube, near its fimbriated extremity, although the latter is not present in the specimen under examination. The remainder of the pedicle is a piece of the broad ligament bordering the Fallopian tube. Thin sections of the outer portion of the latter show transverse and oblique sections of about sixty-eight rounded and ovoid spaces, with a moderately thick connective tissue wall, lined by glandular epithelium. These resemble in structure the tubes of the parovarium. Besides these nothing was seen, excepting the normal structure of the broad ligament and Fallopian tube. Examination of the tumor at its point of attachment gives the same results as elsewhere. I should think its point of attachment was near the corner of the uterus, and from its growth it may have raised and appropriated the posterior fold of the broad ligament.

2. The epithelial covering of the tumor appears to be of the small cuboidal variety of epithelial elements.

3d. The general structure of the mass of the tumor is composed of fibrillar connective tissue, interlacing in various directions; so that, in whatever direction the cuts are made, they include longitudinal, oblique, and transverse sections of the fibrillar trabeculæ. This holds true for the whole tumor, ex-

cepting near its periphery, where they generally run perpendicular to the surface.

It has a connective-tissue basis substance. The tumor throughout is unusually well provided with nuclei. These in some places are collected into little clusters. Excepting those portions which, when fresh, appeared a light pinkish color, the whole structure is undergoing fatty degeneration. In the central portion we see the results of this degeneration in the formation of cysts of varying size (softening cysts), and in the shreddy appearance and soft consistency.

I have not found either sarcomatous elements or smooth muscle-fibres, although in a few places it contains cells bordering on each.

Histological classification would place the tumor among the fibromas.

Fibroma.—Unusually rich in nuclei, and undergoing fatty degeneration, containing softening cysts.

E. A. MAXWELL.

[The patient made a good recovery.]

SPECIMENS OF EARLY OVUM, MENINGOCELE, ETC.

Dr. A. JACOBI presented several specimens:

1. A curiosity from the practice of Dr. Van Wyck—an ascaris, strangulated at about its middle, in the eye of a shoe-button, both of which had been vomited in that condition by a child two years of age.

2. A supernumerary thumb, which he had removed from a child three weeks of age. It had a joint of its own, and was well developed. In another case lately observed, he left a portion of the bone of the extra member, so as not to injure the joint.

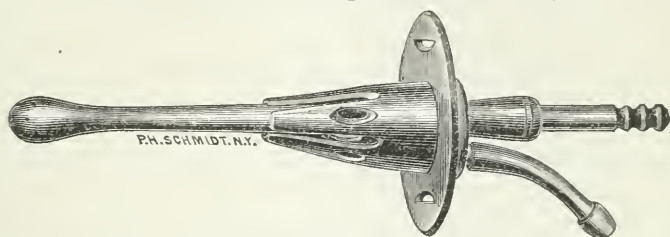
3. An ovum of about five weeks, sent by Dr. John G. Perry, from a lady 22 years of age, who had already had several abortions. The ovum was entire, the placenta just beginning to develop, with hemorrhage into its tissue.

4. Two specimens of meningocele: 1. Abortion at the middle of the third month. There must have been a very early protrusion of the membranes in this case, before the parietal bones had formed. 2. A fetus at term; occipital encephalocele, in the lambdoid suture. The usual place is the small fontanelle, or the region just above the foramen magnum; or, more rarely, near the centre of the occipital bone. The process must be traced back to a meningitis in the cerebral vessel, at the fifth or sixth week.

Dr. MUNDÉ exhibited a

DOUBLE VAGINAL CANULA,

designed to enable the water injected into the vagina, during ordinary vaginal irrigation, to escape without wetting the clothes or bed. It consists of a perforated nozzle, with a tube



returning at the posterior commissure, to which is attached a rubber hose leading into a vessel, while the afferent tube is connected with a fountain syringe or irrigator. The plate closing the vulvar orifice may, in case of deficient approximation and relaxation of the vulva, or in rupture of the perineum, be made larger, and curving slightly backwards at the inferior angle, so as to close the posterior commissure more effectually. The instrument works very well, and is simple and inexpensive. It is the invention of Mathien, of Paris, and was obtained by Dr. Mundé in Stuttgart. Mr. P. H. Schmidt, of this city, has made it of hard rubber, as here presented, an improvement on the original nickel-plate. The central tube can be made to protrude beyond the plate, as in the cut, or be drawn back at will.

Dr. WARD said that Dr. Byrne had presented an instrument, with a soft rubber cup fitting over the vulva, designed for the same purpose, to the Society, some three or four years previously.

Dr. H. T. HANKS showed a

SPECIMEN OF APOPLEXY OF THE LEFT OVARY, FROM A YOUNG GIRL WHO DIED OF CEREBRO-SPINAL MENINGITIS,

and read the following history of the case, which was furnished him by Dr. C. E. Billington, the attending physician:

Minnie McA., aged 14½ years, was first seen on February 2d last. Had been ill since the 30th of January. Had menstruated one week before. Has always been irregular as to the time of the flow. Noticed nothing unusual at this last period; but soon after had slight pains and some tenderness in region of bladder; became nervous and uneasy. Bowels constipated.

Complained of pain in breast and head. Appearance delicate; poorly nourished; chloro-anæmic. Patient very restless; with outcries and gestures of an hysterical nature. Pulse, temperature, pupils, and intelligence normal. Laxatives, iron, quinine, and good food were ordered.

I saw the patient, in consultation, February 6. Bowels not opened. Much tenderness over lower part of abdomen. Bladder distended with urine, which she had not passed for twenty-four hours. Rectum filled with hardened fæces. Slight tendency to stupor. Outcries and ravings more marked when disturbed. Patient was fully roused on making abdominal and vaginal examination. Uterus easily reached, and movable. Nothing detected by touch, more than slight tenderness, and signs of some suffering on pressure upwards and around uterus. Pupils normal. Pulse feeble, but normal in frequency. Temperature normal. One quart of urine was removed, in which no albumen was found.

No satisfactory diagnosis was made. Patient made no complaint of headache, but there was evidence of stupor which causes us to fear a grave prognosis. A brisk purgative was ordered; also injections. They did not take effect, however until thirty-six hours afterwards. About this time ptosis of left eye appeared and paralysis of left side soon followed, which became gradually complete, and death took place on the 11th, nine days after I first saw her, and thirteen days from first attack.

Autopsy, ten hours later, by Drs. Billington and Comstock, witnessed by Drs. Bates, Brennan, and myself. Rigor mortis not marked. In opening abdomen nothing abnormal was discovered, except in the left ovary, which was enlarged about four times its normal size and highly congested; soft and semi-elastic to touch. The congestion had extended along the broad ligament and Fallopian tube on to the peritoneal covering of the fundus uteri. On removing the left ovary, and incising it, a quantity of dark, broken-down, clotted blood escaped (a portion is still retained in ovary for your inspection). A corpus luteum was found both in the left and in the right ovary. The upper and posterior portions of the arachnoid membrane of the brain, and also of the upper portion of the spinal cord, were found highly congested.

DR. PAUL F. MUNDÉ read the following paper on—

THE REPOSITION OF RETRO-DISPLACEMENT OF THE GRAVID
UTERUS BY POSTURE AND ATMOSPHERIC PRESSURE.

Mrs. C. S., twenty-eight years of age, mother of two children—the first premature at seven months, one and a half years ago,

the last at some ten months ago—came to me January 3d last, complaining of a sensation of weight and drooping in the pelvis, hips, and back, which at times increased to severe, almost unbearable pain, especially after prolonged walking or standing. This condition had lasted about a week, and was becoming more distressing every day. Her bowels were very much constipated, moving only by means of cathartics, and each movement aggravated the pelvic and sacral pain; the functions of the bladder were as yet unimpaired, with the exception of a desire for frequent micturition accompanied by scalding and some tenesmus. Her last menstruation had taken place about the middle of October, eleven weeks previously, and she thought herself pregnant. A vaginal examination revealed the uterus in a state of acute retroflexion, the enlarged body and fundus occupying the cavity of the sacrum, and firmly compressing the rectum and the cervix, situated under the symphysis pubis. Below the fundus was the enlarged, exquisitely sensitive left ovary. The size of the uterus, and the soft, doughy feel of the body, left no doubt on my mind of the existence of a pregnancy of about ten weeks; and the tenderness of the organ on pressure clearly showed the necessity for speedy interference and reposition of the dislocation. Although the symptoms of actual incarceration were neither present nor imminent, that condition generally not arising until towards the end of the fourth month, and although the existence of a retroflexion fortunately prevented the compression of the urethra by the cervix uteri, and the consequent partial or total retention of urine, which would doubtless have been present had the case been one of retroversion—still two previous cases of miscarriage during the tenth and twelfth weeks, in which the reposition of the retroverted uterus and a lever pessary after the commencement of metrorrhagia proved unavailing, had shown me the danger of abortion, even at that early period, unless the displacement were soon reduced. I therefore lost no time in placing the patient in the knee-elbow position, and introduced two fingers of my right hand into the posterior cul-de-sac of the vagina—intending to try this, to the patient, less disagreeable and painful method of reposition, before proceeding to the more effective manipulation per rectum. Finding, however that the fundus did not budge, I passed the same two fingers into the rectum, and endeavored for about five minutes to dislodge the body of the uterus, and push it above the brim of the pelvis, but without success. The complaints of the patient, and the fact that her bowels had not moved for several days, to which fact I attributed in a measure the difficult reduction, induced me to desist from my efforts for that day, and to send

her home with directions to thoroughly evacuate the bowels by laxatives and enemata, and to return with an empty bladder on the following day. She did so, and having found the uterus in the same position, I at once again placed her in the position *à la vache*, and renewed my efforts per rectum to push the fundus out of the sacral concavity and above the promontory. After using as much force as I dared for about ten minutes, and causing the patient so much suffering as to require all the influence I had over her to permit me to continue my manipulations, I found that I had not succeeded in elevating the fundus uteri one line from the floor of the pelvis, and that the cervix still retained its original position above the pubic arch. Whatever slight dislodgment was effected by strong upward pressure, the hand at the same time pushing up the perineum, was at once annulled when the pressure ceased, the fundus instantaneously rebounding to its place in the sacral excavation.

Having, in a number of cases of retroflexion or version of the gravid uterus, succeeded, without difficulty, in replacing the organ in the manner indicated, I thought that there must be some special reason for my want of success in this case—perhaps adhesions, which were possible, since the patient could not date the displacement to any sudden accident; and it might therefore have existed for some time before the present impregnation took place. I determined, however, to try first, whether, by drawing the cervix away from the pubis and towards the floor of the pelvis, I could not dislodge the fundus and reduce the dislocation by simultaneous digital pressure per rectum. This I had already tried with the finger, but without avail. The patient still being in the knee-elbow position, I introduced Sims' speculum into the vagina, and pulled up the perineum sharply, intending to seize the cervix with the double tenaculum, when I suddenly noticed that the vagina was distended with air like a balloon, in the middle of which appeared the cervix. On looking for the body of the uterus, I found, to my surprise, that it had disappeared, that the sacral excavation was empty, and that the obstinate retroflexion of the gravid organ had been unwittingly and painlessly reduced. The patient immediately expressed her sudden and entire relief from the previous distressing symptoms, and I hastened to secure the uterus by introducing a proper Albert Smith pessary, which the patient has since worn with perfect satisfaction and comfort. [It was removed at the beginning of the fifth month.]

The explanation of this phenomenon is perfectly simple and obvious: The position of the patient produced a slipping of the moveable abdominal viscera away from the pelvis, and a

suspension of the intra-abdominal pressure, or *vis-à-tergo*, its place being indeed supplied by a greater or lesser amount of suction or traction away from the pelvic organs, a certain *vis-a-fronte*, so to speak. The forcible elevation of the perineum opened the introitus vaginæ, and gave entrance to a volume of air, the pressure of which had already been pushing up the perineum, slightly drawn inwards by the downward gravitation of the abdominal viscera, and the pressure of which, when admitted, instantaneously distended the vaginal pouch, and replaced the uterus—a mechanism identical with that on which the action of Sims' speculum is founded. Thinking the matter over, it occurred to me that it was a case of acute (sudden) retroflexion of the non-impregnated uterus which afforded Sims the occasion for discovering his invaluable speculum. The unexpected reposition of the retroflexed uterus by the distension of the vagina with air, which had rushed into the canal under the volar surface of his two fingers, while he was endeavoring, in the knee-elbow position, to lift up the fundus, first gave him, as we all know, the idea of the duck-bill speculum. But I did not remember seeing this method of reposition of the retro-displaced uterus, gravid or unimpregnated, recommended in any of the works on obstetrics or diseases of women with which I am familiar. Could it be possible that, among the great benefits which this lucky accident has conferred on humanity, this smaller but still estimable benefit had been overlooked? It seemed scarcely probable. However, on looking over all the modern works on obstetrics and gynecology at my disposal, such as Scanzoni, Schroeder, Byford, Cazeaux, Leishman, Thomas, Barnes, Hewitt, I found that, while all recommended the usual manipulations for the reduction of retroflexions and retroversions of the unimpregnated and gravid uterus, as by fingers in the rectum or vagina, or by air or water bags in these passages (Favrot); or by pressing the fundus up with a drumstick or ivory-headed cane in the rectum (Byford); or by drawing down the cervix with one hand, while the other pushes up the fundus, the patient generally being in the knee-elbow or semi-prone position; and, while all these authors agree that the reposition of the uterus with the sound is always attended with more or less danger and pain, still not one even as much as hints at the employment of atmospheric pressure for this purpose. Even Sims himself, whose attention might naturally have been supposed to be directed to this manner of replacing retro-deviations, from the case mentioned above, entirely ignores the minor lesson taught him by that case, and recommends the reposition of the retro-displaced uterus by the fingers and three sponge-holders, or by the uterine elevator.

During a discussion, likewise, on a paper by Dr. Gervis, on "Retroversion of the Gravid Uterus," in the London Obstetrical Society, at its meetings in November and December, 1874 (*Obst. Jour. Gr. Br. & Irel.*, Dec., '74, and Jan., '75), which is probably the latest published general discussion on the subject, Drs. Barnes, Wynn Williams, Aveling, Galabin, Braxton Hicks, Palfrey, Godson, Edis, Hayes, and others, related their experience in forty-eight cases, eight of which were fatal, and the treatment employed and advocated by them; but not one word of air-pressure as a repositor do I find among all the various methods recommended. It is evident that none of the gentlemen named had ever heard of it in that connection.

On January 6th, that is, two days after the reposition of the displacement, as above described, the latest number of the Berlin *Beiträge zur Geburtshülfe und Gynäkologie* (Vol. IV., No. 1), came into my hands; and, looking it over, I noticed an article, entitled "A Hitherto unrecognized Obstacle to the Reposition of the Retroflexed Gravid Uterus," by Dr. Solger of Berlin, read before the Berlin Obstetrical Society, May 11, 1875, in which the author, after enumerating the various well-known obstacles to the reposition of the retro-displaced impregnated organ, such as distension of the bladder and rectum, projection of the sacral promontory and impaction of the fundus in the sacral excavation, and retro-uterine adhesions, mentions a new, hitherto not recognized impediment, viz., the normal intra-abdominal pressure, and relates his experience in a case of difficult reposition, in which he made exactly the same observation as now reported by me, although in a slightly different manner, identical with the original observation of Dr. Sims. In one case of irreducible retroflexion of the gravid uterus, at the end of the fourth month, in which Solger vainly endeavored to replace the organ by the fingers and the *colpeurynter*, in the knee-elbow position, he seized the cervix with a double tenaculum, to draw it away from the pubis, and, while examining with the finger, to see whether the tenaculum was in the right place, the cervix and tenaculum suddenly made a spontaneous evolution, and the cervix was found high up in the sacral excavation, from which the fundus had disappeared. After some deliberation, Solger finally concluded that this voluntary reduction was owing to the influence of the negative intra-abdominal pressure in the knee-elbow position, and was reminded of a case of severe incarceration, in which violent emesis, in the prone and knee-elbow positions, brought about the spontaneous replacement of the uterus. Still, he was not completely satisfied with this explanation, and it was not till about a month previous, that a new case afforded him

the opportunity for ascertaining the true rationale of this phenomenon. After repeated unsuccessful attempts to replace the retroflexed uterus of $3\frac{1}{2}$ months, in the knee-elbow position, with the fingers in the rectum and vagina, Solger again introduced his fingers into the vagina, for the purpose of drawing the cervix away from the symphysis, when he heard and felt the air rush into the vagina between his fingers, which at once found themselves in a large balloon-like space, bounded above and behind by the sacrum. The retroflexion was completely replaced! This, to him (as to me) unexpected and surprising result, Solger attributes to the overcoming of the intra-abdominal pressure (equal to at least 100 pounds) by the atmospheric pressure (which, taking the antero-posterior diameter of the superior pelvic strait only as high as 8 cm., or 3", at 15 lb. to the square inch, amounts to more than 100 pounds), aided by a negative intra-abdominal pressure, not exceeding, according to Schatz, 10 cm. hydraulic pressure, and the weight of the uterus itself. Solger recommends to replace all retroflexions of the gravid uterus by placing the woman in one of the positions which annul abdominal pressure (the other obstacles resulting from distension of the bladder and rectum having been removed), and then lifting up the lower two-thirds of the posterior vaginal wall with one or two fingers, so as to permit the free ingress of air to the vagina. Only in case of this manipulation failing to be successful, is the employment of manual or instrumental pressure justifiable.

Several of the members of the Berlin Obstetrical Society, such as Drs. Louis Mayer, Körte, and Von Haselberg, expressed their doubts as to whether the mere pressure of air in the vagina could replace a retroflexed gravid uterus with greater facility than could be done with the fingers; and the last-named gentleman referred to several cases observed by Martin, Fuhrmann and himself (*Monatschr. für Geb. u. Frauenkrankh.*, 34, p. 173), in which air entered into the vagina in the dorsal decubitus, in which position the idea of traction on the vagina by negative intra-abdominal pressure must be excluded. He was therefore inclined to lay less stress on the influence of intra-abdominal pressure as an obstacle to reposition, than of the projecting promontory.

These doubts as to the power of air-pressure in the vagina certainly confirm the impression I had gathered touching the ignorance or want of appreciation of the profession of this simple, but nevertheless powerful instrumentality.

As regards Dr. Von Hasselberg's objection, it is refuted by Dr. Adolph Rasch, in a paper on "Air in the Vagina" (*Trans.*

Lond. Obstet. Soc., vol. xii, 1871), the conclusions of which read as follows :

"1. No air enters the vagina of a female placed on her back.
 "2. In the prone position, the abdominal walls fall outward, and cause a diminished pressure in that cavity. If the vaginal orifice be open, air will enter and so compress the expanded intestinal gases to their previous volume.

"3. The force with which it enters and consequently the quantity which distends the vagina, varies with the resistance offered by the abdominal walls to the gravitation and the degree of mobility of the viscera.

"4. In replacing the female on her back, the abdominal contents fall inwards and expel the air again from the vagina.

"5. In the position on the back we have an efficient means of keeping the air out of the vagina and uterus, and so preventing the deleterious consequences ascribed to its action on the vaginal and uterine contents."

Dr. Rasch also denies the power of sucking up the air by spasmodic contraction ascribed to the vagina by Hadley, Hewitt, Routh, Braxton Hicks, and others, and shows that the abdominal cavity also possesses no suction power. The incorrect observations of the gentlemen mentioned must be attributed to a lack of appreciation of the time when the air entered the vagina, which certainly must have taken place at some previous time in some other position than on the back, and was not expelled until she occupied the latter position.

While looking, a few days ago, over the numerous periodicals accumulated in my library during the past year, for the purpose of preparing my annual report on Gynecology, I came across another paper bearing on my subject, entitled "Position, Pneumatic Pressure, and Mechanical Appliance in Uterine Displacements," by Dr. Henry F. Campbell, of Augusta, Ga. (*Atlanta Med. and Surg. Jour.*, May, 1875), which I remembered receiving, but had laid aside unread for a future occasion. This paper may have met the eyes of some of the gentlemen present, and a detailed repetition of its contents is therefore unnecessary ; still a brief review of the main features of Dr. Campbell's extensive experience on this topic may not prove uninteresting or tedious.

He says that although the *knee-and-breast* (not knee-elbow) posture has been known and practised in uterine displacement for many years, it is still but little appreciated ; and only two modern works—Thomas's and Barnes's recommend it as a means for reducing uterine dislocations. Not one mentions "the indispensable condition of power and the real instrumentality and *sine qua non* in the process of replacement," the *pneu-*

matic pressure, without which the posture alone is almost useless. He recommends the employment of the knee-breast position and pneumatic pressure together, in all varieties of uterine displacement, not only to aid the diagnosis and replace the dislocated uterus, preparatory to introducing a pessary in the same position, when it can be gently laid on the posterior vaginal wall without forcibly pushing up the displaced organ (the only way in which a pessary should be applied), but chiefly for the purpose of enabling the patient to reduce the dislocation herself every evening before retiring to bed, and thus secure an unimpeded uterine circulation with unstretched uterine ligaments during the whole night, a process which, if regularly repeated for some time, will, he asserts, "go far in favoring a restoration to a permanently normal position of the organ." This self-replacement is rendered practicable by means of the "pneumatic self-repositor," a glass tube with slightly curved bulbous extremity, made of different sizes, which the patient introduces every night in the knee-breast posture, only for a moment, when "the air rushes in, the suction is broken, and immediately, whatever may be the displacement, unless there is adhesion or impaction, self-replacement is completely and instantly accomplished." Sims' speculum was originally used, but proved too expensive and inconvenient to the patient. Dr. Campbell also advises the postural and pneumatic-pressure treatment in the various forms of displacement of the gravid uterus, which "are not only incident to but are almost normal attendants of the earlier months of pregnancy ; and is confident that many of the discomforts and dangers accompanying these conditions will be alleviated or removed by "nightly self-replacement." *Rectal inflation* has also been employed by Dr. C., and is recommended as serviceable in some cases "to dislodge the fundus from the hollow of the sacrum, thereby making restitution by vaginal inflation and inverted gravity easier and more certain." A case confirming this conviction of Dr. Campbell's is reported by Dr. Aveling in the *Obst. Jour. of Grt. Br. & Ire.* for Jan. 1875, in his article on "The Influence of Posture on Women," of a lady afflicted with retroversion, who found her suffering alleviated whenever she knelt with bowed head at her confessional.

A very important, distressing, and peculiarly obstinate class of cases, in which postural treatment alone is of marked benefit (Rigby, *vide* Aveling l. c.) are those in which one or both of the normal or enlarged and congested ovaries have slipped down behind the uterus, and produce the most agonizing torture at almost every movement of the patient. Dr. Campbell entirely omits all mention of these displacements, although they are

very common in connection with those of the uterus. I am confident that the knee-breast position, particularly if reinforced by atmospheric pressure, will prove very soothing and beneficial in these cases.

While Dr. Solger's observation differs from mine in that he did not recognize the advantage offered him by the employment of Sims' speculum for the admission of air to the vagina (and this is by no means a mere nominal advantage, as I have recently ascertained, while experimenting on a number of patients, for it seems almost essential to a complete distention of the vagina to lift up the perineum, which latter, I ought to state, should be lax and distensible, as in multiparæ, with a gaping vulva), it will be seen that Dr. Campbell has covered all the ground (with one exception) and infinitely more than I had expected to occupy when I first made the independent observation which forms the basis of this communication. I must say that, having since replaced a number of retroverted non-gravid uteri by the postural and atmospheric-pressure method, I am very much impressed with the truth and force of Dr. Campbell's statement; and, while the subject of "nightly self-replacement" may perhaps meet with some opposition on æsthetical and moral grounds, I do not hesitate to express my unqualified support of the principles of treatment advocated by Dr. Campbell, and my belief that its universal appreciation and adoption will be of great benefit to the suffering female sex. My object in preparing this short paper is to call attention to the value of *atmospheric pressure* conjointly with the old-established postural treatment in reduction of retro-displacements of the gravid uterus, even of a severe degree, where the employment of an amount of force such as would be perfectly justifiable in case of adhesion of the unimpregnated womb, would surely be productive of metritis or abortion.

This particular class of cases is not referred to by Dr. Campbell; indeed he excepts displacements, where there is adhesion and *impaction*, as not amenable to the beneficial influence of posture and air-pressure. Solger's and my cases both prove that the latter condition in its commencing stages, may readily be relieved by this method. I need scarcely mention, therefore, that all the milder cases of this accident are, as a matter of course, controlled with correspondingly greater facility; and that I entirely concur with the advice given by Dr. Solger, that only after posture and atmospheric pressure (employed in the manner described by me—knee-breast position and elevation of perineum with Sims' speculum)—have failed to reduce the dislocation, are the usual infinitely more difficult and painful manipulations to be resorted to.

Dr. PEASLEE said that he thought the power of air to replace a displaced uterus was liable to be overrated. The air will replace the uterus only as far as the vagina reaches, and a retroversion will then be found reduced from one of the third to one of the second, perhaps from one of a second to a first degree; but the sound will still be felt to pass into the uterus, with its concavity turned slightly backward. He does not think that even a pessary will change a retroflexion into an antelexion; only the sound or repositor will do that.

Dr. CHAMBERLAIN made the following remarks on the subject:

Mr. PRESIDENT: I would like to ask Dr. Peaslee if, provided the vagina be sufficiently extensible, the pressure of an instrument in the commissure behind the uterine neck, applying, as it does, upon the lower portion of the fundal zone, must not carry the fundus, first, against the cavity of the sacrum, and, if further continued, upward along the curve of the sacrum, until it emerges from the true pelvis in a perpendicular to the plane of the brim. Now, this perpendicular is substantially the axis of the superior strait, and it is in that axis that the normal position of the uterus lies. That the pressure of which I speak will compel the uterus to assume this position I have several times verified upon the cadaver.

If Dr. Peaslee's statement be that the uterus cannot be positively antelexed, except by an instrument within its cavity, I may say that I think that, when sustained above the brim of the pelvis and prevented from retroverting, the combined influence of the round ligaments, the weight of the organ and the pressure of the viscera will sometimes positively antevert it; but, at any rate, I have not doubted that substantial restoration to its natural position can be attained by a pressure from below and without, and that the knee-and-breast position is a most valuable aid to restoration.

Dr. LEE asked Dr. Peaslee, whether there is not danger from replacing an endometritic uterus with the sound?

Dr. PEASLEE said that he had never had any trouble, unless pelvic adhesions were still or had recently been present. He thought the sound could generally be used without danger in such cases.

TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

Reported by W. H. H. GITHENS, M.D., Secretary.

Stated Meeting, September 2d, 1875.

The President, DR. A. H. SMITH, in the Chair.

VERSION IN CONTRACTED PELVES.

By DR. ELLWOOD WILSON.

PAPER III.

(Continued from April No.)

BARNES writes that the perineum is almost always nicked in first labors, and I think he is correct so far as the vaginal margin is concerned. Tyler Smith ("On Parturition," p. 24), says, "Laceration of the perineum is no uncommon accident in midwifery. In most cases it begins at the vaginal margin of the perineum, extending towards the anus, and sometimes throwing the cavities into one. It is generally admitted that the laceration of the perineum occurs from the head of the child passing through the vagina so rapidly that the perineum has not sufficient time to dilate." Meigs, referring to the management of the perineum in forceps cases, says, "By rude and untemperized exercise of strength we incur great hazard of rupturing these organs, and of maiming the patient *most injuriously*. An operator ought to be turned out-of-doors as soon as he allows *so scandalous a misdemeanor* to occur from carelessness or ignorance."

Dr. A. R. Edis (*Obstetrical Journal of Great Britain and Ireland*) gives a case in which the perineum was ruptured through the sphincter ani, involving the recto-vaginal septum for at least one and a half to two inches. The forceps was not used. I have seen a number of cases in which this accident was said to have occurred without forceps. Quite recently I saw a case of twenty years' standing which occurred in a first

labor without instruments. Though Dr. Goodell can recall but a single case in his experience in which the perineum was *at all torn* by the after-coming head, the results of such labors prove that such an accident is liable to occur. It is true that in many instances the rent will unite by proper surgical treatment; but certainly there are a sufficient number of cases in which union does not take place to make the accident a matter of too much importance to be regarded with complacency.

Dr. Goodell says, "I should have no misgivings about causing such a lesion were it necessary thereby to save the life of the child." Such a condition or necessity is not likely to arise; because in forceps labors, if the instruments are used as recommended by most modern authors,—that is, as soon as the woman has shown that she is incapable of accomplishing her task, and the tissues are dilated,—there will be no necessity for hurrying the delivery when the head is found to rest upon the perineum, since the foetal circulation will not have been materially interfered with; nor will the compression of the foetal head have been of long duration. Therefore, the few minutes that may be required for the relaxation of the perineal tissues will prove of no detriment to the child; whilst, if the expulsive efforts of the mother threaten this structure, the instrument gives us the best possible means of counteracting this undue force.

In pelvic labors, if the child is not destroyed before the head reaches the perineum, the increased amount of dilatation required for the delivery of the head after the breech and shoulders are born will be but very slight, and will soon occur. Meigs informs us that he has been able to keep the child alive after its body was born, and the head undelivered, for nearly twenty minutes. Simpson gives a like experience. Barnes declares he has for ten minutes. Pugh claims that the child may be kept alive for half an hour or an hour before the head is fully born. This is accomplished by a very simple expedient, familiar to the profession for a century; therefore in neither cephalic or pelvic labors properly managed can the necessity possibly arise for rudely and wantonly dragging the child's head through the perineum in the risk of maiming the mother, and thereby rendering her a wretched sufferer for the balance of her life. Dr. W. T. Smith's statement, as quoted by Dr. Goodell, that "it is better in any case that the perineum should be torn by the foetal head or by the forceps than that a living child should be killed," is true. But, happily, in the present state of our art, we are not limited to these alternatives. We hope to save the life of the child and the integrity of the ma-

ternal tissues at the same time, in an immense majority of our cases.

Dr. Goodell, in his Reply, regards the possibility of contusion of the soft parts, followed by inflammation, sloughings, etc., and resulting in vesico- or recto-vaginal fistulæ or septicæmia, or even the death of the patient in version cases, as "mere assumptions."

Hodge says, referring to the difficulties of making version, "Even should this be effected without the contusion or laceration of the uterus, the subsequent dangers to the mother from the child being rapidly and forcibly drawn through the contracted apertures of the pelvis are of the most serious import; contusions, lacerations, inflammations, and mortifications of the os uteri, vagina, bladder, or rectum, have not unfrequently resulted" (p. 405). Barnes also alludes to these sequences of version (*loc. cit.*, p. 404).

Let me call your attention to some statistics:

First. W. H. Jones (*Management of Labor in Contracted Pelvis*, 1867) gives the following analysis of cases:

(a) 16 cases of labor, pelvis $3\frac{1}{2}$ inches.

6 cases terminated spontaneously.

1 child died.

7 cases by forceps.

3 mothers died; 2 children died.

1 by version (transverse presentation) followed by detrunca-
tion and cephalotripsy.

2 by craniotomy.

(b) 15 cases of labor, pelvis $3\frac{1}{2}$ to 3 inches.

2 terminated spontaneously; 2 mothers and 2 children saved.

1 terminated spontaneously (after uterine douche); mother
and child saved.

2 terminated by forceps; favorable to both mothers; 1 child
died.

1 terminated by spontaneous version; mother recovered;
child died.

2 terminated by turning (transverse presentation); both
mothers saved; 1 child died.

7 terminated by cephalotribe.

(c) 11 cases of labor, with pelvis 3 to $2\frac{1}{2}$ inches.

2 terminated by forceps; 1 mother died; 2 children saved.

1 forceps were applied, but the labor was terminated by cra-
niotomy; mother recovered.

1 turning was performed (transverse presentation); the
labor was terminated by detrunca-
tion and cephalotribe.

1 breech presentation; detrunca-
tion was effected and cepha-
lotribe was used; mother lived.

4 by craniotomy and cephalotribe; mothers lived.
2 cases premature labor was induced (by uterine douches); both mothers lived; children were dead.

(d) 9 cases with pelvis $2\frac{1}{2}$ inches.

8 of these cases were terminated by cephalotripsy.

5 cases, cephalotribe directly applied to the child's head; 2 mothers saved.

2 cases of breech presentation; detruncation was followed by cephalotripsy. 1 mother died.

1 case, cephalotripsy followed by version; the woman died immediately.

1 case by Cæsarean section; mother died; *child lived*.

P. 33. In two cases where the pelvis was narrowed to a little more than $2\frac{1}{2}$ inches, the forceps afforded the means of extracting alive two children at full term. This table of statistics gives us also five cases of detruncation of the child. There is no evidence showing that more than one person made traction upon the child's body.

Second. The obstetrical history of two women. Read before the Obstetrical Society of Edinburgh by Dr. Angus Macdonald.

Case 1st, aged 41 years; pelvis contracted to close upon 3 inches; had given birth to 9 children at term; to 2 at $7\frac{1}{2}$ and 8 months; and had had three abortions.

1st labor terminated unaided; three days' duration; child living.

2d labor terminated by version, after failing with the forceps; child living; a female.

3d labor, by turning; male child; still-born.

4th labor, by turning; female child; living.

5th labor, unaided; at about 7 months; child lived nine hours.

6th labor, induced at 8 months; terminated without instruments or turning; child living.

7th labor, by turning; female child; living.

8th labor, by turning; female child; living.

9th labor, by turning; male child; still-born.

10th labor, unaided; male child; made some efforts to breathe, then died.

11th labor, by turning; female child; living.

Case 2d, aged 25. The sacral promontory was very easily reached, and the whole pelvis small.

1st labor, version was made; it required great force for half an hour, before the head cleared the pelvis; the cervical verte bræ had yielded during the traction.

2d labor, induced at $7\frac{1}{2}$ months; delivery by forceps; child was born alive, but died in six hours.

3d labor, at full term was delivered by forceps; child, a female; living.

Dr. Macdonald goes on to say:

"The cases in which turning is found to succeed best are such as my first case, in which we found, on the whole, that it did very well. But the great majority of such cases are made up of second and subsequent cases, which proved in the primiparous condition excessively difficult, and led to craniotomy and other serious results, as it is seldom in a first case that a difficulty suitable to be overcome by turning is proved to be so in time to allow of this operation being performed. These cases, however, are found to terminate just as easily by the application of forceps, if not without any interference at all. It is quite clear, on reading Schroeder's note after his paragraph on turning in deformed pelves, that his views are arrived at chiefly from cases of great difficulty in first labors proving easy in subsequent labors when turning was employed. But I hold that this reasoning is purely the *post hoc* fallacy. In the earlier part of my practice I turned much more frequently than now in such cases, and on the whole got fairly good results. Now I interfere much less readily, and find the results far better. I am not, however, inclined to hold that turning is such a purely harmless operation for the mother as Schroeder and others maintain. He further states that in his experience turning presents no advantage to the mother over the long forceps."

In his remarks upon these cases, J. M. Duncan stated that a number of cases diagnosed as cases of great contraction of the brim have subsequently come under his care, and that "in almost all of them he could find no good evidence of the contraction."

Dr. W. Y. Martin, in a recent number of the *Obstetrical Journal of Great Britain and Ireland*, is my authority for the following figures:

Of 56 women delivered by forceps, 26 were primipara and 30 pluripara. Mothers all lived. 35 children were males, 18 females. 3 children were dead; one of these was putrid. Omitting the one that was putrid, one child in $27\frac{1}{2}$ died as the result of the operation.

In 20 of these cases, or nearly one-half, the forceps were used in consequence of contracted pelves.

Of 42 cases where turning was resorted to, 37 were pluripara and 5 primipara. Mothers all survived the operation. 30 male and 12 female children were delivered. 18 were dead-born, 8 were putrid. Omitting the 8 cases where the children were putrid, the mortality is one in three and two-fifths.

In 9 of these cases, or about one-fifth, version was resorted to in consequence of contracted pelves. Thus, in the table of for-

ceps cases, where nearly one-half were contracted pelves, the mortality to the child was one in $27\frac{1}{2}$. In version cases, in one-fifth of which the pelvis was contracted, the mortality to the child was one in two and three-fifths.

In the *Philadelphia Medical Times*, August 14, 1875, Dr. George Francis reports three hundred cases of labor which were completed within two hours after dilatation of the os, and in which the forceps were used fifty-one times, or once in six cases. There were 301 children born. All but 4 lived.

Three of the mothers died. One of these was at the point of death when first seen, probably from pulmonary thrombosis; and the child, being nearly born, was safely extracted at the very moment its mother died. Another had erysipelas when labor set in, and died of blood-poisoning. The third was apparently exhausted by a very long first stage, in which no interference was permitted. By the time the os was dilated she was in high fever. There have been no deaths in the 179 women last delivered. While it is true that the forceps was used in all three fatal cases, he sees no reason to think that the operation affected the result in either of them. After delivery the perineum was examined, and in but one case—which, by the way, was not instrumental—was there found sufficient tearing to need the stitch. The application of the instrument rarely gave any more pain than the previous digital examination, and never required anæsthesia. The catheter was not once required, either during or after confinement.

Stated Meeting, Thursday, October 7th, 1875.

The President, DR. A. H. SMITH, in the Chair.

DR. ELLWOOD WILSON concluded his paper on

VERSION IN CONTRACTED PELVES.

PAPER IV.

In the Reply, Dr. Goodell denies that Dr. Dewees applied the forceps to the sides of the child's head, at the brim. I refer to page 294 of "Dewees' System of Midwifery," where we may read as follows:

"The blades of the forceps are always to be applied to the sides of the head,—that is, over the ears of the child. When

necessity (which is very rare) obliges us to depart from this rule, it is but an exception to the rule."

To show that he did not regard it as a dangerous instrument, when skilfully used, let me quote from the foot-note on page 280:

"I am convinced that if the forceps be judiciously employed, the lives of very many children may be saved, and that the death of the mother is a rare occurrence."

Prof. Davis writes: "In my own practice, as one of the physicians to the Maternity Charity of London, I have the satisfaction of being able to assert that I have never incurred the mortification of losing a mother in consequence of a forceps operation."

Let me now call your attention to a remarkable fact. Not one of the authorities mentioned by Dr. Goodell as recommending version as a means of delivery in contracted pelves applied the forceps to the sides of the child's head, at the brim. On the contrary, they applied the instrument over the face and occiput of the child, using forcible and rapid compression, most of them declaring that when the forceps was used the case must be terminated in half an hour, or an hour at most; and they did not resort to the use of the instrument until the woman had been many hours in labor, and was approaching to or was already in a state of exhaustion. It is no wonder, then, that the result of such delays and of such modes of using the instrument were frequently fatal to both mother and child.

I will here give the history of Mrs. G. Four feet and five inches in height; weight one hundred and ten pounds. Her pelvis, I think, is not more than three inches in the antero-posterior diameter. In making the touch, the finger is easily brought in contact with the promontory, and can follow the inner surface of the sacrum to the point of the coccyx. Her first labor occurred July 16, 1872. Vertex presented to the right ilium posteriorly. The forceps were applied to the sides of the head without difficulty, and in the descent the vertex rotated anteriorly. Child a male; labor lasted 48 hours; was still-born. Second labor, February, 1874. Vertex to the left ilium; forceps to the sides of the head; female child; living. Third labor, Sept. 6, 1875. Vertex to left ilium; forceps to sides of head; labor 8 hours in duration; male, still-born; weighed 8 pounds. After the forceps was adjusted the head descended into the excavation with the second pain. Moderate traction, coincident with uterine contraction, was continued for about twenty minutes, when the head was born. The shoulders were found to be fixed at the superior strait, and were

brought down with difficulty. I attributed the fatal result to the child to the traction (moderate,—not more than 30 or 35 pounds,—and strictly in coincidence with uterine contraction) with the forceps before the shoulders had descended into the excavation.

I now ask your attention to another very important argument brought forward in favor of version, viz., the tensile strength of the child's neck.

Dr. Goodell quotes (Clinical Memoir, p. 17) Joulin as asserting that a force varying from 113 pounds to 225 pounds may be brought to bear upon the child's head by means of the forceps, and ventures the opinion, "That if the last-estimated force be prolonged, it is hardly ever compatible with the life of the child or with the integrity of the mother's tissues;" adding, that "the conclusion is inevitable that by the conjoint use of two very nearly equal forces,—viz., that of supra-pubic pressure by the hands of an assistant, and that of traction on the body of the child—by the physician, there can be *safely* brought to bear upon the hind-coming head an extractive force fully as great as that by the forceps on the fore-coming head."

That a force of 225 pounds may be brought to bear upon the child's head by the forceps is a possible problem; and as Dr. Goodell positively asserts that he and an assistant, by the conjoint use of supra-pubic pressure and traction on the body of the child, have together exerted a force nearly equal to this, "certainly not less than 200 pounds," it does not become me to question the statement; but that it can be done with safety to the child I am incapable of believing. He then refers to Meigs, as having thrown his greatest strength upon the head of a child which had been perforated without breaking its neck; and Dr. R. Stewart, who twisted a towel around the neck of the fœtus, by means of which he and an assistant used their utmost traction-force—certainly more than 200 pounds—upon the neck without breaking it; but he does not give the history of the case in which Dr. Stewart, by his unaided efforts, did break the neck of a child, or that of Dr. J. Eshleman, who declared in this hall that he had broken the neck of a dead child with a traction-force of less than 100 pounds.

Dr. Goodell also quotes Hodge thus:

"It would be very easy from the records of the profession to detail the terrible results of forcible traction upon the limbs and body of the child when such traction was made not at the proper time or in the proper direction,—when brute force has been substituted for art and science. Perhaps there are but few physicians who have not seen or heard of cases where the

strength of two individuals" (Dr. Hodge does not call them physicians) "has been applied to the body of the child, while that of three, four, or even more attendants to the body of the mother to accomplish the delivery of the head after the body has been born. That the child should ever escape with its life under such circumstances is surprising." "Thus," Dr. Goodell remarks, "six, and even more persons pulling away at mother and child, and yet, as Hodge clearly implies, the child has escaped with its life." Now, gentlemen, if by this quotation Dr. Goodell has led you to believe that this great conservator of orthodox obstetrics sanctioned, recommended, or in any way countenanced such a proceeding as just narrated, let me give you the closing paragraph of Dr. Hodge's statement. It is this: "But it is not to be wondered at that the spinal marrow has been fatally injured, that the spine itself has been dislocated or even fractured, that the limbs have been torn off, or that in some unfortunate cases the body of the child has indeed been delivered, but the head remained impacted in the strait of the pelvis. Surely, in the present state of our science, such practice is exceedingly reprehensible, not to say positively criminal."

Dr. Goodell also suggests that "the harrowing scenes at the scaffold when the rope snaps, or the prolonged struggle when the neck is unbroken, show conclusively that no *amount of traction, per se*, without twisting will destroy life unless the spinal column yields."

Notwithstanding this pleasing picture, and the comforting assurance of Dr. Goodell that the neck may not break, I believe few men receive the noose with pleasure. To further prove the tensile strength of the child's neck, he says (*Med. Times* March 20, 1875): "I once saw the strength of the child's neck put to a crucial test, and the result amazed me. I frankly confess that had I not been an eye-witness I should have been a doubter. It was a case of primipara with a flat pelvis and a large but putrid fœtus. After craniotomy had been performed, a further obstruction to delivery lay in the bloated chest and belly. Before this second complication was recognized, each one of four physicians present, including myself, took *his turn* at the forceps. From a natural rivalry, the traction thus necessarily made upon the neck of the fœtus by three of us in succession was no child's play. But that made by the fourth gentleman, a distinguished member of this society, exhibited so much power and originality that I shall here describe it. He turned the woman over on her side, brought her hips to the edge of the bed, and applied Hodge's forceps. He next carefully tucked a sheet around the back of the instrument, removed his

shoes, sat in a chair, and placed one foot across the perineum, the other across the vulva. He then grasped the handles, straightened out his body, and pulled with all his might and main, making every muscle of his body quiver with the effort. Yet, in spite of the enormous strain brought to bear upon the neck of the child, it was simply lengthened out, but not broken." The fate of the child in this case was defined,—that of the mother was not given. It is not surprising that Dr. Goodell required ocular demonstration to credit so revolting a scene.

I hope he does not recommend this as a rule in practice. I also hope the case narrated may have a *lasting isolation* in the annals of clinical obstetrics. Dr. Goodell also says he has repeatedly put a force of 130 lbs. upon the neck of an unborn child without injury to it, and that he put this force upon the neck of a child at eight months and four days of gestation, and brought it away alive.

J. Matthews Duncan ("Mechanism of Natural and Morbid Parturition," p. 86) asserts that "the great majority of labors are completed by a propelling force not exceeding 40 lbs., and that in very easy labors, the mother being in a favorable position, the weight of the child is enough to bring it into the world." He says: "I do not deny that in *very rare cases* a force of 100 lbs. may be produced, but I am sure that it is nearer the truth to estimate the *maximum* expulsive power of labor (including with the uterine contractions the assistant expulsive effort) as not exceeding 80 lbs." (p. 136 and 137).

Let me read you the results of his experiments to ascertain the power of resistance of the child's neck:

Case 1. A fresh female fœtus weighing 5 lbs. 6 oz., length 18 inches; spinal column gave way at 90 lbs.; decapitation took place at 118 lbs.

Case 2. A fresh female fœtus weighing 7 lbs. 7 oz., length 22 inches; spinal column gave way at 120 lbs.; decapitation took place at 141 lbs.

Case 3. A fresh female fœtus weighing 8 lb. 15 oz., length 23 inches; spinal column gave way at 122 lbs.; decapitation took place at 136 lbs.

Case 4. A fresh female fœtus weighing 5 lbs. 12 oz., length 21 inches; spinal column gave way at 91 lbs.; decapitation took place at 91 lbs.

The average dissevering or decapitation force in these experiments was about 120 lbs. He says if we are to avoid premature decapitation, the force should not exceed 100 lbs.

Dr. Cleveland has given me the particulars of the following case: Barbara II., a German woman, dwarfish in stature, aged

26, stout, rachitic, fell in labor at term, Jan. 17, 1868, of her first pregnancy. The vertex presented to the right ilium; the head rested above the brim; cord prolapsed; patient was placed in the knee-elbow position. With the escape of the amniotic fluid the cord again dropped down; the os was fully dilated; version and extraction of the fœtus were at once attempted. There was some little delay in bringing the head down, and it was accompanied with a *snapping* sound, distinctly audible to the bystanders. The child was still-born; laceration had occurred between the third and fourth cervical vertebræ; a force much less than 100 lbs. was used in this case; the pelvis was but slightly contracted, as the promontory could not be touched with the index-finger.

I again refer to the previously-given table of W. H. Jones, which gives five cases of decapitation, and to Dr. Goodell (Clin. Mem.), in which he states that he has succeeded in breaking the neck of the child. In the Reply, Dr. Goodell quotes as follows: "J. M. Duncan has shown that after the neck of an infant has been broken it takes from fifteen to twenty pounds more before the head parts from the body. Accepting these data, the conclusion is logical that a living child may be born if the traction power has not reached to within fifteen or twenty pounds of the decapitating limit." He here makes the bold assertion that we may make a traction upon the child's neck up to the luxation point, and yet not destroy the child.

In the *Philada. Med. Times*, March, 1875, he uses the following language: "Far better it is in these emergencies to *kill* in attempting to save than to *kill* by cowardly inaction." This is an expression I very much regret to see in a medical journal. There are probably few physicians that have not been appealed to to terminate the suffering of some hopelessly ill and suffering creature; and it may be possible physicians have sometimes done so. Quite recently the fashionable town was all agog because a physician said to the wife of his patient, "Madam, your husband is dying; let me give him a hypodermic and end his suffering." The wife, horrified at the suggestion, sent for another physician, and the gentleman lived for several months. This appeal has been made to me several times. The fashionable woman who does not wish a larger family will say, when pregnant, "Doctor, if you don't relieve me, I will kill myself." The poor wretch who, deceived, dishonored, abandoned, stricken with poverty and impending starvation, destroys her infant (upon the apparently humane idea of saving it from the more lingering, but no less certain, starvation), is condemned as a

criminal according to the law of the land. Yet Dr. Goodell publishes that, for a condition, it is "*far better to kill.*" Shall any man take unto himself the prerogative to kill? Hearken to the great eternal lawgiver who saith, "Thou shalt not kill."

To recapitulate:

1. In contracted pelves the narrowing is more frequent at the brim than it is in all other varieties of distortions.

2. The narrowing rarely reaches a minimum of three inches.

3. The contraction is unsymmetrical.

4. It is marginal in character.

5. In pelves contracted at the brim the head presents transversely.

6. As a rule, the vertex, when not interfered with, points to the larger lateral side of the opening.

7. When the case is left to the natural efforts, the head is moulded and flexed so that the greatest conjugate (biparietal) diameter enters the brim and descends to the side of the minimum point of narrowing in the pelvic opening, and therefore the head is nipped in its descent by the narrow point (stricture) of the pelvis in advance of the biparietal diameter. And the greater the flexion of the head, the further anteriorly will the head be nipped.

8. By adjusting the forceps to the sides of the head we simply aid these natural forces.

9. The traction force by the forceps need rarely exceed 40 or 50 lbs.; frequently 25 or 30 lbs. will be sufficient.

10. Cephalic labors are less dangerous to the child than pelvic labors.

11. In forceps deliveries the mother's tissues should not be injured.

12. Through pelves of +3 or $3\frac{1}{2}$ inches, as a rule, the child can be safely delivered.

13. With a pelvis of 3 inches the labor may be spontaneous; generally, artificial labor will be required. Usually the labor can be terminated by the forceps without injury to the mother, and frequently with safety to the child.

14. Artificial assistance should be rendered as soon as the tissues are in a sufficiently dilated condition and it is ascertained that the woman cannot accomplish her task; before she falls into a state of exhaustion, and before the head of the child is jammed into and locked in the pelvic opening.

FELT CLOTH.

DR. E. WILSON exhibited a specimen of exfoliated cuticle of nearly the entire hand, which came away in one piece, from a case of scarlet fever.

DR. A. H. SMITH asked whether it was a case of the vesicular form of scarlatina. The most marked case of exfoliation he had seen was of that character.

CYSTIC TUMOR OF THE OVARY.

DR. SAVERY presented an ovarian cystic tumor, removed from the right side of a patient, aged 58 years. The tumor had existed for five years, but had increased very much in the last two years. It consisted of a large cyst, with several smaller ones. Its entire weight with contents was thirty-seven pounds; weight of tumor alone, one and three-quarter pounds; the amount of liquid was fifteen quarts.

The patient required nourishment by enemata, on account of the irritable condition of the stomach. She sank rapidly, and died of exhaustion three days after the operation, which was performed by Dr. Washington L. Atlee.

Dr. R. STEWART read the following paper on

THE COMPARATIVE MERITS OF CEPHALIC AND PODALIC (OR VERTEX-FIRST AND VERTEX-LAST) DELIVERIES THROUGH Pelves
NARROWED IN THE CONJUGATE DIAMETER.

MR. PRESIDENT: In the beginning of this discussion at our April meeting, I endeavored to show that the head of the infant in pelves contracted in the conjugate diameter, should be placed and kept in the transverse position. I also then drew some comparisons between the two modes of delivery, and at the May meeting, I showed on the pelvis the position the forceps should take, if the head is held transverse; and the mode of applying them when the presentations are occipito-posterior. I propose this evening to summarize what I have thus already stated, and in an unbiased manner to examine the comparative merits of cephalic and podalic, or vertex-first and vertex-last deliveries, through pelves narrowed in the conjugate diameter. If my conclusions do not coincide in all respects with either of those who have taken so active a part in the argument, I hope, in what follows, sufficient cause will be assigned for such disagreement.

It would, at first thought, appear strange that any difference of opinion should exist in a matter involving so much of purely mechanical principles, as that of the delivery of the body of a child through the pelvic channel; yet, difference in practical experience, preconceived opinions, fear of making innovations on established teachings, and hesitancy upon entering avenues differing from those in which we have previously travelled, all combine to keep us from independence of thought and action.

In examining a question, the answer to which involves the life of a child, and perhaps that of a mother, let us compare delivery by forceps, vertex-first, with delivery by turning, vertex-last, and thus ascertain whether there are not valid reasons for believing that in certain cases delivering by turning will be preferable to, and more successful than delivery by means of the forceps. What is the cause of the narrowing of the conjugate diameter of the superior strait, as usually found in practice? It is due to the encroachment of the promontory of the sacrum on the pelvic brim; or, in some rare instances, to the projecting forward and downward of the lower lumbar vertebræ; or, the symphysis pubis may bend inward and backward, or have an enlargement thereon. The promontory may be projected forward, or be abnormally increased in size altogether, or at some particular point, and also be located more to one side of the brim than to the other, thus not only narrowing the antero-posterior diameter, but to a certain degree lessening that side upon which it is located. The effect of such condition or conditions, is to cause the vertex to assume a different position from what it would in a normal strait. Thus, supposing the position of the body of the infant to be that appropriate to a left occipito-anterior position of the head, any one of the above conditions may force the head either too nearly to an occipito-anterior position, or even make it present as a left occipito-posterior. If the prominence be more toward the right of the pelvis, the left side of the strait then being the larger, the occiput will be pressed back toward the left iliac junction, thus causing it to assume a left occipito-posterior position; the body meanwhile still being as if the head presented in the left occipito-anterior. So, by the prominence being more to the left, a right occipito-anterior position may be converted into a right occipito-posterior, the body being yet as if the head were still in the right occipito-anterior. The latter condition of the superior strait would tend, in like manner, to make an existing left occipito-anterior position tilt more toward an occipito-anterior; but in all these cases the head tends to present obliquely. In order that the narrowest diameter of the child's

head may pass through the narrowest diameter of the superior strait, the head must be in a transverse position. If the occiput has passed to the left sacro-iliac junction, it must be rotated forwards, until it is nearly opposite the left acetabulum. If it be already toward the symphysis, it must be rotated backward toward the sacro-iliac junction until again opposite to, or slightly beyond, the left acetabulum. The head must be held in this position in order to engage or to pass the obstructing point. At this period, if the forceps are to be applied, they must either be applied with the concavity of the handles looking upwards to the symphysis pubis—thus making the blades lie the one on the occiput and the other on the forehead, or they must be applied with the concavity looking toward the acetabulum—thus making the blades lie on the sides of the head. In the former position, which is the method taught and practised by the continental authorities, compression lessens the occipito-frontal, and thereby increases the biparietal diameter, thus adding to, instead of diminishing the diameter which must pass the narrowed conjugate of the superior strait. This of necessity augments the difficulty to be encountered, and may result in the disfigurement or the destruction of the child. Furthermore, after passing the superior strait, traction cannot be continued long, for the head should not pass through the inferior strait in this position, but must be revolved upward toward the symphysis. This, if done with the instruments still on the head in the occipito-frontal diameter, would make the blades issue from the inferior strait in a line transverse to the vaginal orifice, the concavity of the handles then looking toward the acetabulum. Therefore, in such cases it necessitates the removal and replacing of the instruments after passing the superior strait. Apart from the tractile force used, nothing is gained by such an application of the forceps. On the contrary, a loss ensues, because, just in proportion to the power applied is there a diminution of the occipito-frontal, which has plenty of room in the transverse diameter of the pelvis, and which should be lengthened out so as to allow the biparietal diameter to become shorter; it also increases the biparietal, which should be diminished in order that it may pass through the smaller diameter of the strait. Notwithstanding the instructions of the past, however wise may have been the teachers, we must affirm that this mode of practice is incorrect. The other method of application is the one which should be adopted. That is, the blades should be applied to the sides of the head; the concavity of the handles will then be slightly posterior to and look upward towards the acetabulum whilst making the traction through the superior strait. Thus, if the

head present in the left occipito-anterior position, then to keep the head transverse and to give facility for traction, the woman should lie on her right side, when, if the forceps are applied on the sides of the head and the head to be held in the transverse diameter, the concavity on the handles will, during the traction, be upward and slightly posterior to the left acetabulum (*Fig. 1*), and when the superior strait is passed, the handles will be gradually turned toward the symphysis pubis, thus bringing the head in the position requisite to pass through the inferior strait. When the head emerges, the blades still being at the sides, no change is required, and whatever power is necessary to help turn or revolve the head, is obtained by this use of the forceps.

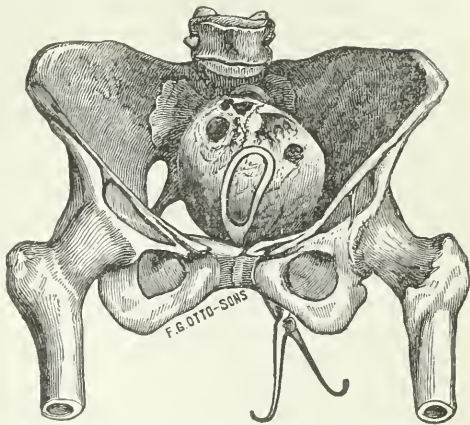


Fig. 1.

What changes are thus produced? The force applied diminishes the biparietal diameter and increases the occipito-frontal, which is the one having the most room for exit. In this manner the diameter having the least room to pass is diminished, and the one having the most room is increased. The child is not disfigured, there is less danger to the soft parts of the mother, and the forceps guide and revolve, as well as withdraw, the head through the channel!

The handles undoubtedly occupy an awkward position in the beginning, especially if the forceps are applied before the head has assumed the transverse position, and by the irregularity of the strait has been forced to present in a left occipito-posterior instead of a left occipito-anterior position, or a right occipito-posterior instead of a right occipito-anterior. The concavity of the handles will then look towards the sacro-iliac junction, but by means of the forceps the head is readily revolved into the transverse diameter of the superior strait. It is in these cases, in which the position of the body does not correspond with the position of the vertex, that, if not correctly diagnosed, traction may be made injudiciously. The body being grasped by a firmly contracting uterus is forced downward, in such a manner as would keep the occiput, were it free, to the symphy-

sis; but the operator who supposes the position to be an ordinary occipito-posterior one, applies the forceps and endeavors to revolve the forehead under the symphysis. If successful in this attempt, the face would be made to look to the abdomen of the mother, but the uterus holding the body fast, would tend to keep it with its back to the abdomen. The neck would run a very fair risk of being twisted to the extent of a semicircle, and the child's life would be endangered. The same danger may arise if one side of the pelvis is larger than the other; for, if the occiput is on the left side of the pelvis, and the right side is the larger, then the occiput should be turned to the right side. To turn it from one side to the other, unless done early, either before or immediately after the membranes are ruptured, may be difficult or impossible.

The forceps having been applied to the sides of the head, it must be compressed to the extent of the thickness of the blades before using traction. An additional power must also be exerted equivalent to that power of the uterus, which, by propelling the body and the head in a different direction from the one it must take to be delivered, is lost in consequence of the head being held transversely. Besides the difficulty in applying the forceps, there is a loss occasioned by the position the uterus must take. The axis of the uterus, in a normal pelvis, is a line perpendicular to the plane, and passing through the centre of the strait. Any protuberance or projection of the promontory makes a new centre nearer to the symphysis pubis. The uterus no longer exerts its propelling force in a perpendicular direction to this plane, but pushes the head against the pubis; or, in other words, propels the head through the strait at an angle, instead of perpendicularly, the head striking against the pubis, and then slanting back towards the sacrum. Whatever loss is thus occasioned must be made up by traction, before any power is really gained to bring the head through the contracted portion. The tractile and compressing power now being brought into exercise, it is held that time can thus be taken alternately to draw upon, and then relax our grasp, according to the contractions of the uterus, and that thus, by a slow, and it may be tedious process, the head will be drawn out or lengthened, so as to pass the nipping points. The amount of compression a child's head can endure is easily ascertained by the craniometer—to which reference will be made hereafter—varying from zero to one and one-quarter inches diminution of the biparietal diameter, the difference in amount being due to the extent of the ossification of the sutures. As the result of compression, I have seen the child's eyes protruded, like a frog's, from the sockets, and the head drawn out like a

eone, and yet the child has survived. With this great advantage of having time during which we may extend the head, there is danger, by too long continued compression of the cranium, of destroying the child's life; and by too long continued traction against the soft parts of the mother of injuring them, besides that of producing vesical troubles. That under the writer's observation there have been cases of great sloughing, and in one case absolute destruction of a portion of the walls of the bladder, may perhaps only be cited as an improper use of the forceps, and therefore not to be considered in the argument. Yet these effects may and frequently do follow, to a greater or less degree. The head, having passed the narrow diameter of the superior strait, is checked in its onward progress by the shoulders and the chest, which meet the same obstacle as did the head. Therefore the forceps are again needed to help draw the shoulders through. In thus drawing on the forceps, perhaps it has not occurred to us that we, in reality, are now drawing on the neck, and that whatever resisting power has thus to be overcome is entirely sustained by it. After all this effort, how often do we find the child still, its life having been destroyed by compression of the brain, or injury to the spinal cord, and the case terminating with a bad "getting up" on the part of the mother.

Suppose, however, that after having made traction, we are satisfied the head will not pass without being reduced in size, or have reason to believe that the child is dead, then there will be no impediment to the performance of craniotomy, which is one of the greatest advantages claimed for vertex first delivery through a contracted conjugate diameter.

Let us now in the same manner examine the process of delivery by turning or vertex-last. As a general fact, I think none will deny that where necessity has compelled turning from causes other than the one under consideration, or in breech presentations after the shoulders have emerged, the head, usually with great rapidity, passes through the superior strait, and at once is found pressing against the inferior outlet—and that too with those, who in other labors, are slow in having the head delivered. But in the cases under consideration, the first difficulty that may present itself will be the act of turning, especially if the water has all been evacuated and the uterus is firmly contracted on the child. There is then the further probability that in withdrawing the shoulders the uterus will strip the arms over the head of the infant. This will certainly be the result if done rapidly, instead of leaving the propulsion of the shoulders through the os uteri to the slow but firmly contracting and propelling power of the uterus. There is the

further danger in this case of injury to the mother, although all of us can probably call to mind instances where turning has been accomplished with the greatest difficulty, and yet unattended by any evil effects to the mother. The turning, then, to be easily and safely accomplished, should be resorted to early, and slowly performed, the head and the shoulders being left as much as possible to the efforts of the uterus.

The shoulders having passed the superior strait, and the arms being withdrawn, the head will at once, on traction being made, or by the uterine power alone, fall into the strait in the transverse position, because the smaller diameter of the head comes first, and therefore drops into the narrow conjugate diameter of the pelvis. There is no force acting in any manner to change this position; but it is at once assumed, and the uterine force and the tractile power all combine to keep it in this transverse diameter; and if one side of the pelvis be larger than the other, we can so turn as to bring the occiput in the larger side.

In the meantime what is the condition of the uterus? Having been relieved of a portion of its contents, it has room to contract perpendicularly upon the head, and loses none of its force because it does not propel the head through at an angle. It contracts more forcibly than it did in the beginning. There being no blades applied to the head, and consequently no increased diameter, no compression is now required to lessen it; nor is any tractile power required to substitute lost or insufficient uterine force. There is, furthermore, the facility for using the same amount of tractile power by drawing on the shoulders and neck that there was in vertex-first when using the forceps. The neck, as has been already stated, in vertex-first deliveries, often endures a very large force required to draw the shoulders through, and why can it not endure the same force, if required, to draw the head through? The amount of traction that can be safely applied, Dr. Goodell has tested practically; and I can bear testimony that on one occasion, after having removed a great portion of the cranium of an infant and finding the shoulders apparently unable to pass the superior strait, one physician grasped the face and the remnant of the head, while another pulled upon a towel tied around its neck. These two, bracing themselves as best they could, used their combined strength to aid me in removing the shoulders, and yet the neck was *not* broken. If the neck of a dead infant will withstand such a force, will not that of a living one, whose tissues possess the power of contractility, endure as much? Although in a similar case the neck *was* broken, and though the force required to bring the shoulders through may not be

as great as that required to bring the head through, yet at all events we must concede that the same amount can be as safely used, as can be by the forceps, in drawing on the head to withdraw the shoulders, when they are held in the superior strait.

Another power can now be brought into very successful operation—that is, pressure through the abdominal parietes on the contracting uterus. This not only aids by increasing the uterine contractions, and thus gaining more active propelling power, but in another most important manner, viz.:

When the head reaches the narrow conjugate, traction on the body and neck will cause extension of the head, the nipping points will thus be made to catch the diameter of the child's head about the parieto-temporal junction. Now, while the operator holds the body firmly, and steadily draws upon it, let the assistant, acting from above, cause flexion to take place. This is the same as making the *nipped* points descend downwards in the arc of a circle whose centre is the occiput, and just so far in the arc as this movement continues, just so far will the nipped points have moved through the narrow outlet. This distance will be represented by a line going from the points first nipped, *upwards* and *backwards* on the child's head. The assistant now holding and pressing upon the forehead, so as to keep the chin immovable, let the operator draw steadily downwards, and the assistant also press upon the occiput, in order to again produce extension of the head. This will now cause the nipped points to move in the arc of a circle, of which the *chin* is the centre, and the effect will be to move the nipped points through the constricting outlet, a distance represented by a line, or an arc *upwards* and *forwards* on the child's head, and thus, by alternately producing extension and flexion, without the terrible amount of pulling with which this mode of delivery is charged, will the head be *pushed* through the contracted part. If the representation were diagrammatically made, the course taken by the nipped points through this lever motion would not incorrectly be represented by curved zigzag lines, first upwards and *backwards*, then upwards and *forwards*. This any one can try on his own head; let him place his forefingers, one on either side of the head about the squamous suture, and let the head be extended, then holding the fingers stationary, move the head or face downwards, *i.e.*, flex the head, and he will find the head has passed itself through the pressing fingers. In this trial, the

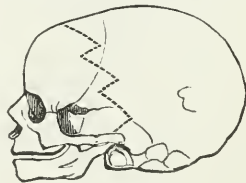


Fig. 2.

fingers represent the pelvis, and the same motion produced on the child's head will have the same effect as regards the pelvis that the motion did on the points of the fingers on the side of the head. Besides, there is also now an entire freedom in the pelvis for the head to be moulded around the jutting promontory. In vertex-first, traction moulds by drawing out the head lengthwise. In vertex-last, it is by pressure on the side, and, as it were, indenting it, and nothing interferes; where, in vertex-first, the forceps, by compression, forces a moulding according to their position as applied. What disadvantage have we in this mode of delivery? After the head has reached the superior strait, the delivery must be rapid, because the uterus is contracting more forcibly, and on a smaller surface, and will therefore be more likely to separate the placenta, and thus destroy the child's life. The cord may be pressed upon and stop circulation; then there may be danger by pulling on the neck, particularly so if the neck be twisted. Then, again, the life may be lost at the perineum. I say may be, for this can rarely happen if the proper course be taken when the head has advanced that far.

As to vesical troubles, there will probably be less than in vertex-first deliveries, not because there is less pressure, but because it is not so long continued. It has been intimated that this is from the former cause, but we must admit that there being nothing between the side of the head and the symphysis, whatever power is required to mould the sacral side of the head around the promontory, must be resisted by the same power on the pubic side, otherwise there would be no moulding or indenting. In vertex-first, the blades, by compressing the sides of the head, tend to relieve the pressure on the urethra, and theoretically, so far as this argument goes, would result in less vesical trouble; but, as stated, it is the continuance of pressure that paralyses the muscles. Besides, in some pelves, just at the symphysis, there appears to be a widening out of the pubes, making a small and almost acute angle, in which the urethra is safe from pressure, even if long continued.

The last source of danger is the uncertainty of successfully withdrawing the child alive, and the greater difficulty that may be experienced in performing craniotomy, especially if the shoulders should be large and the inferior outlet small. I know of an instance where the neck was twisted off, leaving the head in the uterus, and of another in which the mother expired even before the shoulders had been delivered through the strait. Cases may occur, then, in which, after turning, the chest must first be opened and then craniotomy performed, in order to remove the child.

COMPARISON BY ANTITHESIS.

Vertex-first.

The head may be in a wrong position, which must be changed.

If the occiput is on the smaller side of the pelvis, it is hard to rotate.

Difficulty in applying the forceps.

Must make compression equal to the space taken up by the blades.

Loss of propulsive force which is directed at an angle to the superior strait.

Traction must compensate for this loss.

Long continued pressure may produce vesical troubles, and injure the tissues.

Have time for traction.

The forceps allowing unlimited tractile power to be used, the child's life is endangered by compression, of the brain, etc.

Traction is readily made.

Have but little power applied from above.

The head cannot be moved except by the use of the forceps.

Craniotomy may be readily performed.

No danger of irritating the uterus.

No danger to be apprehended from pressure on the cord.

Vertex-last.

The head at once assumes the correct position.

The difficulty is slight.

The forceps not required.

Compression is unnecessary.

This force, being applied in a line perpendicular to the strait, no loss occurs, and the uterine contractions are stronger.

No compensation required.

Delivery is necessarily rapid and therefore these dangers are not so great.

Have not, or will lose the child.

Being able to exercise the same power by drawing upon the neck, like results may follow.

Traction is more difficult.

By pressure a force can be exerted upon the abdomen equal to the tractile power.

By alternately flexing and extending the head, it can be made to pass the constricting points.

This operation may be very difficult.

In turning there is this danger.

There is danger unless the delivery be rapid.

The placenta will not be detached before delivery of the head.

The child's life may be lost from this cause.

Of these comparisons nine are favorable to the method of delivering the vertex last, and six to that of the vertex first. If turning is performed early, the danger of irritating the uterus is, to a certain extent, avoided. The dangers from pressure are proportionate to the amount and continuation of the force employed in making traction, and the circulation in the cord may be interrupted if the size of the pelvis will not admit of the cord lying along the spine. There remain unfavorable to vertex-last the necessity for rapid delivery of the head, after the shoulders have passed the strait, on account of the probable separation of the placenta; the danger from drawing irregularly upon or twisting of the neck, and finally the difficulty that may be experienced if required to perform craniotomy. The object of all manipulation is to deliver a *living* child, and if there is no hope of this, our duty to the mother and the child requires us to resort to craniotomy, which, if done early, may be readily and safely performed. I say safely, having performed this operation a great many times without a bad symptom resulting.

How are we to know that turning will be more favorable to the child? There are two unknown quantities, one the length of the conjugate diameter of the superior strait, the other the biparietal diameter of the child's head. Knowing the conjugate to be small, will this knowledge alone warrant us in turning? Perhaps the head is unusually large, and if so we may add danger to the mother, and be obliged to open the chest of the child, as well as to perforate and crush its head, before delivery can be accomplished. In other words, there is a certain relationship between the size of the pelvis and that of the head which necessitates the death of the infant; and how are we to know when this relationship exists? Does it follow that a conjugate of four and a half inches will always allow a living child to pass, or, that one of two and three-quarters will always prevent one from passing? I recently delivered a child at full time, whose measurement by the craniometer was three inches, easily reduced to two and three-quarters, and by harder pressure could be reduced to two and five-eighths inches. Another, where the head before and after birth measured two and thirteen-sixteenths inches, which had, by lying in a wrong position, retarded a labor for hours. The pains and bearing down efforts were of the most rapid and forcible character, and the pelvis so large that in all previous births the child was born before the physician arrived. This child's head could have been ex-

tracted easily through a conjugate of two and three-quarter inches, and with difficulty through one of two and a half. We must then, in addition to the size of the conjugate diameter, ascertain that of the child's head. This is accomplished by having placed on one of the handles of the forceps a bar three inches long, so divided as that when the handles are separated the divisions on the bar will represent the extent of the separation of the blades when they are applied to the sides of the child's head. By means of this craniometer one can very accurately measure the biparietal diameter of the head, and the conjugate being known, a reasonable conclusion can be formed as to the probability of a successful delivery by turning. We must likewise take into consideration the size of the inferior outlet of the pelvis, which will also determine the difficulty or ease of performing craniotomy, if this operation should become necessary. As an instance, a few months ago I was prompted to turn and attempt delivery, when I found that it could not be accomplished by the use of the forceps; but on measuring the head with this craniometer, I found the biparietal diameter was four and five-eighths inches; the conjugate diameter of the superior strait was less than three and a half inches. I also found that I was unable, by all the compressing force I could exert, to reduce the diameter of the head to less than four and one-eighth inches. The inferior strait was narrow, the pubic rami forming an acute angle. I therefore perforated the head, and after very great difficulty, during which the neck was broken, delivered the child. Upon making measurements of the child after it was delivered, I was fully satisfied that turning would have involved craniotomy at last. The relative size of the head and pelvis being known, and the difference after compressing the head being slight, will not the chances of delivering a living child be more favorable by turning? They certainly will be, if the forceps are not applied to the sides of the head and injudicious traction is made. There is not a practitioner of extensive obstetrical experience who has not met with difficult deliveries, produced by the head having been embraced by the blades in such a manner as did not bring its diameters in correlation with the diameters of the pelvis. And how often are the forceps applied without any definite idea of their position, except that the handles shall lock. Suppose, in the case of a projecting promontory, the forceps should be applied to the sides of



Fig. 3.

the head, and traction be made with the concavity of the

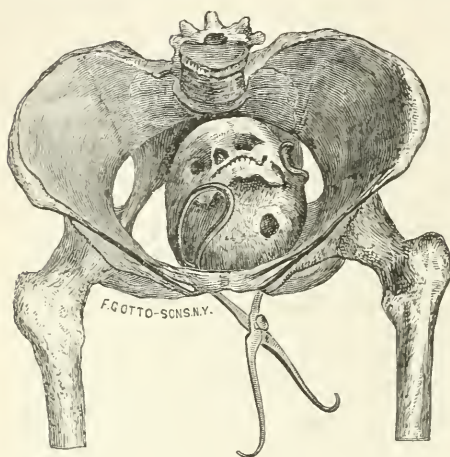


Fig. 4.

handles looking up towards the symphysis, how, under these circumstances, could the head be delivered? The forehead would come down directly upon the jutting point of the promontory, and before the head could pass it would be indented or crushed. To a degree the same effect must result if the head were drawn upon while in the left occipito-anterior position. (*Fig. 4.*)

Although in such cases traction for hours be unavailingly made, and though turning would accomplish our object, yet, it might also be attained by making traction in a proper manner; and success by turning *does not*, in such cases, prove its superiority over the delivery by the forceps, but only proves that *traction* was *improperly* made. If, then, the choice is between turning and an uncertain application of the forceps, to be followed by improper traction, we must concede the chances are more favorable in vertex-last than vertex-first deliveries. But supposing the forceps are on the sides of the head, which is in a transverse position, and that the *same points* are impinged upon by the contracted conjugate diameter as would be if we had turned the child, and the vertex come last: will not the delivery be as effective, safe, and easy with the vertex first, assisted by the forceps, as it would be by turning? If the head be impinged upon at the same points, can we not pull it through more readily by means of the forceps than by turning and pulling upon the neck and body of the child whilst we are aided by pressure made from above? To this, I think, I must reply in the affirmative. But if the occiput lies on the smaller side of the pelvis and cannot be rotated, then, the occiput being pushed nearer to the conjugate diameter, the impinging points will be moved further towards the vertex, and therefore to a larger diameter of the head. In this case, even if the forceps are applied correctly, and traction be properly made, the same diameter of the head *not* passing through the contracted conjugate of

the pelvis, there cannot be the same facility in withdrawing in vertex-first as if the head was turned and drawn down so as to bring the occiput to the larger side of the pelvis.

We must therefore conclude that whenever, by carefully ascertaining the size of the head and pelvis, there is a reasonable hope that a *viable* child can be delivered, and especially when, with this, the head lies with the occiput on the smaller side of the pelvis and cannot be changed, and if a judicious trial with the forceps has proved unsuccessful,—delivery should be attempted by turning. But to make this operation efficacious, we should turn the child so that the occiput will be brought to the larger side of the pelvic brim; and before making this attempt it must be ascertained whether the inferior outlet is of such a capacity as to render craniotomy practicable should it be found necessary to resort to it.

How long shall we wait before determining upon this mode of delivery? Time is an uncertain element in such calculations. Measurements must influence us greatly in deciding the case. I have perforated the head after waiting but a few minutes, and yet have waited hours upon a hard but eventually safe delivery. In general, for reasons already given, turning, to be done easily and with safety to the mother, should be done early.

When the height of the head above the parietal protuberance is not very great, I have delivered by keeping the head transverse, raising the vertex and turning the forehead downwards. This makes the sides of the head, just above the ears, slide through the narrow conjugate. After passing the superior strait, if the inferior pelvis be large, we can revolve the vertex under the symphysis. In turning, we endeavor to cause a portion of the sides of the head to pass somewhat in the same lines as if the forehead descended first; but if the forehead comes first, we have many of the advantages, and yet avoid all the dangers attendant upon turning.

Stated Meeting, Thursday, November 4th.

The Vice-President, DR. JOHN S. PARRY, in the Chair.

URETHRAL TUMOR.

DR. WILLIAM GOODELL reported the case of a woman with a painful urethral tumor, which bled at the slightest touch. He exhibited the tumor, which was as large as a walnut. The examination gave rise to so much hemorrhage that the woman became blanched, and he was obliged to staunch it

by the application of Monsel's solution. He found that the tumor sprang from the lower two-thirds of the urethra, which was elongated and greatly dilated. He removed it by the galvano-cautery. It seemed impossible that the tumor could be a caruncle. It was dense and looked like a malignant growth.

It had been two years in growing. The woman had passed the climacteric.

DR. LUDLOW asked the experience of the members in regard to urethral caruncles.

He had found the only method of treatment which was permanent in effect was that of excision of the tumor and searing the point of attachment with red-hot needles, etc., combined with the use of Monsel's solution, when necessary to check bleeding. He had used nitric, carbolic, and chromic acids, acid nitrate of mercury, etc., without permanent benefit. He recommended as useful in some cases an ointment of arsenious acid, three grains, and iodine, five grains to the half ounce of lard.

DR. WILLIAM GOODELL remarked that the only certainty in the destruction of these caruncles was by the use of the galvano-cautery, or the red-hot irons to the raw surface. Fuming nitric acid sometimes acts very well.

SUDDEN EFFUSION OF BLOOD INTO THE LABIA OF PREGNANT WOMEN BEFORE LABOR.

DR. J. V. INGHAM reported two cases of sudden effusion of blood into the labia of pregnant women occurring before labor, and without any evidence of direct violence. The first case which he saw, about four years ago, had the following history: A young married woman, 25 years of age, was five months advanced in her second pregnancy. One evening while walking across the floor she tripped, but was caught by her husband, however, before she fell. At the moment of tripping she was conscious of a sudden, sharp, lancinating pain in her external genitals. This pain was so acute that she was unable to walk up-stairs to her bedroom. He was at once sent for, and when he reached the house, not more than twenty minutes after the occurrence, found her lying upon the floor, with her legs widely separated, and evidently in great pain.

On making an examination he found the left labium enlarged almost to the size of an orange, livid in color, and so exquisitely sensitive that the slightest touch was unbearable. Ether was sent for, and nearly ten ounces were administered without producing unconsciousness. He was, however, able to introduce his finger, part the tumor, and ascertain that it had

no connection with the vagina or abdominal cavity; that it was not a hernia, but an effusion of blood from a broken vessel. The patient was placed in bed, and cloths saturated with laudanum and lead-water were at once applied. The next morning the tumor was less painful; the dressing was changed to an evaporating solution. The tumor slowly contracted to the size of a walnut, when it opened and discharged about an ounce of a thick, fetid, and almost black fluid, and the labium regained its normal size. A week later the opening through which the discharge came had closed, when, while the woman was in the close stool, the labium again partly filled up. This was treated in the same way. Suppuration again occurred, and the labium resumed its normal size. She was delivered by Dr. Ingham, at the end of the full period of pregnancy, without any difficulty or any increase in the size of the labium.

The second case came to his notice about two weeks ago. A young woman, about 23 years of age, eight and a half months advanced in pregnancy, found one morning a lump in her right labium, about the size of a walnut. The night had been quiet; she had been unconscious of any injury or pain, and had slept alone. This tumor slowly increased in size and became so painful that when Dr. Ingham saw her, three days after the appearance of this trouble, she could hardly walk. The tumor was then about the size of a quarter section of an orange, and dark in color, though not so livid as in the previous case. A mixture of lead-water and laudanum was kept constantly applied with advantage for a few days, when she fell in labor. As the tumor then interfered with the passage of the child's head, it was opened, and the pus and decomposed blood escaped. The forceps were applied and the child safely delivered without a recurrence of the effusion.

In this case there was no subsequent recurrence of the effusion, the labium healing up during the lying-in period.

DR. INGHAM then stated that he would like to ask the other members of the Society if they had met with similar cases, and how were they in the habit of treating them? He would also like to ask if the immediate use of the knife to evacuate these tumors was advisable? and whether its use would not increase the liability of a second effusion of blood, similar to that which occurred in the first case after the suppuration. He was aware that the knife must be used to open these tumors when they occur during labor, but could find no authority advising its use before labor. He believed that this was a rare accident before labor, as the obstetrical and surgical authors make but slight allusions to it.

DR. ELLWOOD WILSON remarked that he should not hesitate

to make an incision on the vaginal surface of the tumor and let out the blood.

DR. LUDLOW asked whether the woman had been troubled with varicose veins.

DR. INGHAM replied that both patients were young women of 23 and 25 years respectively, and that there was no appearance of varicose veins.

DRS. WILSON and LUDLOW cited cases of vaginal tumors composed of large masses of enlarged veins, but neither of them had ever seen cases similar to the ones reported by Dr. Ingham.

DR. JNO. S. PARRY related the case of a woman five months advanced in pregnancy, admitted to Blockley Hospital with a thrombus of the vulva. She was seized with a severe pain and started to walk to the hospital. On the way she experienced a sudden relief, and at the same time felt a warm fluid running down her limbs. There was found on examination a large cavity in the vulva, all the clots having escaped. It healed up entirely.

He believed that, in those cases in which the skin becomes livid and blue, the proper treatment is to open the thrombus, remove the clots, and arrest the hemorrhage.

He had seen a case of tumor of the anterior portion of the vagina of the size of an orange, made up of varicose veins entirely.

He had never seen any of these veins give way during labor.

He thought that he would not use the knife if there was any chance of absorption, but only where the tumor is livid and likely to burst.

BLIGHTED OVUM.

DR. R. A. CLEEMANN exhibited an ovum with the following history:

About a year ago I presented to the Society a specimen of "vesical mole," hydatidiform degeneration of the chorion. I show now a blighted ovum undergoing the change which has suggested the name of "fleshy mole," apoplectic membranes.

I was called to the patient from whom it was expelled about two weeks ago. She was a young woman living with her husband and their child, an infant of two years of age, in a canal boat, on the Schuylkill. A murderous fight had occurred that day among some sailors of a neighboring vessel, which had caused her great alarm for her husband's safety, in consequence of which, as she herself supposed, she was seized with violent continuous pain in the lower part of the abdomen and with flooding. How much blood had been lost before my arrival I

did not learn, but the woman's countenance was pale and anxious. She did not consider herself pregnant, as she believed her catamenia had been regular. I elicited by questioning, however, that she had missed a period four months before. Vaginal examination revealed the os uteri somewhat enlarged, softened, and patulous, and within a firm elastic mass: by counter pressure on the abdomen the size of the uterus was easily made out, about the dimensions of a large orange; *bal-lottement* furnished no evidence of the presence of a foetus. Hemorrhage was not at the moment profuse, but intending to leave the patient, I stuffed the vagina with strips of cotton cloth smeared with lard—the best tampon at my disposal—and prescribed the wine of ergot. When I returned, after an absence of some hours, I learned that the pain had ceased about two hours after my departure, and I found in the vagina, behind the saturated tampon, an unruptured ovum.

In appearance this had reached, as you observe, three months of development; but incising the membranes, from which the inclosed liquid gushed with some force, I discovered a foetus of only about five weeks' growth, since it was less than an inch in length and with the lower extremities represented as yet by single bud-like projections. The embryo hung by a somewhat translucent attachment, a little longer than itself, nearly as thick, and expanded at the placental end. The interior of the amniotic cavity presented the "tubercular" appearance, being scattered over with prominences of varying size, the "bosselures" of Velpeau: some of these were as large as filberts, and looked like venous varicosities; they, of course, add considerably to the thickness of the membranes. I have separated the amnion from the chorion at a translucent portion, and the deciduum from the edge of the forming placenta.

The inference drawn from the specimen is, that the foetus perished at five weeks, from amniotic dropsy perhaps, but that the membranes not collapsing, their hold was sufficiently maintained upon the maternal tissues to furnish them the materials of growth: their little prominences are due to extravasations between them, the result of varying uterine pressure. The limit of retention within the womb was determined by the mental shock of the fright which induced reflex contractions of the uterus.

Dr. JAS. TYSON reported a somewhat similar case which had recently occurred in his practice. The foetus had, however, entirely disappeared. The membranes were well developed and presented a normal appearance under the microscope.

Stated Meeting, Dec. 2d, 1875.

The President, DR. A. H. SMITH, in the Chair.

DR. WM. GOODELL read some comments on DR. WILSON'S Papers on

VERSION IN CONTRACTED PELVES.

PAPER V.

MR. PRESIDENT AND GENTLEMEN OF THE PHILADELPHIA OBSTETRICAL SOCIETY:—I am beginning to think with Newton, that "a man must either resolve to put out nothing new (I still venture to call my method *new*) or to become a slave to defend it." If, therefore, no other member of this Society is disposed to continue the discussion on my "*Clinical Memoir on Turning in Pelves Narrowed in the Conjugate Diameter*," I shall beg leave to offer a few remarks upon Dr. Ellwood Wilson's papers, read on the evenings of September 2d and October 7th. It was my purpose to have replied to them at the last meeting of this Society; but as I was not able to get all the necessary papers from Dr. Wilson until late on October 26th, I did not have time enough for this purpose.

And first, although, as pointed out by Dr. Wilson, my paper of June 3d was written with haste, I beg to say that its conclusions were the result of deliberate consideration, and that I have no wish to recall any portion of it. Let me also remind the Society that the question raised in my original memoir was not that of the merits of the obstetrical forceps—an instrument for which I profess admiration as great as that felt by any member of this Society, and which I constantly use—but that of the propriety of substituting for the use of the forceps the operation of version in certain special cases when the conjugate diameter is between 2.75 and 3.25 inches, and of resorting to version in cases of even less deformity, when the forceps has been tried and has failed. Had Dr. Wilson simply read over the final "Propositions" of my memoir, he would have seen that I am not amenable to the twice-repeated charge of advocating "the indiscriminate resort to version in coarctated pelves." Hence I am unmoved by his lengthy and instructive quotations from various authors in praise of the forceps,—praise which I am quite disposed to consider well deserved by that useful instrument.

A few points in Dr. Wilson's papers must now be adverted to, for, although he holds in this city a high rank as an obstetrician, he must, like other people, submit to have his assertions canvassed and corrected. The statements to which I shall refer have doubtless been made, partly from misapprehension, and

partly from the lack of time to read attentively my papers and his own authorities. I regret that the exigencies of this debate have hindered him from having time enough for carefully examining my paper on "*Head-Last Labors*," and my "*Clinical Memoir*." These he had in his possession seven months only; and my *Reply* to his criticisms on both of these papers not more than four months. Had he not been greatly pressed by the shortness of this time, I feel assured that he would hardly have made it needful for me to make the following corrections:

1st. I have not denied, but, on the contrary, have affirmed that when left to itself the foetal head will eventually dip in a narrow pelvis. But I have averred that when the narrowing is mainly limited to the conjugate, the head does not *tend* so to dip, and usually not until the fore-cone has by extension become engaged and fixed in the conjugate. That this mechanism is the true one is proved, not only by the well-known preliminary dip of the anterior fontanelle, but by the invariable site of the fracture or of the bending in of the cranial bones, which in such unaided labors is found, not at one of the poles of the biparietal diameter, but in the fore part of the head, near the coronal suture. Further, I still hold that a prudent practitioner will, in such cases, usually deem it wise to come to the patient's relief before the final dip of the vertex takes place, although not ignoring, like Dr. Wilson, the resources of nature to the extent of applying the forceps before the membranes are broken.

In this relation, let me ask Dr. Wilson to be good enough to tell us whether, in default of accepting the above mechanism of labor in narrow pelves, he has yet reached any fixed or definite conclusion with regard to the manner in which the head enters and passes the brim. In his first voluminous paper he cites Meigs, Hodge, and Leishman to show that "the vertex does enter the brim." He also adds that Hodge says "the head is not flattened out in narrow pelves, as represented by Simpson, but demonstrates that one pole or extremity of the biparietal diameter rests on the pubes, and *the other glides over against the promontory*, making the head oblique at the brim." "These observations of Meigs Hodge, and Leishman are," he adds, "*precisely* what I have observed in my own practice." In addition to these observations of his own practice, to clinch still further the proof of this mechanism, he in his second paper quotes from Milne as follows: "In vertex presentations the chin lies to the sternum, and therefore the vertex and the biparietal part of the head enters the brim first; that is, *with the larger diameter—the biparietal—opposed to the antero-posterior*." Now, this of course pledges Dr. Wilson to the passage

of the biparietal diameter through the conjugate. But, in a rider tagged on to the end of his third paper, he thus contradicts this observation of his own practice: "When the case is left to the natural efforts, the head is moulded and flexed so that the greatest conjugate (biparietal) diameter enters the brim and *descends to the side of the minimum point of narrowing in the pelvic opening*, and therefore the head is nipped in its descent by the narrow point (stricture) of the pelvis *in advance of the biparietal diameter.*"

Here are two different mechanisms of descent executed simultaneously, not by the two biparietal diameters of the infant Janus in Hecate's labor, but by the single biparietal diameter of the ordinary unmythological human fœtus. By the one mechanism, the biparietal passes obliquely through the conjugate diameter; by the other, directly to one side of it. In other words, the biparietal diameter is in two different places at one and the same time,—a trick indeed worth knowing. But since it is impossible for a finite human mind to believe in both of these cranial movements *simultaneously*, the only way out of this difficulty is to believe in both *alternately* in rapid succession.

2d. I am represented in Dr. Wilson's second paper as *disregarding* in version "the rising up of the *arms*, even directing us to proceed to deliver, when the pelvis is ample, without restoring the *arms.*" Now I have never advised such a practice, and all that I have said that could possibly be so construed is the following sentence: "In cases of pelves known to be ample, I can *conceive* of its being perfectly justifiable to follow Giffard's and Froriep's plan of dragging the head through with the impacted *arm* extended above rather than that of losing golden minutes in liberating it." I do not say "arms," but *arm*, and the incorrectness of the above assertion I pointed out to Dr. Wilson in my reply to his first paper. But he has seen fit to repeat it more than once in another form, thinking possibly that the repetition of an incorrect statement makes it correct. In his comment on this mere hint of mine,—a hint referring to manual delivery in *ample* and not narrow pelves, a hint which I have not yet found occasion to put in practice, but which he calls my "*rule for obstetric guidance*," he says: "In my opinion, no more dangerous rule for obstetric guidance than this has been announced since the publications cited, and Dr. Goodell may share jointly with these gentlemen the honors derived from it." Perhaps it may prove a source of gratification to him to learn that "the honors derived" from this "dangerous rule," I can "share jointly," not only "with these gentlemen," but also

with Millot¹ and Lachapelle,² Paré,³ Guillemeau⁴ and Deventer,⁵ whose experience in turning has never been surpassed; with Petit,⁶ who defends it by a long argument; with Jacquemier,⁷ Robert Lee,⁸ Velpeau,⁹ and Schroeder;¹⁰ and last, not least, with two gentlemen whom Dr. Wilson respects very highly, and whom, in this discussion he has repeatedly cited as excellent authorities—Denman¹¹ and Smellie.¹²

3d. I have not *denied* that Dr. Meigs applied the forceps to the sides of the child's head. But I have shown, by quotations from his writings; that he deemed such an application, *when the head is transverse at the brim of a narrow pelvis*, PREPOSTEROUS, and that he declared that COMMON SENSE would dictate the application to the face and occiput. Again, I have shown by quotations from Dr. Warrington's *Obstetric Catechism* that this experienced teacher considered that if the head is arrested at the superior strait TURNING WOULD BE SAFER than the use of the forceps, unless the practitioner had much experience in the use of that instrument. I shall not, of course, dispute Dr. Wilson's assertion, that he has repeatedly seen Dr. Meigs apply "the forceps at the brim to the sides of the child's head," or that he has heard, as he implies, Dr. Warrington teach for ten years the exclusive use of the forceps when the child's head is arrested at the brim. But I shall beg you to observe that it is Dr. Wilson, and not I, who thus charges these distinguished gentlemen, the one with practising what he did not teach, the other with teaching in private what he did not think proper to publish.

4th. Dr. Wilson says in his second paper that I am "mistaken" in saying that he "quoted Hodge as advising the application of the forceps to the sides of the head *when at the brim*." (The italics are his). But since our whole discussion referred to the management of the head "*when at the brim*,"—for of course version could not be resorted to after the head had passed the brim,—if Dr. Wilson did not refer to the course advised by Dr. Hodge when the foetal head is in that position, I utterly fail to understand to what he does refer in the following

¹ Supplement à tous les traités d'Accouch., vol. ii., p. 40.

² Pratique des Accouch., Paris, 1821, vol. i., pp. 102, 313.

³ Opera, Lib. xviii., cap. xxxiii.

⁴ De la Grossesse, Paris, 1642, lib. ii., p. 192.

⁵ Novum Lumen, Lugduni, 1733, cap. xlv., p. 198.

⁶ Cours d'Accouch., Paris, An vii., tom. i., p. 314.

⁷ Manuel des Accouch., Paris, 1846, vol. ii., p. 352.

⁸ Lectures on Midwifery, Philada., 1844, p. 322.

⁹ Traité Complet, Paris, 1835, p. 311.

¹⁰ Manual of Midwifery, New York, 1873, p. 184.

¹¹ Introduction to Midwifery, chap. xiv., secs. 3 and 5.

¹² Midwifery, vol. i., chap. iv., sec. 2: also Collections, xxxii., case ix.

sentence from his first paper: "He (Hodge) further states that with a pelvis of three inches in the antero-posterior diameter, the forceps should be applied to *the sides of the child's head*, which can be done without accident to the child, and with but little or no discomfort to the mother." If this does not mean "the application of the forceps to the sides of the head *when at the brim*," then I am forced to conclude that Dr. Wilson has accepted Talleyrand's definition of language.

Further, in Case VI. of my *Clinical Memoir*, I say, "The head was perched above the brim, with the occiput looking toward the left ilium. Failing to get the blades over the sides of the head, I had to content myself with their oblique application; but the head being a large one, they did not hold well, and we therefore decided to turn." In Dr. Wilson's comments on this case he says, in his first paper. "In her fifth confinement Dr. Goodell saw her. He informs us that the forceps slipped. It was evident, therefore, they were not applied as taught by Hodge." Now let us box the compass of this foetal head. It was *above the brim*. It was *transverse* and to the left. Failing to get the blades on the *sides* of the head, I applied them *obliquely*,—precisely as Hodge teaches. They slipped, as Hodge's forceps will sometimes slip from a large head. "It was evident, therefore," remarks Dr. Wilson, "they were not applied as taught by Hodge." Now, in his second paper, Dr. Wilson observes *correctly* that "Dr. Hodge prefers that the forceps should be always applied to the sides of the head, except when it presents *obliquely* or *transversely* at the brim." According, then, to Dr. Hodge I should not have tried to apply the blades to the sides of the head. But, according to Dr. Wilson, in getting them on obliquely I did not follow Hodge's directions. Am I then to understand that he chides me for not adjusting the blades over the face and occiput?

Again, in this discussion, which at the outset was limited to "Whether in these transverse cranial positions turning has advantages over the application of the forceps to the sides of the child's head?" (*Clinical Memoir*, p. 21). Hodge is Dr. Wilson's mainstay, almost his "only stock and store." He is constantly referring to that author, and quoting his success with the forceps,—a success beyond that of any of the other cited authors who applied the forceps invariably to the sides of the head. Now, since in all these quotations it appears that Dr. Wilson did not mean that Hodge applied the forceps to the sides of the head at the brim, and since the most remarkable cases of forceps-delivery in narrow pelves he has cited, come from men who did not apply that instrument to the sides of the head, the reader cannot evade the logical inference that, after all, Hodge's

oblique application at the brim is a more effectual one than Dr. Wilson's biparietal application. Indeed, by this fatal admission of Dr. Wilson's, I myself am beginning to become a convert to Hodge's fronto-mastoid application of the forceps at the brim,—an application which, in the face of Dr. Wilson's great indebtedness to Hodge in this discussion, he strangely enough calls, in his second paper, a “reprehensible procedure.”

5th. Dr. Wilson does me too much honor in attributing to me, in his second paper, the remarkable experience, in applications of the forceps at the brim, of having prolapse of the cord “in the ratio of twice in three cases.” This experience is not mine, but that of Chailly-Honoré, duly credited by quotation marks and by a marginal reference,—the *ipsissima verba* of an eminent author, whom, as a strong partisan of the forceps *versus* version, Dr. Wilson has quoted no fewer than six times, but whom he wisely quotes no more.

6th. Dr. Wilson says in his third paper: “Simpson informs us that he turned and delivered in two or three minutes,—Dr. Goodell accomplished the same thing in three minutes.” I have never claimed, directly or indirectly, to have “*turned and delivered*” in three minutes; but to have delivered within three minutes after traction and propulsion had been begun,—that is, after turning, after the breech and body had dilated the soft parts and were born, after the arms had been brought down, and the head alone remained for delivery. This mistake I pointed out to Dr. Wilson in my *Reply*, but he has seen fit to repeat it, and I therefore correct it in precisely the same language that I used before.

7th. There are some slight objections to the two cases, viz., that of the Princess Charlotte, and the one reported by Dr. Mayles,—cited by Dr. Wilson as “*illustrative*” of the dangers from shock and from post-partum hemorrhage after “*such rapid labors*” as Simpson and I are alleged to have caused by version. To begin with the less aristocratic case, Dr. Mayles (*Obstetrical Journal of Great Britain*, 1874, p. 405) does not take the trouble to note the presentation, and since also the child was born in a few minutes' time, with “the placenta attached,” and before the doctor and the nurse reached the woman, I think we may venture to assume that the vertex presented and that the child was not turned. These are the only objections to this “*illustrative case*.” With regard to the other case, the child presented by the vertex, which remained *first* throughout the labor, and the Princess died from either shock, hemorrhage, or heart-clot, induced, not by a “*rapid labor*,” but by a very *tedious* one, lasting fifty hours, viz., from Monday at 7 P.M., when the membranes broke, to Wednesday at 9 P.M.

(*London Medical Repository*, December, 1817, p. 536, and *Memoir of the Princess Charlotte*, London, 1818). These are the only objections to this "illustrative case," only these and nothing more. In my humble opinion these two cases, carefully selected from "many illustrative cases" whose "list might be increased indefinitely," need not deter us from turning,—certainly not by their appositeness.

8th. By devoting more than a page of his third paper, bristling with quotations, to show that the child can be kept alive above a rigid perineum by "a very simple expedient familiar to the profession for a century," viz., by conveying air to it,—Dr. Wilson implies that from ignorance of this expedient I "rudely and wantonly" drag "the child's head through the perineum at the risk of maiming the mother, and thereby rendering her a wretched sufferer for the balance of her life." Now, in my paper on *Head-Lost Labor* (*Philadelphia Medical Times*, March 20, 1875), a paper which, in this discussion, Dr. Wilson has gone out of his way to attack, I provide for such a contingency as a rigid perineum, and advise that "if air cannot be communicated to the mouth or to the nostrils of the child through the gutter made by the physician's fingers, he must disregard the consequences to the mother, and forcibly deliver by traction, or, this failing, by the forceps." I also showed, in my *Reply*, that the perineum has been already so stretched open by the half-breech, that no great laceration could at the worst be produced by the passage of the head. Indeed, Dr. Wilson himself admits that "the increased amount of dilatation required for the delivery of the head after the breech and shoulders are born will be *but very slight*." Had Dr. Wilson only had the time to read me attentively, he would have seen that we agree perfectly on this point, and he would have saved the Society much time, and himself much foolscap.

9th. "Are we to accept," asks Dr. Wilson, in his third paper, "on the evidence of a *single* case, the bold statement 'that in nine out of ten cases of rupture into the rectum, the forceps will be found to have been the cause?'" The evidence is altogether insufficient." Now, the evidence that I gave was by no means the "single case," that of Mrs. L., so opportunely offered by Dr. Wilson in his own argument. I adverted to "quite a large number of cases of torn perineum" which I had operated on. But since he calls for "opinions derived from the observation of facts" other than mine, let me cite the testimony of a man who has probably operated for this lesion more frequently than any other physician. Emmett declares (*Medical Record*, March, 1873, p. 121) that "when the laceration has been so extensive as to involve the sphincter ani, and a portion

of the recto-vaginal septum, it is the result *generally of instrumental delivery*, . . . and may occur in the hands of a most skilful operator." Prof. W. T. Howard (*Transactions Medical and Chirurgical Faculty of Baltimore*, 1875, p. 106) contends that complete laceration "is *usually* the result of instrumental delivery." "The forceps is quite often," writes Jacquemier (*Manuel des Accouchés*, vol. ii., p. 309), "not only among primiparæ, but in women who have borne many children, the cause of the more extensive lacerations of the perineum. The complete and very extensive lacerations observed in many women operated on by Dieffenbach and Roux were caused by this instrument at a second, fourth, and fifth labor." Joulin (*Traité Complet*, Paris, 1867, p. 1050) enumerates among the lesions produced by the forceps in the hands of a skilful physician (*praticien habile*) "the lacerations of the perineum, which cannot always be avoided." Madame Lachapelle considers (*op. cit.*, 8th Memoir, p. 140) "the imprudent application of the forceps"—for every one is not "a master with forceps"—as the most frequent cause of complete lacerations of the perineum. She further states (vol. i., pp. 46, 70, and 71) that "in the last stage of labor the forceps *almost inevitably* causes a laceration of the perineum, unless it is withdrawn just before birth." Dewees advises (*Midwifery*, 1853, par. 796) the same thing for the same reason. Finally, let me jog Dr. Wilson's memory by one more citation. Out of seven cases of complete laceration of the perineum operated on by Dr. D. Hayes Agnew (*Penn. Hospital Reports*, 1868), five were positively ascertained to be owing to the use of the forceps. By a curious coincidence I also find that *two* of these five cases were patients of Dr. Wilson's; but I will do him the justice to add that he did not deliver them. Yet, in spite of this fact, he indignantly adverts to "*the evidence of a single case.*"

10th. In his last paper he quotes from W. H. Jones, and very inaccurately, the statistics of Dubois's cases of labor in narrow pelves, and adds: "In two cases where the pelvis was narrowed to *a little more* than 2.5 inches, the forceps afforded the means of extracting alive two children at full term. This table of statistics gives us also five cases of detruncation of the child. *There is no evidence showing that more than one person made traction upon the child's body.*" To this I reply, in the first place, that these two forceps-cases in pelves "*a little more than 2.5 inches*" are as follows: (a) Conjugate measured 2.9 inches; child weighed *four* pounds, and died on the twenty-fifth day; mother died from peritonitis. (b) Conjugate measured 2.7 inches; length of labor 50 hours; child weighed *four* pounds, and had facial paralysis; mother did well. (Jones,

Management of Labor in Contracted Pelves, Appendix). In the second place, the above "five cases of detrmncation," twice triumphantly cited by Dr. Wilson as an evidence that the head may be decapitated by the traction efforts of one person, are, unfortunately for him, not those of detrmncation by traction. They were not *accidents* but *operations*. Dubois, whose cases they were, and other French obstetricians, not understanding the mechanism of head-last labors in narrow pelves, and fearing the extension of the head by traction, amputated the neck close to the head with a strong pair of curved scissors, in order both to gain room for the cephalo-tribe, and to expose the *foramen magnum* as the means of exit for the brain. Had Dr. Wilson read Jones's little brochure more carefully he would not have made this very unfortunate mistake.

11th. With regard to the child's neck, I never said, as Dr. Wilson implies, that the "unaided efforts" of one person are not equal to its luxation, or even to its decapitation. For I have in my Index Rerum references to cases, to which he is heartily welcome, in which the *braceil* and prolonged traction-efforts of a single person effected both luxation and detrmncation. But I gave classical examples in which the efforts of two persons were not able to effect either of these lesions,—one of them, in which the child was born alive, being unwittingly furnished by Dr. Wilson himself. Nor did I assert that *every* child's neck can "with safety" withstand a strain of one hundred and thirty pounds. On the contrary, I offer an example (Case X.) in which, under such a weight *repeatedly applied*, the spinal column finally gave way. What I do say (*Philadelphia Medical Times*, March 20, 1875, p. 387) is, "But I do not believe it possible for a physician even to break the neck of a mature child, much less to behead it, if he applies a *steady* traction-force by *pushing* the neck and body of the child backward and downward, just as he makes downward pressure on the lock of the forceps." For I show that by this method of traction "one can for thirty seconds exert a steady downward pressure of about *ten pounds more than half the weight of one's body*."

Again, I showed from a passage from Hodge, which Dr. Wilson was kind enough to furnish me with, that children have been born alive after "*the strength of two individuals* have been applied to the body of the child, and that of three, four, or even more attendants to the body of the mother." This is an ugly fact to meet—a very ugly fact—but it must be met at all hazards; and how does Dr. Wilson meet it? He is usually frugal of arguments, but here he lavishes two. In the first place, he contends that "Dr. Hodge does not call them physicians," that is, the individuals and attendants tugging away at

mother and child were not physicians. As if traction is not traction unless made by a physician; and as if, by a wise provision of Providence, the fœtal neck readily snaps and parts only under official traction, and not under lay traction. In the second place, he rejoins, *ore rotundo*, that Dr. Hodge, "the great conservator of orthodox obstetrics," does not *sanction, recommend, or in any way countenance* "such a proceeding as just narrated." Pray, what earthly bearing has this answer on the question at issue? Is the human fœtus a respecter of persons? And does it courteously make the tenacity of its neck depend on the *sanction and countenance* of "the great conservator of orthodox obstetrics?" I protest, gentlemen, that after such a specimen of ratiocination, I dare not yield to the temptation of calling this brace of arguments a *non sequitur*, lest I might be met with the retort of the recruiting sergeant in "Tom Jones,"—"You're another, if you come to that. I'm no more a *sequitur* than yourself."

But Dr. Wilson is more happy in another argument, one levelled at my statement that the gallows-drop rarely destroys life by breaking the neck of the criminal, an argument so unanswerable that I fear that, after all, I shall have to yield the point of the remarkable strength of the child's neck. "Notwithstanding this pleasing picture," he urges very forcibly, "and the comforting assurance of Dr. Goodell that the neck may not break, I believe few men receive the noose with pleasure." *Few men receive the noose with pleasure!* How true this is; and yet, to my shame be it said, I have not hitherto thought of this in connection with breech and version cases; but I shall often think of it hereafter. In addition to the *à priori* and *à posteriori* modes of reasoning, medical logicians must in future teach the *sus. per coll.*

12th. In his second paper Dr. Wilson remarks: "You will remember that Dr. Goodell denied this reductibility of the child's head, or the possibility of delivery at term, with a pelvis of two and a half inches, and, greatly to my astonishment, he questioned the truthfulness of Madame Lachapelle's statistics." The cases Dr. Wilson here cites are those of conjugates of $2\frac{3}{8}$, 2.5, and 2.75 inches, and were *natural* labors—"by the efforts of nature alone." Nor have I denied, but positively affirmed, that the unaided, and therefore unhampered efforts of nature, by perfectly moulding the head, will deliver where the forceps utterly fails. As vouchers for this statement I then cited Cazeaux, Chailly-Honoré, and Gardien. To these names I am now able to add Baudelocque,¹ Capuron,² and Desor-

¹ Accouchemens, par. 1607, 1608, et seq.

² Cours Theorique et Pratique, Paris, 1823, p. 515.

meaux,¹ who, although applying the forceps invariably to the sides of the head, contend that this instrument does not imitate nature, which moulds the head better, and adduce two of these very cases in proof. I thank Dr. Wilson for saving me the trouble of citing these cases to substantiate my position. I also thank him for here giving me the opportunity of tendering an humble apology which I owe to the memory of that illustrious midwife, Madame Lachapelle. I doubted the accuracy of her statements in regard to delivering by version in very narrow pelves, because, through culpable carelessness, I overlooked the fact that W. H. Jones gives the conjugates of her cases in French inches. She herself remarks (11th Memoir, p. 436), as I have recently discovered, that "2.25 English inches equal $2\frac{1}{2}$ French inches." The French *pied de roi* measures 12.79 English inches and the *pied usuel*, being one-third of a *metre*, measures 13.12 English inches. I fancy, however, that the *pied du roi* is the measure used by the older French obstetricians, for the decimal system did not at once find favor. This important difference between the length of the French and the English foot must be borne in mind whenever pelvic measurements are cited from French authors, or from those earlier German writers who, like Stein, adopted the *Pariser Zoll*. Even W. H. Jones (*op. cit.*) has overlooked this distinction. For instance, the cases which he cites from Dubois, of conjugates "above 3.5 inches," and "between 3.5 and 3 inches," should read, "above 3.7 inches," and "between 3.7 and 3.15 inches." This correction I beg the reader to make when he refers to these cases in my *Reply*.

13th. A little further on, Homberger and Hasslocher, whoever they may be, are cited by Dr. Wilson as having delivered by the forceps in pelves measuring 2 and 2.25 French inches in the conjugate diameter. Now, apart from the melody and alliteration of these names, I can conceive of no reason why they have been introduced into this discussion. The pelvis in each case was distorted by malacosteon—a disease in which the bones are so flexible as to yield to the pressure of the fingers—and this discussion has nothing to do with this lesion.

14th. Not satisfied with subjecting in his first paper my tabulated cases to exegetical torture, Dr. Wilson makes the child in Case IX. "dead," when it was alive, and is still alive. In Case V. he twice very carelessly ignores the uncongenial fact that I, by version, delivered the woman twice (since thrice) of living children. I may also add that when the child is undersized, its weight is the burden of his refrain, and he

¹ Dictionnaire en 30, Article Forceps, p. 370.

criticises the case with pre-Raphaelite minuteness of detail. When the child is a large one he forgets to give its weight, and either glosses over the case, or else flutters away from it. These are by no means all the evidences of his carelessness that I could adduce, but I do not wish to weary your patience. I will simply ask you, when reading this discussion, to examine for yourselves whenever Dr. Wilson says that I *deny* or *assert* anything.

Finally. I have before referred to Dr. Wilson's respect for Moses and the prophets, and am, therefore, not surprised to find him impressively, and very irrelevantly, quoting the sixth commandment, "Thou shalt not kill!" Let me hasten to assure him that I also reverence the decalogue, and that remembering moreover the words of the apostle, that "Whosoever shall keep the whole law, and yet offend in one point, he is guilty of all," I strive, while humbly trying to keep the sixth commandment, not to fail in observing the ninth also.

QUARTERLY REPORT ON OBSTETRICS, GYNECOLOGY, AND PÆDIATRICS.

By

DR. C. WILLIAMS.

OBSTETRICS.

1. AGAINST THE PENDULUM MOVEMENT IN WORKING THE MIDWIFERY FORCEPS. By J. MATHEWS DUNCAN. (*Edinburgh Medical Journal*, Feb., 1876.)

The object of this paper is to publish what the author has long taught, viz., that it is bad practice to use the rotary or pendulum motion of the forceps in the extraction of the fœtal head. It is not intended that the objections shall apply "to changes in the direction of traction by the forceps, such as may be required according as the dragged head descends, or such as may be called for when the head has been previously inefficiently dragged in a wrong direction." He denies that the pendulum, oscillatory, or leverage-like motion of the dragging instrument leads to economy of force—claims that the opposite is true, and that such motion is injurious. That if thereby economy of force was gained, it is not needed, for the accoucheur has practically a boundless supply, "greater than he is willing to employ." The lever and double rack hypothesis is too flimsy to get hold of. "There is no toothed rack on the wall of the pelvis," or roughness to take its place; if there were, the worst use that could be made of them would be to make them assume the function of a rack. The head may be seized firmly and made to revolve as upon a pivot, but without simple traction it would not be advanced. Pulling the head down upon one

side and then upon the other is inutile, the pressure exerted and the traction are probably greater, certainly not less than if simple traction be used—it is an unnecessary movement, imitating the lever and double rack without a trace of its usefulness, and has no analogue in natural or morbid parturition.

Corks are extracted from bottles in this way without the aid of a cork-screw, but could be better and easier extracted by simple traction.

There is not an analogy between the sides of the pelvis and the neck of a bottle.

The idea that there is saving of force, as concerns the parts of the mother and child, in the pendulum or oscillatory movement, is considered as refuted by its statement.

There is the mechanical difficulty of bringing the child's head through a resisting passage, not to be evaded by changes of position; per contra, it may be supposed that the position is most favorable to propulsion. Simple traction does the work, which cannot be diminished by any complicated motion below the expenditure of force requisite. The most important forceps cases are where the obstacles to progress arise from hard parts, and it is here that the greatest injury is done to the maternal structures and those of the child by the pendulum and oscillatory movement where the head has to be dragged and moulded between the promontory of the sacrum and the pubic bones; the combination of squeezing and wriggling much aggravates the unavoidable mutual pressure.

The practitioner should confine himself to simple pulling, as contributive to the forces which nature provides for the expulsion of the head. An appeal to the extensive experience of the author and other practitioners would only be evidence to show that the pendulum movement is not necessary, and not that the abstaining from it is the better plan; both plans are used by good practitioners. No available method is at present suggested whereby the results of experience can be made suitable for the final settlement of the matter.

2. MISSED LABOR. FETAL SKELETON RETAINED IN UTERO SEVEN YEARS.

By WM. ALEXANDER FREUND, of Breslau. (*Berlin, Beitr. z. Geburtshilfe u. Gynäkol.*, iv. 1.)

K. St. æt. 44, peasant woman, came on June 11th, 1875, with a letter from Dr. Geldner of Pitschen, stating that patient had borne six children, labors all normal, last child born eight years ago. Seven years ago patient was taken about the end of the sixth month of her seventh pregnancy with hemorrhage and slight pains. Patient remained only three days in bed, and with administration of some medicine internally, without especial care regarding diet, the pains and bleeding disappeared; then began a thick offensive discharge, mixed at first with stinking tissue-threads; it became gradually thinner, odorless, and acid. Occasionally, for a year, "little bones" had been discharged by the vagina.

The ichorous discharge still remains and is very abundant.

During these seven years the woman has had excellent general health, performed her heavy household duties without interruption, only menstruation had not reappeared.

The woman, careless about such matters, would never have sought medical aid had it not been that, somewhat suddenly, after several days of considerable dysuria with drop-wise evacuation of the bladder, in March, 1875, absolute incontinentia urinæ occurred.

Two fetal bones were then removed from the cervix uteri with forceps (femur and tibia). The remaining portion of the skeleton within the uterus could not be removed.

Examination of the otherwise healthy woman disclosed the condition of the external genitals which is usual in long existing urinary fistulæ, everything swollen, covered with incrustation, erosions, and painful fissured ulcers. The ammoniacal-smelling whey-like urine flowed continually from the vagina.

By vaginal exploration, by pressure upon the anterior wall of vagina, as well as by application of the catheter in several directions and expulsive efforts

upon part of the patient, a pretty regular quantity of above-described urine (about 50 gramm.) could each time be obtained.

Careful inspection of the vagina and vaginal portion of uterus showed the former entirely whole and the fluid flowing from the latter. The sounding of the *læx* urinary bladder through the not unusually large urethra disclosed the bladder incompletely divided into an upper and lower chamber by a septum of mucous membrane arising from its posterior, and not quite reaching its anterior wall; passing over this, a rough hard body is struck, giving the sensation of a stone.

Through bimanual examination is found a moderately elevated uterus, size of man's fist, much anteverted and fixed in this position; walls hard and infiltrated; os round and admits finger. In the cervical canal are sharp-cornered bones, lying irregularly one upon another.

By aid of a Sims's speculum (lateral position) and strong forceps, especially the American bullet forceps, twenty portions of the skeleton were at once, and at other sittings, removed, some recognizable, others not. The bones were covered with a small quantity of purgent sebaceous-like matter, and occasionally a shell of hard incrustation. Cervical canal considerably elongated (four or five centimetres).

Above the internal os, a rough, firm circular body, but little moveable, implanted in the anterior wall of uterus, surrounded by a slightly raised wall of mucous membrane. Frequent explorations prove this body to be the posterior end of the one felt in the upper chamber of the bladder. It is clear that we have to do with a double-buttoned obturator in a large utero-vesical fistula.

June 18th, with patient in the side position, a strong uterine sound was passed into the upper chamber of the bladder, and the index finger of the right hand into the canal of the fixed cervix uteri, the foreign body was raised by means of the sound, and finally pushed into the bladder.

A somewhat oblique fistula was felt by the finger, which included the entire width and upper part of the cervical canal, opening directly into the upper chamber of the bladder. At once a multitude of very portentous symptoms arose, expulsive efforts of the abdominal muscles attended, upon the examining chair, by copious and forcible emission of the contents of the bowel and bladder, extrusion of the mucous membrane of the rectum, and inversion of the walls of the vagina; the body was bathed in sweat, when, pale and trembling, the woman was carried to the ward groaning with pain, not before it had again been determined that the foreign body lay moveable in the upper chamber of the bladder, though the door of communication was sensibly wider and covered in part by the concretion.

Within a few hours the continuous vesical and rectal tenesmus had rendered the condition of the patient almost unbearable. Symptoms of inflammation of the viscera were developed. At nine P.M. with the assistance of PROF. FÖRSTER KEMPNER and DR. B. M. FREUND, Prof. Freund proceeded to the extraction of the foreign body—first, and unsuccessfully (for want of appropriate instruments), by crushing and extraction through urethra. Under these attempts the bleeding from the much torn mucous membrane was continual.

The urethra was then cut with scissors, from before backward through the neck of the bladder, and the stone extracted, and the wound closed at once with silk sutures.

After twenty-four hours, during which the pain and fever were inconsiderable, the incontinence of urine had entirely disappeared. Three days afterwards the wound had healed entirely by first intention, and the stitches were removed.

The uterus is contracted to size of child's fist. "The utero-vesical fistula is closed, of course not healed, but in all possible positions of the body, and after several examinations, not a drop of urine passed through it. Assurance of the manner and firmness of the closure is obtained through the sound."

After three days more, the woman left for home, cured. A letter from Dr. G., August 11th, reports her as having continued well; especially is the evac-

uation of the urine entirely normal. "The body removed from the bladder has the shape of a sheaf, and the following measurements: longest diameter 3.9 ctm., greatest breadth, 2.2 ctm., least 1.9 ctm., thickness of the vesical end 2 ctm., the extra vesical end 1.6 ctm." It is composed of concentric layers of urinary salts, of grayish color; for about 3 mm. externally, small sharp points, next a layer of 1 mm. thick, stalactiform incrustation light yellow color, similar to the coating upon the bones taken from the uterus. "The nucleus of the whole is recognized as the lower extremity of the left femur."

3. ON THE SECTION OF THE UMBILICAL CORD. (*Annales de Gynécologie*, Jan. 1876). M. Budin has sought to determine, by numerous experiments, the moment most favorable for ligation of the umbilical cord. He concludes that if the cord is tied immediately after the birth of the child, he is deprived of about 92 grammes of blood contained in the placenta, equivalent to a bleeding with loss of 1,700 grammes in the adult; hence it is better to wait until a minute or two shall have elapsed after complete cessation of pulsation in the cord. M. Budin has been able to assure himself that the placenta void of blood and doubled upon itself, passes the cervix much easier than if filled and extended.

3. RETROVERSION AND RETROFLEXION OF THE PREGNANT UTERUS.

Some additional practical observations were contributed by the late Prof. E. MARTIN, of Berlin, to the *Zeitschr. für Ger und Frauenkr.* vol. 1, No. 1. He says that the diagnosis of retroflexion of the gravid uterus may be confounded with other conditions, and is in fact difficult. He mentions retro-uterine hæmatocele, tumors, or other enlargements behind the posterior wall of the uterus; for instance, extra-uterine pregnancy in Douglas' *cul-de-sac*, joined with absence of menstruation for several months.

The difficulties are only to be overcome by careful and long observation, and finally by the cautious introduction of the uterine sound.

In the *healthy* bladder, if unclean instruments be not introduced, the urine, though retained for many hours, will not be decomposed. M. claims particularly, what has been insisted upon by others before, that a male catheter be used for emptying the bladder. One of his cases belonged in the category of spontaneous reduction of the retroverted gravid uterus. A thirty-two year old, strong laboring woman, in the fourth month of her third pregnancy, after unusual straining in lifting, felt pain in lower abdomen, difficult micturition, which finally passed into complete retention. Upon examination, a long, smooth, tense elastic swelling was found, extending 17 ctm. above the symphysis, to the inner side, a retroverted gravid uterus. With the male catheter 1450 c. ctm. of urine were evacuated.

Immediately afterwards the finger was introduced—patient retaining the position upon the back—and the vaginal portion of the uterus, with the os, was found directed backward and downwards, and anteriorly the anterior wall of the womb enlarged by the attached placenta. After keeping the side-abdominal position for several days, and urinating without difficulty—the local conditions corresponding fully to the fourth month of utero-gestation—patient was discharged from treatment. A short time after it was again demonstrated, by retention of urine and considerable distention of the bladder, that the womb was retroverted anew. The reapplication of the catheter in the supine position was again followed by immediate return of the uterus to the position of anteversion. The further course of the pregnancy was undisturbed.

M. contends that this result is accomplished through agency of the round ligaments.

4. EFFICACY OF THE MILK DIET IN THE ALBUMINURIA OF PREGNANCY, AND ITS INDICATION AS A PREVENTIVE OF ECLAMPSIA. By DR. TARNIER, of Paris. (*Annales de Gynécologie*, Jan., 1876.)

Dr. Tarnier has been very successful with the milk diet in albuminuria of

pregnancy; he has only seen it fail to produce a cure in one case; and in this the post-mortem showed advanced Bright's disease.

His *résumé* of the formula of administration is:

1st day, 1 litre of milk¹ and two portions solid food.

2d day, 2 litres milk and one portion solid food.

3d day, 3 litres milk and one-half portion solids.

4th, and the following days, 4 litres milk, or at discretion, without other beverage.

In grave cases, where symptoms of eclampsia were evident, the patient was put upon three or four litres of milk a day at once, without the above gradation.

The influence of the milk regimen never failed to manifest itself in one or two weeks, either in the very notable diminution of the albuminuria, or in the cure of the patient.

Dr. Tarnier is convinced that if the milk regimen is enforced *in time* that eclampsia may always be prevented. He has never seen convulsions occur in a case so treated, but admitted the desirableness of a large number of observations; his own amount to about seventy cases

5. FIBRO-MYOMA IN THE VESICO-VAGINAL SPACE, OBSTRUCTING DELIVERY; SPONTANEOUS EXTRUSION; ARTIFICIAL ENUCLEATION. (*Annales de Gynécologie, Jun., 1876.*)

DR. EDUARDO PORRO reports the above case from the Maternité of Milan.

Patient æt. 26, primipara; previous history good; admitted October 11th, 1875. A month before had twice considerable hemorrhage from vagina, with interval of eight days between; labor begun at midnight of the 11th; examination revealed the following: pelvis well formed; uterus right antero-lateral oblique; contractions frequent, long, very energetic; foetal pulse feeble, irregular, very frequent, heard to the left and low down; amniotic fluid and meconium flowing from vagina, which is very sensitive; urethra very large, distended, cannot be followed to superior surface of the symphysis, because behind this is felt a smooth tumor, fibrous consistence, occupying all the anterior, the right, and a part of the left side of the pelvis; the finger carried posteriorly toward the concavity of the sacrum finds the os dilated to about six centimetres; the head found presenting probably occipito-iliac, left anterior; the catheter was passed, after some resistance, vertically upwards, and bladder emptied. The general condition of the patient being good, it was decided to wait. At 9.30 the tumor appeared outside the vulva, upon a level with the anterior commissure, and resembled the head of a foetus; contractions continuing energetic, the tumor emerged entirely and remained firmly fixed in the anterior commissure under the pubic arch. The head was soon born, without rotation, and emerged occipito-iliac left, transverse; the body soon followed, and the labor terminated at 10.15; child dead; weight 1840 grammes; seemed at about the end of the eighth month.

The tumor's surface is smooth red-brown, with ecchymotic slate-colored patches; fibro-cartilaginous consistence fluctuating at some points; has the form of the foetal ellipse; antero-posterior diameter 104, transverse .09 centimetres, circumference 324 centimetres. Examination finds posterior vaginal wall normal, while the anterior is extended, tense, and seems to present a pedicle binding the tumor to the antero-inferior portion of the cervix. The mouth of the uterus is quite free and regular; the meatus is drawn downwards and forwards; one seems to be able to recognize the pedicle of the tumor with the sound through the posterior wall of the bladder. Upon the third day, the tumor having notably diminished in size, it was found that it was covered by the mucous membrane of the bladder, and did not therefore come from the cavity of the uterus. A careful examination discovered that the tumor must originally have been sub-peritoneal, and behind the bladder,

¹ Equals $\frac{3}{4}$ viii. troy.

upon the vagina, and a definitive diagnosis was made of *pediculated uterine fibroid*.

The mucous membrane of the vagina beginning to become gangrenous, two curved incisions were made in the median line; the mucous membrane dissected up, and the tumor, *without any pedicle* or adherence to the uterus whatever, was easily enucleated. It weighed 332 grammes, circumference 284 centimetres; and had the microscopic characters of fibro-myoma.

Fifteen days afterward, patient left hospital nearly cured; and on 7th Nov. is quite well, only a tendency to prolapse of anterior vaginal wall when the patient walks much.

GYNECOLOGY.

6. CASE OF EXCISION OF THE UTERUS, AS A CURE FOR TOTAL PROLAPSE.

By ALEXANDER PATTERSON, M.D. (*Glasgow Medical Journal*, Jan. 1876).

Patient at 49.—married 26 years, nine months after birth of seventh child suffered from "falling down of the womb," this has continued ever since. For last three years (August 17th, 1875) the procidentia has been constant, and latterly, dating from an attack of diarrhœa, the tumor has increased rapidly in size. For three months patient has seldom left her bed; could neither stand nor sit without much discomfort. Catamenial discharge regular.

The tumor, found projecting between the woman's thighs, is larger than the human heart, cordiform in shape, has an opening at its lower part, is consistent to touch, and covered with a semi-mucous skin resembling the scar left from a burn. The finger cannot be passed higher than half an inch at any part of circumference.

On Aug. 24th patient was put under influence of chloroform, tied in the position for lithotomy; a catheter being introduced turned downwards, until its point could be felt near the lowest point of the mass. The finger placed into the rectum was found to take a like course on the posterior aspect of the protrusion, indicating a falling down of the entire pelvic viscera.

A horizontal incision was made, and the bladder carefully dissected off from the front of the womb, and the same process repeated with regard to the rectum behind.

The uterus was then separated from its superior attachments and removed entire; the peritoneal cavity being laid open. There was little hemorrhage. Four wire sutures were introduced, the ends being left long.

The bladder and rectum were then carefully replaced, and the four sutures fastened to each thigh by means of adhesive plaster. Eleven and a half hours after the operation, patient has had some pain, has vomited, abdomen distended, not painful on pressure, no blood.

Sept. 4th, patient has progressed well, chloroform administered; patient tied as before, wire sutures removed, edges of the labia, from the fourchette to within an inch of the meatus urinarius, pared and brought together by deep and superficial wire sutures.

21st, Sutures removed, firmly united. Patient expressed herself very grateful for the cure of her distressing deformity.

7. ON ABLATION OF THE BODY OF THE WOMB IN IRREDUCIBLE UTERINE INVERSION BY EXTERNAL HYSTEROTOMY. By M. DONNÉ, of Paris. (*Annales de Gynécologie*, Jan., 1876.)

M. Donné presented a paper to the Academy of Medicine, Paris; Nov. 9th 1875, upon the above subject, of which the following are the conclusions:

1st. External hysterotomy is an extreme surgical resource, but precious, in cases of irreducible inversion menacing the patient with immediate death.

2d. This operation does not offer a greater mortality than most grave operations.

3d. In the present state of science, it should be done preferably by ligature, taking into account the improvements which have been brought to this method.

4th. In the first months of an inversion, and even the first year, there should be, as often as possible, repeated attempts at reduction; lactation, which generally suppresses hemorrhages, should be performed, and there should be used palliative measures of all sorts.

5th. The operation should be reserved for the cases known as irreducible; and for that epoch, already distant from the beginning of the accident, when the inverted uterus has completely returned upon itself and has taken a new and definite form, supported by the neighboring organs, thus leaving much less to fear from peritoneal inflammation, which is to be avoided at any price.

8. MEMBRANOUS DYSMENORRHOEA. By DR. LOUIS MAYER. (*Berl. Beitr. z. Geb. u. Gyn.*, IV. 1.)

M. contributes the results of an extended experience in dysmenorrhœa, with development of a deciduous membrane, six cases being recorded as occurring in a term of years; he concludes that the treatment must depend much upon the preceding disease to which the *dysmenorrhœa membranacea* owes its existence, viz.: metritis, chronic endometritis, peri- and para-metritis, and constipation. His remedies were local blood-letting, evacuates, general baths, simple and medicated, topical injections, in case of anæmia, iron; against pain, hypodermic use of narcotics, or by means of suppositories, or through the stomach.

Rapid dilatation of the external os, and the canal of the cervix, by compressed sponge or laminaria, intra-uterine injections of tinct. iodine, carbolic acid, sol. argt. nit, and chloride of iron, etc.

In some cases intra-uterine injections did harm, especially sol. nitrate of silver. Dr. Mayer concludes with the remark that, under all conditions, the hope must not be cherished that these cases will be cured by a few applications, but that much patience must be exercised both by physician and patient.

The cases reported upon were benefited.

9. HORSE-SHOE KIDNEY, WITH ABSENCE OF THE INTERNAL ORGANS OF GENERATION. By PROF. WILLIAM ALEXANDER FREUND, of Breslau. (*Ibid.*)

On the seventh of June, 1875, Mrs. J. Z., age 32, married nine months, presented herself; complained of malaise, nervous prostration, anorexia, cough, and loss of flesh.

All these symptoms had appeared since marriage. First marital approaches had been very painful and fruitless as well; gradually intercourse had become practicable. Never the slightest sign of menstruation.

Through the persuasions of her parents, against her own will she consulted a physician concerning her physical fitness for marriage.

The reply was, the husband will soon overcome the slight obstacle which closes your womb. Intercourse at first afforded only pain; later, however, pleasure, and in this respect both parties now appeared satisfied. She applied solely on account of her general bad condition, which she ascribed to the absence of menstruation.

The woman was a brunette, lean, pale, heavy eyebrows, considerable mustache and side whiskers, rough, hairy legs, strongly developed pubis, mammae lax, with few glands, in general build feminine. Heart healthy, some circumscribed catarrh of the small bronchi, liver small, and crossed by transverse colon anteriorly. By close examination it was found that the lower border of liver was sunken unusually toward the back. Spleen somewhat enlarged. The flaccid condition of the abdominal walls rendered it possible to press the examining hand high up into the "kidney corner," yet even upon the deepest expiration no indication of these organs could be detected.

Mons veneris, labia majora, and nates remarkably devoid of fat; vaginal opening not entirely concealed by labia majora, labia minora and unusually developed clitoris lie in plain view. Urethra normal; anal opening, because of abnormally short perineum, too far forward; vaginal opening narrow, surrounded

by very flat carunculæ myrtiformes ; upon digital exploration the finger is arrested at about 3 ctm. within by a *cul-de-sac* ; no cicatricial tissue ; upon pressure the sac may be made about 1 ctm deeper ; urinary bladder normal ; the urine withdrawn normal.

Examination by rectum, and through bladder with catheter, and by rectum and hand upon abdomen, detected not the slightest sign of internal organs of generation. Repeated digital examinations by rectum finally discovered upon the posterior wall of the bladder, and capable of being pulled backwards from it, a fold of very insignificant thickness and apparently lax construction.

The fold is found by bimannual palpation to be higher posteriorly than anteriorly ; its height may be estimated at from 2 to 3 ctm. It soon became evident that this fold turned laterally in the form of a shallow arch, its concavity posteriorly, left to the left wall of the rectum, right to a firm body which lies tolerably fixed upon the posterior wall of the pelvis.

This body from the rectum and then bi-manually from rectum and abdomen, may be minutely palpated, has the smoothness and consistence of the anterior surface of the kidney, is flat-round-like, and possesses the precise form of a not quite symmetrically formed bean, with its convex surface looking downwards. It lies near the wall of the pelvis, is slightly moveable. The middle portion (in comparison to the other portions, flat) reaches hardly above the promontory, and may without trouble be brought within reach of the finger by downward traction ; the right well-filled out extremity extends into the right posterior quarter of the pelvis ; the smaller, shorter, slenderer left, into the corresponding left portion of the pelvis, where they cover a portion of the inner surface of the sacrum and the soft parts of the sacro-sciatic notch.

From the downward directed concavity of the body, one feels three deep notches upon the anterior surface, which diverge upwards, and are gradually lost. The abdominal aorta is felt very plainly upon the side of the spinal column downwards to the described body, *behind* which it passes. The rectum lies with its upper curve to the left of the left extremity of this body, with its anterior and inner part for a distance upon this extremity. Upon the right extremity is inserted, as above mentioned, the fold between the bladder and this body. This circumstance is of the greatest importance, because it proves indubitably the extra-peritoneal position of these bodies.

Two attempts to pass a sound into the pelvis of the diagnosed horse-shoe-kidney failed ; the sound passed about 3 ctm. to the right upward.

10. INTERMENSTRUAL PAIN (MITTELSCHMERZ). By DR. FASBENDER. (*Zeitschrift für Geb. u. Frauenkrankh. Martin & Fasbender*, vol. 1, No. 1.)

Patient æt. 24, single, has menstruated regularly since fourteen. Flow has always been spare, attended by severe pain in abdomen, and gastric disturbance, preceded by *malaise*, shivering, and flushes of heat ; countenance expressive of anxiety ; for years moderate leucorrhœa. For two years has had exactly the same pain about the middle of the menstrual interval as is experienced with each flow, accompanied by symptoms of congestion of the genitals. Because of long suffering, unrelieved by medical treatment, patient is exquisitely hysterical, approaching to melancholia. Uterus sharply antelected, endometritis, chronic colpitis, erosion of the os. Examination during the time of this intermenstrual pain reveals discharge from the vagina freer, and parts much congested.

By long use of intra-uterine pessary, the condition was bettered, especially mentally, and the inter-menstrual pain was entirely relieved.

11. ENUCLEATION OF INTRA-PARIETAL TUMORS OF THE CORPUS UTERI. (*Ibid.*)

A. MARTIN reports five cases, part of them his own. It is considered important to wait with the radical operation till the uterine bed of the tumor is distended and its walls thinned.

The patient is to be prepared for the loss of blood by appropriate measures, and the procedure delayed till the lower segment of the uterus is prepared for the operation by the contractile pains. The position

of the patient is to be governed by the position of the tumor; if in the posterior wall, the position for stone operation is recommended; if laterally, the patient is placed upon the side corresponding to the tumor. "The patient being anesthetized, the capsule is freely split with the sheathed knife of Paul Dubois. If the capsule gapes after the incision, one or two fingers may at once be pushed between it and the tumor; if it does not gape, material aid is gotten by strong outward dragging; thus inducing gaping, this always succeeds when the opening is large enough." In the author's cases, the bleeding was slight when the capsule was opened, after which an immediate attack upon the tumor is preferred, especially in those that are rich in muscular fibres. Martin only used the finger in enucleating, introducing the whole or half hand into the vagina. Instruments are to be feared as dangerous, even the enucleator of Sims, for the capsule may be penetrated, and the peritoneum or bladder wall be wounded. In the enucleation itself there appeared to the author two principal difficulties:

1st. It is difficult to reach the upper part of the tumor after pulling off the lower portion.

2d. Among all the known traction instruments which may be employed here, none of them answer to the claims made for them.

To obviate the first the author was obliged to amputate a piece of the tumor, when enucleation was easily accomplished. For the second he had constructed straight forceps (like obstetrical forceps), with short, strong teeth at the end; one blade is introduced at a time, and the teeth, which are the while sheathed, projected by appropriate mechanism. The relative mortality in the operation in question has of late been materially lessened from that given by West at 50 per cent. Sixty-six new cases collected only show a death rate of 12 per cent.

12. CASE OF MULTIPLE FIBROIDS IN A VIRGIN UTERUS—ENUCLEATION.— (*Ibid.*)

P. RUGE contributes the following:

Patient, æt. 39, virgin; had suffered for a long time with profuse menstrual flow and obstinate constipation. For fourteen months, it was said, patient had had constant profuse bleeding.

Examination discovered eight or ten fibroid tumors, from size of a plum to an apple; some sub-peritoneal, some intramural; body of uterus not immoderately enlarged. At first, subcutaneous injections of ergotin had good, later no result.

Ruge determined about the middle of May, 1874, to dilate the cervical canal, and for this purpose compressed sponge and sea-tangle tents were used for several days—patient being then much debilitated; the os was dilated so as to admit two fingers, when a tumor, size of a small apple, was felt in the upper part of the posterior wall, its greater portion projecting into the uterus. The capsule was—under chloroform narcosis—split with Simon's curette, when with some trouble the tumor was shelled out, seized and extracted with the forceps of Muzeux. A second tumor, size of small apple, was then observed in the anterior wall, which was treated like the other, save that when the shelling out was nearly completed, uterine contractions came on—the operation having lasted nearly two hours—so that it then became impossible to introduce more than one finger in the os. An attempt at extraction with the forceps of Muzeux was unsuccessful, and the operation was suspended. In the first days, patient felt well and recovered some strength; at the end of this time, however, an exceedingly stinking discharge appeared from the vagina; upon the third day there was active fever; and upon the fifth day, after some short pains, a putrid disorganized fibroid was expelled. The patient then rapidly became well and soon quitted her bed; the menses appeared three weeks after the operation, and have continued regularly every four weeks, lasting from two to three days, and not profuse.

This case is interesting because of the number of tumors, the manner in which ergotin acted, and particularly because dilatation was undertaken without any previous preparation of the genitalia.

13. ON THE STERILITY OF WOMEN AFFECTED BY DISEASE OF THE SEXUAL APPARATUS.

O. VON GRÜNEWALD (*Arch. f. Gynäk.*, VIII. 3, 1875. *Centr. bl. für med. Wissensch.*, 1, 1876) considers that, as obstacles to conception, impediments to the union of the semen and ovum play a very subordinate part, *save when there is complete atresia*. He instances the great fruitfulness of some animals in which the vagina and uterus join each other at a very acute angle, and supposes that conception in such cases can only occur as the result of self-propulsion of the spermatozoa.

G. believes almost the unique cause of sterility to be disease of the tissues of the uterus, incapacitating it for the nourishment of the ovum. Among 900 married women suffering from disease of the uterus, and not past the menopause, 50 were sterile; and in over 50 per. cent the disease consisted in an inflammatory affection.

It is well known that pregnancy may be interrupted by acute disease; it is even probable that chronic general debility impairs the developmental power of the ovum.

In endometritis the cervical canal is generally wider than usual; menstruation is not disturbed; the secretions are little dangerous to the spermatozoa as, for instance, in carcinoma uteri, yet the disease leads to sterility as soon as the greater portion of the mucous membrane of the corpus uteri is affected. Certainly, when the connective tissue is disorganized and degenerated into a thin layer. Mesometritis obstructs the nutrition of the ovum in proportion to the mass of tissue involved—it is most frequently a sequence of parturition, hence many women thus suffering remain sterile after one pregnancy; while among those rendered barren by endometritis, 8.4 per cent. are cured and conceive again, only 3.1 per cent. among those afflicted with chronic metritis become again fruitful.

To para- and perimetritis, frequent complications of other inflammatory forms, may be attributed at least 10 per cent. of the sterile women. They occasion disturbance alike of the nutrition of the organ and its position, beginning often in the first weeks of marriage, and having frequent accessions.

The position of the exudation is probably of greater influence than the amount. Probably the re-occurrence of pregnancy, in some cases, is due to gradual development of collateral circulation.

Stenosis of external os never exists idiopathically, but remains from former inflammatory processes the same as conoid cervix (endometritis, with conglutination, connective tissue hyperplasia, vicious positions), and these only occasion the unfruitfulness.

Slitting heals the vice of nutrition; and thereby (but seldom) the sterility also. Malformations of the cervix always develop themselves through previous catarrhal or inflammatory impressions.

In relation to the hurtfulness of flexions, the author adheres to the views of Scanzoni. The sound passes without force the point of flexion, and it is fair to presume that the canal is patent to spermatozoa; conception is therefore not infrequent.

Primarily, orthopædic treatment may relieve the disturbance of nutrition, but seldom gives a good result, intra-uterine stems certainly bring about endometritis and erosion of the mucous membrane, when long used.

Neoplasms do not prevent conception, so long as they are confined, and do not induce participation of the mucous membrane; the latter, though, they always do in the end, and their removal is generally only undertaken after they have long existed, therefore pregnancy rarely succeeds them.

Inasmuch as the disturbing influence of these diseases is most felt when the greatest claims are being made upon the uterine mucous membrane for attachment and nutrition of the ovum, so it is found that then, *i.e.*, in the third and fourth months, the most abortions occur, and may easily become habitual.

REVIEWS AND NOTICES OF BOOKS have been unavoidably crowded out of this number, but will all appear in the August number.

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ORIGINAL COMMUNICATIONS.

INCISION AND DISCUSSION OF THE CERVIX UTERI.¹

BY

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IF "meddlesome midwifery is bad," meddlesome surgery is not less so; and that form of it which attacks the uterus is the worst of all—since it not only injures the patient herself, but also compromises, and perhaps destroys, her prospects of offspring. Trachelotomy,² or cutting the cervix uteri, has been of late so indiscriminately, and often so unnecessarily performed, as to suggest this general remark. And though perhaps somewhat less common, and with some operators less severe, than five years ago, a reference to the most authoritative treatises on gynæcology shows that it has not yet reached its legitimate limitation, even among gynæcologists; while many general practitioners operate as frequently and as blindly as ever—it being so facile a procedure that nobody hesitates to attempt it.

¹ Read before the New York Academy of Medicine, June 1, 1876.

² From *τραχηλος*, neck, cervix, and *τεμνω*, to cut or incise.

But Trachelotomy, in some form, must continue to be recognized in the treatment of many cases of dysmenorrhœa and of sterility, when depending on stenosis of the cervical canal; and the least hazardous, if equally curative, should be preferred. I propose to consider its usual methods and their uses, abuses, and actual value; and also to explain a new method, which, I maintain, includes all that is valuable in them and still more, without their objectionable characteristics.

The two authoritative methods of Trachelotomy, hitherto practised, with some modifications hereinafter to be specified, are:—

I. By Simpson's Metrotome, or some modification of it—deep incision of the cervical canal.

II. By the scissors—discission of the cervix, or Sims' operation.

To these I add a third method, first suggested by myself, and which I will designate as "superficial trachelotomy."

Each of these methods will be separately considered.

I. DEEP INCISION OF THE CERVIX UTERI. SIMPSON'S OPERATION.

Prof. Simpson, of Edinburgh, maintaining that the stenosis producing dysmenorrhœa and sterility exists usually at the internal, and not at the external os uteri, in 1844 devised his metrotome for overcoming the constriction. It is shown by Fig. 1, and is too well-known to require any special description. I improved it, I think, some fifteen years since, by lengthening the sheath to the extent of three-quarters of an inch beyond the blade, so that the full strength of the fingers can be brought to bear upon the blade without displacing the sheath, in case the uterine tissue requires to be divided without the application of traction at the same time. Subsequently, Dr. Greenhalgh, of London, proposed a two-bladed instrument, it being merely a double, as Dr. Simpson's was a single "*bistourie cachée*." This divided the cervix symmetrically, or very nearly so, as Dr. Simpson's instrument did *not*, except by chance—and, generally, more extensively than the latter was intended to do. I might also mention several other metrotomes devised in this and in other countries, were they of any special importance to my present purpose. These are almost all double-bladed, and act on the same principle as Dr. Greenhalgh's instrument, which

soon became generally preferred to Simpson's. I therefore speak of its action especially in what remains to be said under this head; there being, in fact, no practical difference between its effects and those of Simpson's metrotome, when freely used, as by its originator himself.

Advocated by so able a defender, Simpson's operation soon became quite common in Great Britain and this country, though it was not accepted on the Continent till 1860. Its range of application also became extended. For while, rationally, it was at first invoked only in cases of stenosis of the cervical canal, including of course the two ora, it ere long became common enough in cases in which no obstruction at all had existed, and as a mere matter, as it were, of fashion. During a sojourn abroad in 1866, I witnessed a number of operations which I could place only in this category; and several of the same kind have I since seen at home. It seemed to be generally assumed that the uterus is an organ quite indifferent to cutting and hacking, and that the deep incision would, at any rate, do no harm. Indeed it was known that Dr. Simpson had often performed the operation at his consulting-rooms, and afterwards sent his patients home in a cab; and I cannot learn, up to the present time, that he ever reported an adverse case in his experience. It is, however, stated, on unquestionable authority, that some of his patients died in consequence of the operation, and others narrowly escaped death;

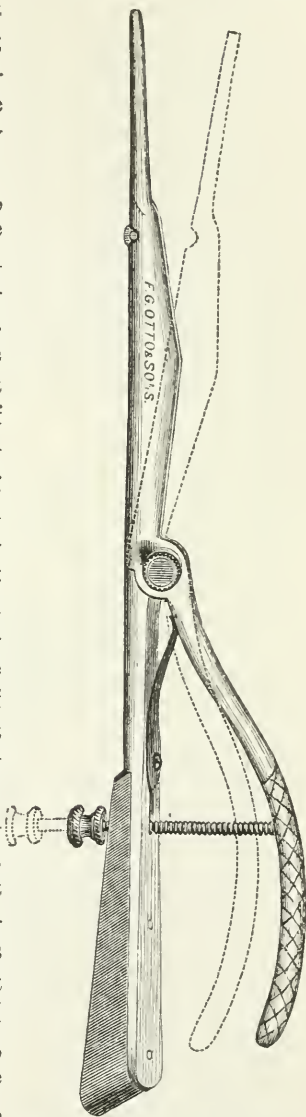


Fig. 1.—Simpson's Metrotome, one-third size.

and it is proper that I here definitely specify its immediate and remote effects. Of course, it is to be recollected that I am speaking of deep incision of the cervix, as performed by means of Greenhalgh's metrotome, or of Simpson's, in a bold hand like that of its originator. And I have to consider:

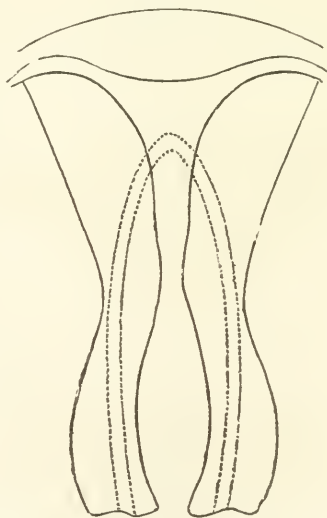
1. The change in the shape, size, and relations of the whole uterine cavity by the deep incision.

2. Its immediate dangers.

3. Its remote effects.

1. *The change in the shape, size, and relations of the cervical canal, produced by the deep incision, are shown by Fig. 2, where the normal shape and size of the canal are seen; while the dotted lines outside of it show the "smaller and the larger incision," as Dr. Hewitt calls them, made by Dr. Greenhalgh's instrument.*

Fig. 2.



Large and small cut by Greenhalgh's Metrotome.—HEWITT.

It is seen that the internal os after these incisions is somewhat more than three times its normal width, while the rest of the canal is increased in nearly as great proportion; and that the whole cavity of the uterus, cervix and corpus together, no longer retains its normal form as shown by Fig. 10; but resembles an erect, wide-necked, flattened flask without a bottom,

(Fig. 13.) We shall see whether such a cavity can be depended upon, as a receiver or as a retainer, farther on. This operation ignores the importance of the normal relations of these cavities, even more than would one, were such a procedure possible, which should permanently dilate the urethra to the size of the small intestine. I fully assent to Dr. Sims' criticism, that this operation cuts altogether too extensively; an objection which will, however, be seen to apply as truly to his own.

2. *The immediate dangers* of so deep a division of the cervix are, a profuse and sometimes even a fatal, hemorrhage, pelvic cellulitis, and septic peritonitis, which is almost always fatal. It is known that these results occurred to some of the patients operated on by Dr. Simpson at his consulting rooms. There is also a risk of cutting through the cervix into the peritoneal cavity.

Such effects can, however, surprise no one who is aware of the extent of the lesion produced. Indeed, it is surprising, rather, that they are not more frequent than they are actually

Fig. 3.

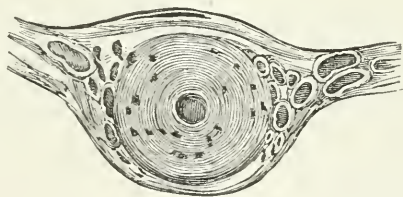
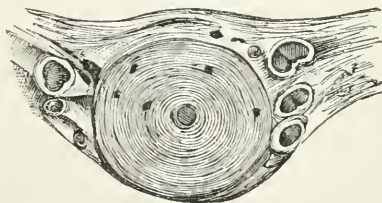


Fig. 4.



Sections of uterus made at os internum—(ad nat.).

Showing the normal size of the os internum, the circular disposition of the fibres around it, and the blood-vessels in proximity.—BARNES, p. 207.

found to be. Referring again to Fig. 2, it will be seen that the walls of the cervix are cut more than half through on both

sides to a considerable extent by the lesser incision with the two-bladed metrotome, while, by the greater, should there be a slight inclination of the uterus or of the instrument to either side, or a slight thinning of the uterine walls—an opening would be made into the peritoneal cavity. This has actually occurred. The entire cervix may also be split completely through, as in a case mentioned by Dr. Sims (p. 171).

A free hemorrhage, at least, is inevitable in such circumstances, and even the lesser incision may divide one or more arteries on the level of the internal os, as shown in Figs. 3 and 4; where it is seen that, in the second preparation, there is an artery within one-eighth of an inch, and in the other, three arteries within one line, of that opening. The lesser incision cuts to the depth of at least three-sixteenths of an inch, and the greater to more than one-quarter. If sufficient for all practical purposes, it is therefore of the greatest importance to restrict the depth of the incision of the internal os within the limits of the arterial distribution, and which I shall show may be done.

The dangers of pelvic cellulitis and septic peritonitis are referable to the fact that the medullary portion (Savage) of the cervix is laid open, and that thus perfect facility is afforded for the absorption of septic matters. This also may be avoided.

Hence Dr. Barnes¹ regards "incision of the internal os as being attended by great danger." "First, there is profuse, even 'furious' bleeding; next, from the gaping of the divided veins, and the injury of the tissues in which they run, there is liability to pelvic inflammation and septicæmia. These are no theoretical dangers. Many cases, some fatal, are well known" (p. 206). He says that "an incision even one-fourth of an inch deep will be very liable to divide some of the vessels;" and, therefore, though he still sometimes uses Simpson's metrotome, he cuts only from below the internal os to the external.

¹ Dr. Barnes' objection to Dr. Greenhalgh's instrument, because it acts automatically, is not, I think, well taken. If an instrument acts automatically, but always within certain perfectly well-known and safe limits, thus enabling the skilful surgeon to succeed, and securing the bungling operator against an accident, it is not to be rejected merely because automatic. But if an automatic instrument acts at random, or always beyond certain limits of safety, causing both the scientific and the ignorant operator equally to do harm, it is certainly very objectionable. This I hold to be the *real* objection to Greenhalgh's instrument.

I am obliged to speak thus generally of the dangers of Simpson's operation, since no statistics have ever been published, as should have been done, especially by those who have operated most frequently.

From the facts I have given, it may be inferred that no amount of experience in this operation will prove a safeguard against its dangers, and the following case illustrates this, as well as the unpardonable carelessness which great familiarity with an operation sometimes engenders :

A lady, 28 years of age, who had been married eight years without ever having been pregnant, applied to me several years ago to remove the cause of sterility. She had no dysmenorrhœa, no uterine displacement, no stenosis of the cervical canal—nor, indeed, any uterine symptom at all, excepting a slight leucorrhœa, from congestion of the endometrium. This having been cured, she soon after left, with her husband, on a short summer trip to Great Britain and the Continent, I having advised her, if any uterine symptoms returned, to get the advice, while in Edinburgh, of the then most distinguished gynaecologist there. No uterine symptoms did return; but while in that city for two or three days, she decided not to lose such an opportunity to obtain his advice respecting the sterility, and sent for him. After a rapid examination, he remarked that a very slight operation was required, and at once introduced his metrotome, and incised the cervical canal, and left the room within about three minutes afterwards. The husband left about five minutes after the physician, and did not return for an hour, when he found his wife had fainted from loss of blood, which had saturated the bed, and escaped upon the floor. Rushing to the doctor's residence, he found the latter had gone five miles out of the city, to the wedding of one of his assistants; and going next for another assistant, he also was found to have gone thither. The nearest physician was then called in, who found there was no time to be lost, and arrested the bleeding by continued pressure, until the operator could be brought back to the city. Returning, he remained with her the next twelve hours. Her life was barely saved; and, after passing the summer in that apartment, she had only recovered sufficiently to be able to travel; and the

time for the journey to the Continent being exhausted, she returned directly home. She required more than a year for the recovery of her strength and color, and now, at the end of seven years, still remains childless. The surgeon had performed the operation, probably, more times than any one else in Europe, and must have previously found it to be a treacherous procedure.

3. Having found the immediate effects of this operation thus undesirable and even dangerous, I now proceed to consider *its more remote results*, both curative and otherwise.

As a remedy for dysmenorrhœa and sterility, when depending on stenosis of the cervical canal, Simpson's operation usually succeeds, temporarily at least, with the former; and as generally fails with the latter. As the contents of a bottle without a bottom can have no difficulty in escaping from it, so the menstrual fluid should have none in leaving such a uterine cavity as that shown by Fig. 13. And if pain still attends menstruation, it is, of course, due to some other cause than stenosis. The incision, however, not seldom gradually closes up, in spite of the surgeon's intentions, and the relief proves to be temporary; and sometimes the cicatrix, continuing to contract, finally reproduces the dysmenorrhœa in a severer form than existed at first.

On the other hand, it can scarcely be expected that such an enlarged open cervical canal, as Figs. 2 and 13 represent, can exert any active influence in favor of conception, or retain the spermatic fluid, if by chance entering it. Besides, if pregnancy should actually supervene, in spite of such conditions, the ovum would probably escape from the uterine cavity prematurely. And these expectations are confirmed by observation. Conception but rarely follows the operation, as performed by Dr. Simpson; and when it does, abortion is very likely to ensue. Dr. Gream¹ reported a case of this kind; Chrobak has had several cases, and I have myself known of six. Scanzoni admits that dysmenorrhœa is frequently relieved by this operation, but objects that sterility persists notwithstanding. Hegar and Kaltenbach had fair success in the former, but their results in the latter were "less brilliant." And Barnes remarks that

¹ Dr. Sims, p. 170. From *Lancet*, April 8, 1865.

“the cure of sterility is not nearly so frequent as the cure of dysmenorrhœa (pp. 212, 213). I think the main facts on this point may be summarized as follows :

1st. If the incisions close up, there is for a time an increased chance of conception ; but the progressive induration and deformity of the external os—since nature generally effects the closure very awkwardly—finally increase rather than diminish its original improbability. If the incision remain entirely unclosed, the sterility is generally confirmed.

2d. The further the operation stops short of the deep incision of Simpson, the better the prospect of curing the sterility. Hence, in some hands, it not very seldom succeeds, though performed by Simpson's or Greenhalgh's metrotome, simply because the cervical tissue is not divided deeply, as was done by the former. A distinguished obstetrician of this city informs me that he cured his first two patients of sterility by Simpson's metrotome ; but has very seldom succeeded since, for the reason, I suppose, that he divided the cervical canal but very slightly at first, and became bolder from experience. Whatever of success Chrobak has had, in the treatment of sterility by the incision of the cervix, I attribute to his modifications of Simpson's method. He incised the internal os but three times in 250 cases. He, indeed, varied his operations, and also resorted to other treatment, to such an extent in different cases, that his statistics are of no value for my present purpose, except so far as they recognize the decided tendency to abortion, if pregnancy afterwards ensues. There cannot, I think, be a reasonable doubt that Simpson's operation, performed on a woman in perfect health, would almost certainly render her sterile, unless the incisions closed up.

3d. Sterility is more likely to be cured by the deep incision, as Dr. Barnes remarks, “the younger the patient and the more recent the stenosis,” and before its complications are developed. I will also add that more prompt and complete closure is likely to occur in such patients.

The last remote effect of this operation which I shall mention, is the eversion of the labia uteri, in case the vaginal portion has been cut through on both sides. I agree with Dr. Sims that this is a very grave objection, though his own operation

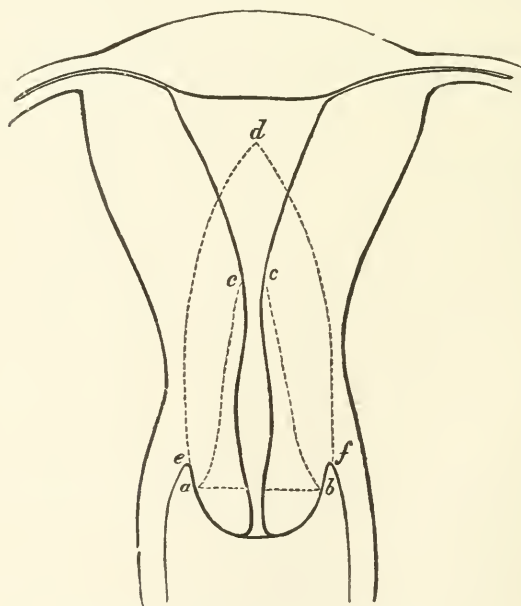
will be seen to produce the same effect far more certainly and frequently.

Finally, in specifying the *uses* of Simpson's incision of the cervix, I cannot recommend it in the treatment of stenotic dysmenorrhœa or sterility. It frequently cures the former, and the latter very seldom. But it is unnecessarily severe and dangerous; and a safer operation may and should be substituted. Of the two metrotomes, I would not recommend Greenhalgh's in any uterine condition which occurs to me; but Simpson's will be found a very useful instrument for incising uterine fibroids projecting into the cervical canal or the uterine cavity, and still covered by the uterine mucous membrane.

II. DISCISSION OF THE CERVIX UTERI.—SIMS' OPERATION.

This method of Trachelotomy was first practised by Dr. Sims in January, 1857. His operation consists:

Fig. 5.



Discission of the Cervix.—SIMS, p. 172.

1st. Of a complete severing, or discission¹ of the whole of

¹ From *discindo*, to sever, to cut apart.

the vaginal portion of the cervix, up to its vaginal attachment on both sides, by scissors ; and

2d. An incision of the whole canal above, and including the os internum on both sides, by a narrow, razor-pointed knife.

Fig. 5 shows the manner of performing the operation, and Fig. 12 the condition of the parts after it. Since the former is taken from Dr. Sims' work on "Uterine Surgery," it may be regarded as expressing his average intention in respect to the extent of the second step in the operation ; for it is impossible for any one to know precisely how much he has cut into the canal, or to certainly cut the two sides precisely alike. From the tendency to hemorrhage after this operation, I think the cuts at the internal os are usually deeper than here represented. The cervix is also only three-fourths of its normal size.

Dr. Sims claims that his operation does not divide the entire cervix to so great an extent as the metrotome of Greenhalgh. But I shall show that the division is still much in excess of what is required, while it is attended by as great a change in the uterine cavity, and the same dangers in greater degree ; and is followed by as undesirable remote results.

1. It is shown by Dr. Sims' diagram (Fig. 5) that the first step in his operation practically shortens the cervical canal to an amount precisely equal to the length of the vaginal portion, or, on an average, nearly half an inch ; for its discission on both sides practically annihilates it as completely as if it had been entirely removed by amputation. The relative length and shape of the whole uterine cavity, including the cervical canal, after this operation, as compared with its normal condition, is shown by Figs. 12 and 10. It has assumed the form of a flattened hour-glass, and the cervical canal is no longer fusiform, has lost nearly one-third of its length, and become as capacious as the cavity of the corpus.

2. The *dangers* of the operation are the same, but considerably exaggerated, as those of Simpson's operation ; viz., hemorrhage, pelvic cellulitis, and septic peritonitis.

The danger of hemorrhage from the arteries around the os internum (Figs. 3 and 4) is not materially greater or less than in Simpson's operation. A bold or a careless operator will cut as far with the knife as with the metrotome, and still more at

random and less symmetrically; while a timid cutter will probably go less deep with the knife without a less probability of dividing the arteries. But, in another respect, the danger of hemorrhage is much greater in Sims' operation, viz., from the fact that the whole vaginal portion is completely severed, on both sides, up to the vaginal attachment. An alarming bleeding is also liable to occur suddenly at any time within the four or five days after the operation, though it was not very profuse during it, and special precautions should be taken both to prevent it, and to arrest it if it takes place. It is usually arrested at the time of the operation by the application within the incision, of cotton dipped in the persulphate of iron, and of the vaginal tampon. But a surgeon who had performed this operation very many times, assured me that he never undertook it unless he could have his patient so situated that she could be visited at the shortest notice by himself, or a competent assistant, for the next four or five days. But, in spite of these precautions, fatal hemorrhage has ensued.

The risk of pelvic cellulitis and of septic peritonitis is also greater in discission of the cervix than in Simpson's operation. For, since the cut extends *through* the medullary portion of the cervix on both sides up to the vaginal junction, in addition to the incision in the canal above, a greater surface and facility is afforded for the absorption of any septic agent.

I fully agree, therefore, with Dr. Thomas, that "had all the fatal cases which have occurred in consequence of this operation been published, as they should have been, the list would be a startling one" (p. 414). He had known of five cases, and had rumors of others. I could myself add as many more.

It would seem hardly necessary to state that an operation liable to jeopardize a patient in the ways just specified should never be performed without an imperative necessity. Dr. Skinner, indeed, maintains that the vaginal portion should never be cut through.¹ This, however, like the preceding procedure, has been but too often practised apparently as a matter of fashion, or of the force of habit; for I can give no more charitable explanation of several cases which have fallen under my observation. In one instance, no reason had been assigned

¹ *Liverpool Medical and Surgical Reports*, 1865.

for the discission. It could not have been done, I think, for dysmenorrhœa, for the patient had passed the menopause at least ten years before; and the age of 55 to 60 years would also seem to exonerate her, also a maiden, from an operation for sterility. In another instance, the patient had a short time previously been under my care, and I knew she had no stenosis of the cervical canal, nor dysmenorrhœa, from any cause. And the operation was hardly demanded for sterility, since she had been married but four or five months. I ascribed it to the force of habit, or possibly to a reckless "*besoin d'opérer*" which sometimes possesses a mere surgeon.

3. The *remote effects*, curative or otherwise, of discission of the cervix are in general the same as those of Simpson's operation, already specified. That is, it generally relieves, and often cures, dysmenorrhœa, when depending on stenosis; while as generally it does not cure sterility, and does predispose to abortion, if pregnancy occurs. Indeed, in these last two respects, it is more objectionable than Simpson's method. For:

(1). The severance of the cervix on both sides not only at once destroys all contractile force in favor of conception, as completely as if it had been entirely removed; but

(2). The two pendulous flaps also act as valves to prevent the entrance of the spermatic fluid into the cervical canal. Besides, the retentive power of the proper uterine cavity is diminished by the practical ablation of the vaginal portion, and the change in its form, as just explained.

I should therefore apply to this operation also, as far as its value in the treatment of sterility is concerned, the three propositions already applied to Simpson's incision (p. 361).

Sometimes, however, the flaps become everted, and present the same appearance as if the cervix had been ruptured to the vaginal attachment in parturition; which latter condition, like the high amputation of the cervix, is well known to produce sterility in most cases, and to predispose to miscarriage, should pregnancy, notwithstanding, occur. This eversion mentioned by Dr. Sims as an objection to Simpson's operation, occurs far more frequently in his own; since, in the latter, the entire cervix is *always intentionally* severed, while in the former it is only seldom and accidentally so. All gynæcologists at the present day recognize the importance of the re-closure of the

cervix ruptured in parturition. I maintain that the same should be done here ; and the sooner after the discission the better. In other words, if a surgeon finds he has severed the cervix on both sides to the vaginal junction, in his treatment of dysmenorrhœa and sterility, he should *at once close it up again* by the proper operation. After a few months the flaps become atrophied to such a degree that it will be difficult, and perhaps impossible, to restore the external os and the cervical canal to their normal shape and dimensions.

I have already alluded to the tendency to closure of the incisions after this operation ; and which is, comparatively at least, a fortunate event, as increasing temporarily, if not permanently, the chances of conception. But nature generally accomplishes this in a very imperfect manner. The two incisions are scarcely ever completely closed ; and sometimes the attempt is made only on one side, in which case the contractility of the vaginal portion is still completely lost. If the two incisions are unequally closed, the form of the external os is, of course, abnormal ; and, equally as if no closure had occurred, is unfavorable for conception. And, finally, if both incisions are quite closed, just as if only partially so, the cicatricial tissue becomes indurated and contracted ; so that in the end, a return of the stenosis and of the dysmenorrhœa, and in an aggravated form, may be the consequence.

Such being the merits of discission in the treatment of dysmenorrhœa, I should next speak of it as a remedy for ante-flexion.

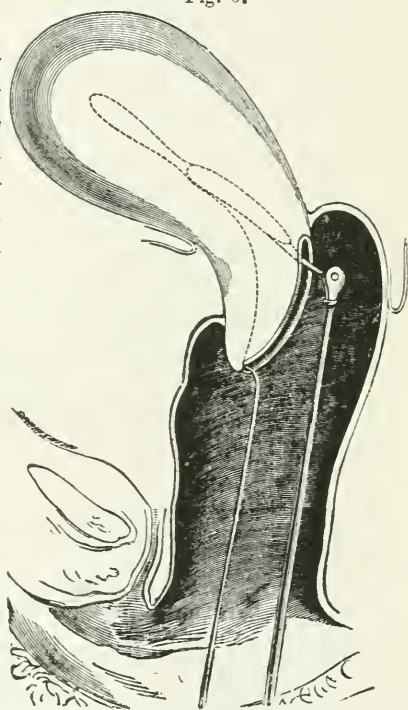
Surprising as it may appear to one familiar with the anatomy of the uterus, mere discission of the cervix to the vaginal attachment was formerly practised for its flexions.¹ I was told several years ago, by one who had operated many times for these affections, that complete discission of the vaginal portion actually did, in his experience, return the body of the uterus to its normal position !! This is precisely as logical a procedure as dividing the sphincter ani for stenosis of the sigmoid flexure ; or slitting the meatus urinarius for a stricture of the membranous portion of the urethra. On expressing my scepticism,

¹ I am, of course, not here speaking of flexions of the cervix alone, but of ante-flexion and retroflexion of the uterus.

and enquiring how a mere cut could affect the reposition, when it did not reach to within at least three-quarters of an inch of the point of flexion, he replied that he could not explain, but was sure of the fact. Of course the operation merely relieved temporarily the congestion of the displaced uterus, and removed some of the symptoms, by the free hemorrhage it occasioned;¹ and which half a dozen leeches would have done far more safely. I allude to this application of discission of the vaginal portion, merely as a matter of its history.

Fig. 6.

Next, we find, the following advance in the treatment of ante flexion was proposed: First, the vaginal portion was divided, posteriorly, up to the vaginal attachment, by scissors; and then a narrow, razor-shaped knife was passed up the cervical canal, and through the internal os; and an incision made anteriorly above and posteriorly below,—so as “to bring the incision of the cervical canal into a line with the uterine cavity.” From its object in this latter respect. it is sometimes termed the “Sagittal incision.” Fig. 6 shows the manner of making the incision, and the straightening alleged to be effected thereby; it being the now stereotyped diagram used by most writers on this subject, since first published by Dr. Sims in 1866.



Manner of operating in alleged ante flexion.—SIMS, p. 169.

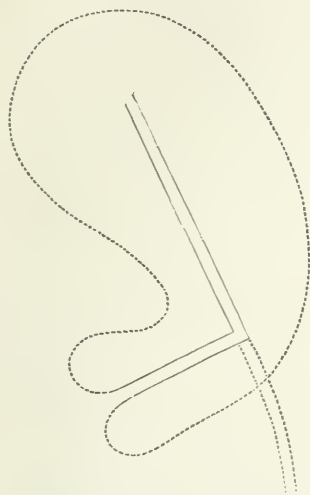
¹ Dr. Barnes, indeed, speaks of the bilateral division of the cervix as a very effectual method of local depletion in the treatment of anteversion (p. 583). But the hemorrhage is not the effect aimed at by him in this condition; but an accident that may do good in that way also.

Even if this operation could accomplish what it promises in anteflexion, there would be an insuperable *à priori* objection to it, since it ignores and contravenes the first indication in all flexions of the uterus, viz. : to return the corpus uteri first of all—unless inflammation, preventing the necessary manipulations, co-exists—to its normal position; for this procedure not only leaves the corpus in its abnormal position, but aims to keep it there permanently, by producing in the cervical canal another abnormal condition still more serious (and it is often incurable,) than the original displacement. Hence Dr. Thomas proposes it only in case the anteflexion is irreducible. It will, however, be seen that this operation does not at all effect what it proposes in anteflexion; and simply because in that condition of the uterus it is impossible.

Since the axis of the uterine cavity and the cervical canal meet at the internal os at an angle of 165 degrees, it will be seen by referring to any diagram representing an antero-posterior section on the median line, of the normal uterus, that an incision made into the cervical canal in a straight line, in continuity with the uterine cavity, would fall barely below the vaginal attachment, and within the vagina, just as Dr. Sims represents it; or, in other words, that Dr. Sims' sagittal incision can really be made only when the corpus uteri itself still remains in its normal position. If the corpus be bent forward at a right angle, the cervix would have to be split through posteriorly to the internal os, and into the peritoneal cavity, to carry out the idea; and, in the extreme of anteflexion, the incision must aim toward a point somewhere in or above the lumbar region. Indeed, the total inapplicability of the sagittal incision to anteflexion is admitted by Dr. Sims himself, when rightly interpreted. For, after describing his case as "curvature with elongation of the vaginal portion accompanying anteflexion" (p. 166), and showing how the "canal of the cervix is made to run in a straight line from the cavity of the uterus to the terminus of the incision," by two diagrams (pp. 167 and 169), he adds, "the operation is wholly inapplicable, except in just such cases as the one above described" (p. 168). But neither of these figures represents anteflexion at all; but simply curvature of the cervix alone, without any abnormal position of the corpus above it. The same remark is also

applicable to the diagram, (Fig. 7), intended to represent the

Fig. 7.



Sagittal incision in alleged ante flexion.—DR. THOMAS, p. 412.

sagittal incision in ante flexion, in Dr. Thomas's valuable work (p. 412). The preceding figure (6), copied by most writers on ante flexion since 1866, has misled hundreds of operators, who have never challenged its accuracy, in actual cases of ante flexion; and it is time it is generally understood to be imaginary and fallacious.

The application of the sagittal incision (posterior discission) in flexion and curvature of the cervix alone, in both of which it is very often, but by no means always, required, is well shown by Fig. 8, which is also copied from Dr. Sims' work (p. 167.)

In specifying the *uses* of Sims' operation, I should, on the before-mentioned grounds, reject it in the treatment of sterility,¹

and also object to it in cases of dysmenorrhœa, as unnecessarily severe and dangerous, in comparison with another operation which I shall advocate; and I know of no other condition in which it should be recommended. But bilateral discission alone, up to the vaginal attachment, is justifiable in some cases already described on a preceding page, of uterine fibroids, as a substitute for incision with Simpson's metrotome, to aid their descent into the vagina.

But, in such cases, the incisions should be closed with sutures as soon as the fibroid is removed. In these cases, also, an incision on one side only is

Fig. 8.



Sagittal incision in flexion of the cervix.—SIMS, p. 167.

¹ See the results of Dr. G. Braun, of Vienna, (*Wien. Med. Wochenschrift*, 1869; and of Chrobak, *Weibliche Sterilität*, Wien, 1876.

frequently sufficient. Bilateral discission, to a limited extent, ($\frac{1}{4}$ inch, or even less), as practised by Dr. Barnes, is also applicable to some very rare cases of conical cervix. Finally, it has been shown that complete posterior discission is required in many cases of flexion and curvature of the cervix alone.

Having thus stated my own convictions respecting the real value of Simpson's and of Sims' operations, and incidentally also of mere bilateral and posterior discission, I close this part of my paper with some quotations from the latest writers on this subject; stating especially the points upon which they are not in accord with myself. Schröder,¹ who practices Sims' operation, says inflammation of the uterus and cellular tissue does not occur if the operation is performed with clean instruments; the hemorrhage may, however, be considerable, and occasionally serious." (p. 71.) Dr. Thomas thinks "Sims' operation, *per se*, is not attended by great danger. It is the performance of it when pelvic peritonitis exists in chronic form, that has caused it to produce such bad results." (p. 414.)

Hegar and Kaltenbach regard the sagittal incision as more dangerous than bilateral discission, the latter being less dangerous than heretofore supposed. But they really follow neither Simpson nor Sims, never using scissors, except as preliminary to amputation of the cervix; but merely a knife, cutting the internal os two to three millimètres ($\frac{1}{12}$ th to $\frac{1}{8}$ th of an inch), and the external os $1\frac{1}{2}$ centimètres ($\frac{3}{8}$ th inch). Thus, in fact, they do not perform discission at all; and may well find their own procedure not so dangerous as actual discission "has heretofore been supposed to be." Still they lost two patients in 150 cases. Their results were good in dysmenorrhœa, but less brilliant in sterility.

Dr. Hewitt prefers Sims' operation; but with the important modification of cutting but very little of the internal os. He thinks that "the incision treatment will be restricted within narrower limits than was the fashion three or four years ago." (p. 399).

Dr. Barnes' testimony is seemingly somewhat conflicting in different portions of his work; but he is sometimes speaking of Sims' operation, and sometimes of incision of the cervix as

¹ Diseases of the Female Sexual Organs.

practised by himself. He says: "Sims' is an effectual operation, but unnecessarily severe." He uses scissors, or Simpson's metrotome, but never cuts the internal os at all. He thinks it generally quite proper to divide all the vaginal portion of the cervix (p. 210), the risk of the operation itself being infinitely small, if proper precautions be taken. From the want of them, bleeding and peritonitis are not uncommon results. He has seen several cases of chronic pelvic cellulitis arising in this manner; and some fatal cases of bleeding are known to have occurred; (p. 211). Further on, however, he accounts for the safety of the operation performed by himself, when he says: "It is enough to make a good transverse slit, or os tinæ, which shall give free communication between the cavity of the cervix and the vagina. The part that is divided is not very vascular; and it is rare that any bleeding of importance occurs (p. 216). He, however, keeps his patient four days in bed, and in her room for a week. He advises bilateral incision, (but not discission), in some cases of primary retroflexion; but merely to remove the stenosis of the os externum, which usually attends it. (p. 612). Finally, he indorses Sims' operation in ante flexion. (p. 594).

From the preceding facts, I deduce the following conclusions:

I. The deep incision of the cervix throughout, and complete bilateral discission of the vaginal portion with deep incision above, are alike frequently attended by certain immediate dangers, and, not seldom, productive of certain serious remote consequences; viz., profuse and sometimes fatal hemorrhage, pelvic cellulitis, septic peritonitis, (usually fatal), sterility, (if not previously existing), and a tendency to miscarriage.

II. Those risks and effects are all due to the extensive division of the walls of the cervix, and to the consequent enlargement of the cervical canal; and the sole compensation for all of them which can be calculated upon, is the relief, and very often the cure, of stenotic dysmenorrhœa.

It therefore becomes a question of very great practical importance whether the amount of cutting may not be so far diminished as to avoid all these risks, and at the same time be sufficient for the cure of stenotic sterility and dysmenorrhœa. But another inquiry, antecedent to this, is: How large

a calibre of the cervical canal is actually required for the relief of these two conditions?—and a reply sufficiently definite for all practical purposes is not so difficult as might appear.

In the *imparous* woman, the narrowest point of the cervical canal, viz. the internal os, is, when opened by the passage of the menstrual fluid, an ellipse, whose conjugate and transverse diameters average respectively $\frac{1}{6}$ and $\frac{1}{8}$ of an inch; its area corresponding very nearly with that of a circle $\frac{1}{4}$ in. in diameter.¹ The external os, also elliptical when moderately dilated, has diameters averaging $\frac{1}{4}$ and $\frac{1}{6}$ of an inch. It thus has an area exactly twice that of the internal os, and equalling that of a circle $\frac{1}{8}$ inch in diameter.² The larger size of the external os doubtless has a special reference to conception, and favors the entrance of the spermatic fluid into the cervical canal. It has no special influence against dysmenorrhœa; since the menstrual fluid, after having passed through the internal os into the cervical canal, would pass just as easily from the latter through an opening of the same dimensions into the vagina. Hence we not very seldom see imparous women with the external os no larger than a “pin-hole,” and who nevertheless do not suffer from dysmenorrhœa, though, as a rule, they are sterile. But if the lining membrane of the canal becomes thicker, from congestion or some other cause, such patients suffer at once from stenosis at the external os.

In the *parous* woman, the size of the external os varies within quite extensive limits, since it is exposed to so many of the accidents of parturition; while the internal os is more nearly uniform.

I have deemed it desirable to ascertain the lowest average diameter of the two ora uteri in parous women, who are neither sterile nor have dysmenorrhœa, as a rational standard for determining the extent of incision actually required for the removal of these two conditions, when stenotic. And after a good deal of observation in this direction, I find that the inner os presents nearly twice the area of that of the imparous woman; in the majority of cases admitting a sound one-fifth of an inch in diameter—though, in a large minority, one from one-fifth to

¹ The circle is smaller than the ellipse, in the proportion of 144 to 147.

² Circle to ellipse as 72 to 75.

one-sixth of an inch only can be easily passed. I therefore regard a diameter of one-fifth of an inch as ample for the removal of stenotic sterility and dysmenorrhœa. I find the external os admits a dilator one-fifth of an inch in diameter and upwards—in some cases as high as one-fourth, or even three-tenths, of an inch—but, as a rule, I think one-fourth of an inch sufficient for the purpose. It is, of course, to be understood that no narrowing of the canal exists between the two ora. Since, however, there may be some degree of stenosis for the menstrual fluid, while not for the sound, it is sometimes judicious (and especially if congestion of the cervical lining membrane co-exists) to increase the dimensions just named, by the use of a dilator of the next larger size. I do not assert that the preceding dimensions are always required in the treatment of stenotic sterility and dysmenorrhœa, for they are not; nor that they are never to be exceeded; but that in almost all cases they will be found sufficient.

Should this precise specification of dimensions seem too minute for practical purposes, we must remember that dimension cannot here have a less important relation to function than elsewhere; and that enlarging the internal os to the diameter of half an inch, as is often done by the deep incision, is, as has been seen, like permanently dilating the urethra (if it could be done) to the size of the small intestine. And the importance of making an incision of the internal os, with a precise intention, and a precise knowledge of the mode of accomplishing what is intended, may be understood, when I state that if the circle representing its area in the imparous woman be increased equivalently to surrounding it by a ring only one-thirty-fifth of an inch wide, its area is increased as forty-nine to twenty-five, or almost exactly doubled. Or if an incision be made on each side of it to the extent of half of a line (one-twenty-fourth of an inch), and it then be dilated to a circle, it is increased two and a half times. And if the cut should extend one line to the right and the left, or the added ring were one-twelfth of an inch wide, the area would be increased more than four times and a half. This last increase is far more, in my experience, than is ever required in stenotic sterility and dysmenorrhœa.

III. SUPERFICIAL TRACHELOTOMY.—MY OWN OPERATION.

Desiring to restrict the operation of trachelotomy in the treatment of stenotic sterility and dysmenorrhœa within the limits actually required, I, some ten years ago, devised, and brought before the New York Obstetrical Society,¹ a series of five steel cervical dilators, to be used instead of incision, where the stenosis is slight and the cervix is normally soft and pliable. These, in shape and size, have a precise reference to the dimensions of the cervical canal, and especially of the two ora uteri, as already specified; and each is guarded by a bulb, so as to project through the internal os into the uterine cavity only about one quarter of an inch.

But finding that almost all cases of stenosis of the cervical canal are relieved more promptly, more permanently, and also with less pain, by incision, or this together with dilatation, than by any form of dilatation alone, I next endeavored to restrict the extent of the incision within the absolutely necessary limits, having determined them approximately by the preceding facts and calculations. To this end I devised a new method, and an instrument for executing it, which I also laid before the New York Obstetrical Society about eight years since; but the former was so simple, bloodless, and unpretending, in comparison with the procedures of Simpson and Sims, that it excited but little interest. Meantime, however, it has been sufficiently tested, I think, by myself and my pupils in different parts of the country, to entitle it to a more general notice.

Since the superficial incision, as suggested by myself, has for its direct object merely the removal of stenosis of the cervical canal, and is therefore proposed for the treatment of stenotic dysmenorrhœa and sterility only, it is previously to be decided whether stenosis actually exists. And the following propositions will aid in settling this question, it being understood that the exploration is to be made at least four days after, and at least three days before, the catamenial flow.

A. *Respecting Stenosis of the Internal Os.*

1. If a sound one-fifth of an inch in diameter passes easily

¹ Also described in the *N. Y. Medical Journal*, July, 1870, p. 478.

through the cervical canal, there is no stenosis at the internal os, and no incision is there required. This is the size, therefore, of my large sound.

2. If a sound one-sixth of an inch in diameter be easily passed, as above, there is no absolute, though there may be relative, stenosis of the internal os; *i.e.*, there may be stenosis for the passage of a fluid, though not of the sound; and an incision to one fifth of an inch may be required, but not unless the symptoms indicate it.

3. If the sound easily passed be but one-seventh of an inch in diameter, and there are no symptoms of stenosis, no incision of the internal os is required. This is the normal size in the imparous woman, and the average size of Simpson's sound.

4. If a sound but one-eighth of an inch in diameter cannot be passed through the internal os, there is either stenosis, or, what is very much more probable, one of the flexions. Prove, therefore, that there is no flexion in this and every case in which a sound of any size does not traverse the internal os, before operating for stenosis. I consider an internal os of one-eighth of an inch, or less, to be stenotic. Chrobak's highest limit for stenosis of the internal os is one-tenth of an inch (two and a half millimètres).

B. *Respecting Stenosis of the External Os.*

5. On the other hand, there is no stenosis of the *external os*, if a sound one-fifth of an inch in diameter easily traverses it. If there be congestion of the lining membrane, however, there may be stenosis, practically, in respect to conception; and the operation somewhat enlarging it (to one-fourth of an inch or more) may be required.

6. If the external os will not easily admit a sound one-sixth of an inch in diameter, there is probably stenosis in respect to conception, and the operation is required. If not more than one-seventh of an inch, the operation will also probably be required for dysmenorrhœa.

7. In case of operation, the whole cervical canal must be made still to retain the normal fusiform shape as far as possible.

I. My *method* consists in incising the internal os, if the stenosis exist at that part—and the external, if at the latter—to

such an extent as to give to both their precise average dimensions in the parous woman—neither more nor less—and, of course, also overcoming any other point of stenosis existing anywhere else in the cervical canal. In cases complicated with congestion, however, I have shown that a slightly larger opening may be required; and, therefore, that the limits may extend beyond one-fifth of an inch (to nearly one-fourth of an inch) in the case of the internal os, and to three-tenths of an inch, and possibly more, of the external.

I do not, therefore, incise the internal or the external os to a given depth in all cases; but, taking them as I find them, cut just enough to give them their average normal size in the parous uterus. This is seldom one-half a line and often not more than one-third of a line for the internal os, or more than a line for the external. But, of course, there is far more variation in the latter. If the internal os admits a sound of but one-eighth of an inch in diameter, a cut on each side of nearly half a line (but three-eightieths of an inch) is required; and if but one-tenth of an inch in diameter, it must be one-twentieth of an inch deep on each side. The incisions are of precisely the same depth on the two sides.

Since the lining membrane at the internal os is at most one-twenty-fifth of an inch thick, it is seen that I generally do not cut nearly through it. Indeed, when the os is but one-eighth of an inch wide, I cut almost through the membrane; and when one-tenth of an inch, I divide it, and one-hundredth of an inch into the tissue beneath it.¹

II. The *instrument* devised to secure this effect consists of a flattened tube, containing a blade. The former is eight inches

Fig. 9.



Dr. Peaslee's Metrotome. One-half size.

long, and seven-sixteenths of an inch wide, except its terminal one and three-fourths of an inch, which has a width of but one-

¹ The details of all the preceding calculations are properly omitted here; as a slight acquaintance with mathematics will enable the reader to verify them.

eighth of an inch, as shown in Fig. 9. This portion is made curved by some instrument makers, which is not an improvement. The blade is of such a width as to slide accurately within the tube, having a nut and a screw attached to its proximate extremity to gauge the extent of its passage into the cervical canal, and a blunt point, and lateral cutting edges for one and five-eighth inches at the distal end. There are two blades for each instrument, the cutting portion of one being one-fourth of an inch wide, and of the other three-sixteenths of an inch. If the stenosis is confined to the internal os, the narrower blade alone is used. If both ora are contracted, the wider instrument is passed through the external, and the other blade then introduced, and the inner os incised by it; and, in cases of decided congestion, the wider blade alone is sometimes used for both ora. In this case, a sound one-fifth of an inch in diameter is easily passed through the inner os; while, if the smaller blade had been used, a considerable force would be required to carry it through.

In hospital practice, I place the patient upon the side, use the duck-bill speculum, hold the cervix by means of a uterine tenaculum, pass the tube into the canal up to the shoulder, and, therefore, one-quarter of an inch into the uterine cavity through the internal os; when the blade, previously gauged, is introduced into the tube, and carried up the cervical canal, as far as is required to overcome the stenosis. My large sound (No. 10 American scale), or, still better, the conical dilator of the proper size, is then passed up the canal, and the operation is completed. In private practice, I generally place the patient on the back, and pass the tube into the cervical canal precisely as I would Simpson's sound; and then pass the blade through it, as just described.

If the external os is too narrow for the admission of the extremity of my instrument, it may be enlarged by the introduction—generally one-eighth to one-quarter inch is far enough—of a narrow-pointed bistoury. I have not found the internal os too narrow to receive it, except in cases of flexion, or of previous traumatic injury of the cervix.

The changes in the whole uterine cavity from this operation, are shown by Fig. 11. Respecting its dangers I have but little to communicate. The hemorrhage following it seldom exceeds

one or two drachms, and never requires any special attention. The pain is very slight and merely momentary, and no anæsthetic is ever required. The medullary structure of the cervix never being cut into, pelvic cellulitis and peritonitis do not ensue. The only exceptions to this statement, in nearly 300 cases, are one case in private practice, in which some febrile reaction and uterine tenderness ensued, which subsided entirely, without cellulitis, in four days; and two cases in the Woman's Hospital of slight cellulitis. But both the latter were patients who were known to have had cellulitis a short time previously; and I was obliged by some peculiar circumstances to operate sooner than I otherwise would have done. The final results were precisely as desired in each of these three cases. Otherwise I have never had any unpleasant symptoms follow the operation; and the only precautions taken are to keep the patient two days, and sometimes three days, in bed; and not allow her to walk out under a week. I use the dilator every second day after the operation for a week, and two or three times more once a week. I have very often performed the operation at my office on residents of the city, and sent the patients home to bed after half an hour's rest, and have never had to regret it. I decline to operate within four days after or six days before the catamenial period.

I claim for the method just described the following recommendations in the treatment of stenotic sterility and dysmenorrhœa:

1. It aims to restore the normal dimensions, as existing in the parous woman, throughout the cervical canal—nothing more and nothing less—unless when a slight exaggeration of size is required on account of co-existing congestion.

2. It effects this object definitely and with certainty, and with incisions exactly symmetrical, or equal on the two sides.

3. It gives no danger from hemorrhage, since the arteries nearest the internal os, if that is to be divided, are never reached, and the whole thickness of the lining membrane even is generally not divided; and there are no arteries within the portion divided at the external os.

4. There is no danger of pelvic cellulitis, except in those patients in whom the least operative interference with the cervix, or the use of the sound, or of a sponge-tent, will produce it.

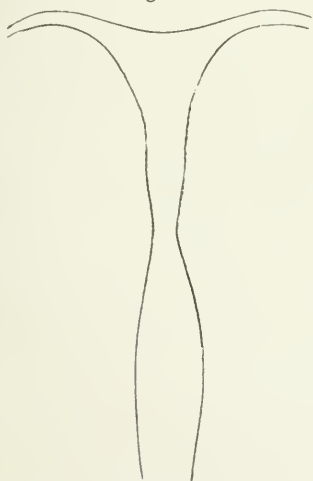
I consider the operation less dangerous in this respect than the last-mentioned.

5. There is no danger of septic peritonitis, since the medullary substance is not reached by the incision.

6. It does not produce sterility, or tendency to abortion, by mutilating the cervical canal. The changes it produces in the latter, as compared with those from the operations of Simpson and Sims, are shown by Figs. 10, 11, 12, and 13.

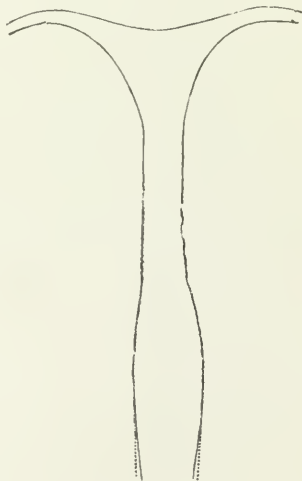
7. It removes stenosis perfectly, and in most cases permanently, since there is very little tendency to closure of the slight incision made. I have had to repeat the operation only twice in my practice, except in cases in which there is cicatricial tissue to be divided; as after imperfect and partial closure, following rupture of the cervix in parturition, or ensuing after Simpson's or Sims' operation. Here the operation will usually have to be repeated in a year or two, unless pregnancy

Fig. 10.



Normal uterine cavity.

Fig. 11.



Do., modified by Peaslee's method.

should occur; an event not to be expected in such cases, as we have seen.

Finally then, since my experience has shown that a diameter of one-fifth of an inch for the internal os, and one-quarter to

three-tenths of an inch for the external os is sufficient in the treatment of stenotic sterility and dysmenorrhœa, I suggest the disuse of Simpson's and Sims' operations in the treatment of

Fig. 12.

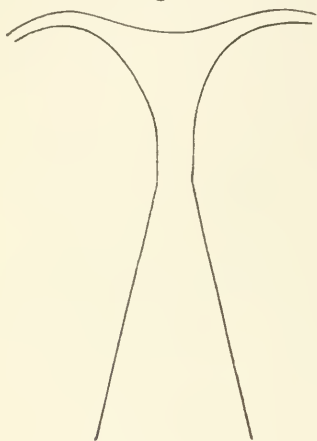
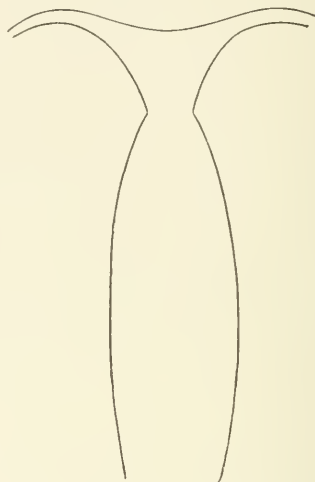


Fig. 13.



Uterine cavity after Sims' operation. Do., after Simpson's operation.

these conditions; and the substitution of a milder, safer, and more efficacious method—of which, perhaps, my own is, however, merely the forerunner. At least, further experience in the line I have indicated will doubtless afford still more accurate conclusions.

WHEN AND WHY WERE MALE PHYSICIANS EMPLOYED AS
ACCOUCHEURS?

BY

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THERE can be no doubt that, until within comparatively recent times, the general practice of midwifery lay in the hands of midwives. From the works of Hippocrates, Galen, Celsus, and of their disciples, it is evident that male physicians were called in only when special difficulties arose. These writers, were, therefore, ignorant of the more natural processes of labor, and their works treat of dystocia alone. Moschion,² of the second century, was in fact the first author, and for many centuries the only one, who describes a natural labor. He is consequently the first one who writes like an eye witness upon lacerations of the perineum, and the first one who in difficult cephalic presentations resorted to podalic version. "Do not refuse," says Hippocrates, "to believe women on matters concerning parturition."³ "It is needless," writes Aëtius, "to give a treatise on midwifery, because from long experience, not only do midwives, but also all other women, know this subject perfectly."⁴ "I am informed by midwives," explains an unknown writer of the thirteenth century, "that when the head presents, all goes well; but when an arm or a foot, then danger arises."⁵ How can we interpret the inconsistency of Hippocrates, who compares the foetus in the womb to an olive in a bottle, which can only be withdrawn by the one or the other pole, and yet asserts that a pelvic presentation is generally fatal to both mother and

¹ Read before the Obstetrical Society of Philadelphia, May 4th, 1876.

² Περὶ τῶν Γυναικείων παθῶν, in I. Spachii Gynæciorum Harmoniâ Argentine, 1597.

³ Œuvres Complètes d'Hippocrate, par Littré, Tom. vii, p. 441.

⁴ Tetrabibli iv, sermo iv, cap. 14.

⁵ De Secretis Mulierum, Argentorati, 1537.

child,¹ unless the only breech cases he ever saw were those in which the head was arrested by a narrow brim, and he was called in by the midwives to extract it? How otherwise can we account for the perpetuation of this error, in spite of Moschion's teachings, until the seventh century, and for its ultimate refutation by the second practical accoucheur of antiquity—Paul of Ægina, surnamed Obstetricus?

Still, although both Moschion and Paulus Ægineta were much sought after by the women of their day, they were but isolated examples; and midwives, as in the time of the Pharaohs, continued until a much later period to monopolize this branch of medicine. These facts bring up two very interesting questions: When were male physicians first employed by women to attend them in ordinary labors? What were the causes of such a departure from a custom hoary with antiquity?

This innovation Astruc² dates from the night of December 27th, 1663, when, from motives of secrecy, Julien Clement was summoned to deliver the frail and beautiful Duchesse de la Valliere. Le Grand Monarque, having never read the history of Portia, the worthy daughter of Cato, nor that of other reticent ladies of antiquity, had the ungallant idea that a woman cannot hold her tongue. So Clement was mysteriously conducted to a certain house where a veiled lady lay in the throes of labor. She was delivered of a boy, Louis de Bourbon, and it is said that the king watched the proceedings from behind the tapestry. Clement afterwards openly attended this lady in her other labors, and this circumstance, it is alleged, set the fashion of employing a male physician, first to the princesses and to the *dames du grand monde*, and afterwards to the *bourgeoisie*. Thus does Astruc account for the origin of "male midwives," as they were contemptuously termed in England, and of "accoucheurs," as they were for the first time then called in France. "I am assured," he adds, "that the period of employing men does not date earlier than this."

Julien Clement afterwards delivered Madame de Montespan, and secrecy was again deemed so important—for Louis XIV. threw a halo of sentimental mystery around his amours—that the

¹ Op. cit. Tom. viii. p. 79.

² Histoire Sommaire de l'Art d'Accouchements, Paris, 1776, p. 38.

confiding doctor was conducted blindfolded to her bedside. So ignorant was he of the quality of his patient, that he bade the proud king, who stood by disguised, hand him a glass of water. This accoucheur had so wide-spread a reputation, that Philip V. repeatedly summoned him to Madrid, to attend the labors of his wife, Louise-Gabrielle de Savoie.¹

That this much quoted assertion of Astruc's, made just one hundred years ago, is in every respect incorrect, I shall now try to show. In the first place, it was evidently not through a fashion set by royalty that accoucheurs were first employed; for Maria Theresa herself, the wife of Louis XIV., following the custom of Austrian ladies, employed a midwife in all her labors, although she always kept Francois Bouchet on hand in an antechamber, against any emergency. Nor in the second place, is Astruc more correct in regard to the time when this innovation took place. From a very interesting little book, first published in Paris early in 1609, by Louise Bourgeois,² I gather that, for many years before this date, the services of male physicians were being preferred to those of midwives for ordinary cases of labor. It also appears that so steadily did this innovation grow into favor, that, by the year 1600, at the time of Marie de Medici, queen of Henri IV., accoucheurs were in such repute, as to make her midwife, the aforesaid Louyse, as she spells her own name, very jealous of them.

There was, as I have elsewhere shown,³ a certain M. Honoré, who—beshrew him—was a great favorite with all the ladies of quality who were breeding in those days. To him, whenever the occasion offered, this midwife behaved most spitefully. In one place she sneeringly refers to him as “that man of Paris who delivers women.” In another, she writes, “I performed this operation (version) in the presence of Messieurs Hautin, Duret and Seguin, and of that surgeon who the most frequently delivers women. He wished to help me, but I refused, knowing that I was able to do it without risk to the

¹ *Essais Historiques*, par Sue, vol. i, p. 118.

² *Observations Diverses sur la sterilité, perte de fruit, fécondité, accouchements, etc.*, par Louyse Bourgeois, dite Boursier, Sage Femme de la Roine. A Paris, 1617.

³ *A Sketch of the Life and of the Writings of Louyse Bourgeois, midwife to Marie de Medici*. By William Goodell, M.D. Philadelphia, 1876.

lady." Once she took good care to keep him twiddling his thumbs in the royal closet, while she was delivering Marie de Medici. The breech presented, and the king, Henri IV., sent for M. Honoré; but Louyse put elbow-grease on the legs of the young prince, and saved both his life and her honor. In narrating a case of tedious labor, she piously crosses herself, and returns thanks to God for permitting her to receive the child before the arrival of this horrid M. Honoré, who had been sent for.

Then there was a Maistre Charles Guillemeau, the favorite pupil of Ambrose Paré, and one of the Chirurgeons to the king, who, in 1609, published an excellent work on Obstetrics.¹ He, also, was in such demand by the ladies of his day as to earn the undisguised hatred of our jealous midwife. She takes pains to belittle him, and to embalm one of his blunders in the amber of her sarcasm. So common, indeed, had the custom of employing male physicians become by this time, that Guillemeau himself took alarm. Devotedly attached to the traditions of his Church, and accepting in full her tenets on this subject, he trembled lest this innovation should undermine the chastity and the decorum of his fair countrywomen. On this score, therefore, he urged that the number of midwives should be multiplied, and that they should be made to qualify themselves for their calling by a special training.

But the chief evidence that Astruc is entirely wrong, can be gleaned from the advice of Louise Bourgeois to that one of her daughters who followed her calling. It forms the closing chapter of that edition of her book published in 1617, and contains fifty-five pages. Of this advice the greater part is taken up by her in bemoaning the wantonness and immodesty of the ladies of her day, who employ accoucheurs in preference to midwives. In it she relates at length the history of a midwife nigh sixty years old, of the Faubourg St. Germain, who got poxed in the hand by delivering a "whitened sepulchre" (*sepulchre reblanchy*) of a courtesan, and thereby infected thirty-five different households. "The husbands took the disease from their wives, the children from their mothers." The mutual recriminations of husbands and of wives set the whole faubourg by the ears, and

¹ De La Grossesse, Paris, 1609.

the scandal was terrible, until the bandaged hand of the midwife revealed the innocent cause of the scourge. Friends besought her to consult a physician; but the modest midwife, who by this time had a bubo, would not submit to a treatment which involved the exposure of her person. Her daughters went down on their knees before her, but the noble matron preferred to wrap the drapery of death around her than to unwrap her own. These good girls, therefore, hunted up "an old surgeon who lived in the street of the dove-cote of the Abbey St. Germain," stated the case to him, and begged him to marry their mother. The surgeon was a "*fort honneste homme*," and, withal, a bachelor, and he did not hesitate. Yes; this brave man sacrificed himself on the altar of duty; he espoused the modest midwife of nigh sixty, he dressed her sores, and—he cured her. "I knew them both," adds Madame Bourgeois-dite-Boursier, "but the *greater part* of women, now-a-days, do not put their friends to so much trouble before allowing themselves to be handled by men, and for far less need. M. Honoré knows well to what I am referring, for a vast number (*une infinité*) of coquettees declare that, even in ordinary labors, they prefer him to a woman. This is at present the fashion (*cela est à présent de la mode*). Let me tell you, my daughter, what I have seen in my younger days. Twenty-five years ago [viz., about 1590,] the great majority of women were of different humor. There were always, it is true, men-midwives (*mal-sages*) but they were then not so common as they are now. I have reflected much over the source of this license and attribute it in a great measure to two causes."

This worthy midwife now devotes so many pages to the consideration of this heartfelt grievance that I shall merely give the gist of her arguments, following as closely as possible the original text. "One cause is, that in times past, when a young girl was married, her husband put her under the authority of her mother, her mother-in-law, or of some aunt whom she feared. In default of such a person, her relatives selected some God-fearing matron, to whom they gave her in charge, and whom they commanded her to obey. When the husband saw his wife downcast, he forbore to notice it, well judging that she had committed some fault of youth, for which she had got a scolding; nor did she dare to complain." Now-a-days, young wives maintain separate establishments; and, instead of such pious duennas,

"who kept them in the fear of God and at their embroidery," they keep about them as companions, giddy and wanton girls, "the refuse of the provinces. In verity, these wolves in sheep-cotes ruin a vast number of our young women, even of good family, by wheedling them into intrigues, and enticing them into every kind of extravagance. . . . Children formerly remained children a long time, but now they are very knowing, and resemble those trees which flower betimes, but which the slightest frost blights. All this evil [viz., the employment of male physicians] springs from the license of young women. They roam about as free as the does of the forest, and are like young colts which sadly need a bridle. You could not think otherwise, were you to see the husbands of many of them, so overburthened by their extravagance and by their bad house-keeping, as to become withered, thin, and as yellow as wax. . . . Our young women think themselves wiser than ever their mothers were, and in very truth they certainly have greater boldness than the women of bygone times. They are always dressed for paying or for receiving visits, where there is no lack of tittle-tattle. When their conversation flags, for it is as incoherent as the dung of a goat, they set upon any chance visitor who may be a breeding, and entertain her with all the possible dangers of travail, and even invent those which have never happened." And this, of course, frightens her into the employment of a male physician.

She then goes on to say that she knows this from sad experience. In one instance the poor young lady had been so wrought up by these idle tales, that when the midwife called for thread and a pair of scissors to cut and tie the cord, she, supposing it was to cut her open and sew her up again, went off into fits, which never ceased until she died, "which shows", says our excellent authoress, "that a midwife should never be without her own thread and scissors."

"There are at present," she continues, "very few women who so *affection* their midwives as they did of yore, when, upon the death of their midwives, they wore deep mourning, and prayed to God not to give them any more children,—which was not right, but their affection carried them that far. Many women still employ them, but simply as female vintagers who are changed at every vintage, and are paid by the day.

A sance needs much piquancy to make it taste pleasant to a sick person without appetite, as our young women do, who from their first labors make choice of a man to deliver them. This makes me blush for them. For to resort to this without need is a great piece of shamelessness, (*une effronterie trop grande*), such as, I am sure, their mothers and grandmothers would never have exhibited. Difficult cases of labor will, it is true, happen, in which, as I have enjoined and still enjoin, a surgeon should be called in. But his presence is enough to make the woman blush up to her ears, and the husband greatly vexed, were the need not urgent, or the affair whispered to others. It should, therefore, be so arranged, that neither the woman nor her husband should know of his coming. Neither should the woman see the surgeon, nor he her face." She then proceeds to relate that, in a tedious labor, being importuned by the lady's friends, she took advantage of the absence of the husband, and sent for a surgeon. But, knowing that her "patient would die from very shame and fright at the sight of him," she so disposed the pillows, bolsters and coverlids as to obstruct her view. The surgeon then crept up noiselessly to the foot of the bed, and made the needful examination without the knowledge of the "*honneste Damoiselle*," who after all "was delivered by no other help or artifice, than that of God and of nature. . . . Since this indecency has become the fashion, dangers greater than those of former times present themselves, which would be better met by skilled persons, [viz. by midwives,] were they only let alone."

These extracts conclusively prove that accoucheurs were employed long before the year 1600, and that Astruc is therefore historically incorrect. It remains, therefore, for me to consider the causes that brought about this very remarkable change in public opinion — an opinion coeval, as far as history records, with the pyramids of Cheops. It was not royalty, as Astruc contends, that set the fashion, because both Marie de Medici and Maria Theresa, the queens respectively of Henri IV. and Louis XIV., were delivered by midwives. Nor was it the wantonness and the immodesty of the women, as the blushing Louyse Bourgeois complains; for, as the current literature abundantly attests, the further one goes back in French history, the greater does one find the immorality to be. But it was, as I shall try

to show, the art of printing that gave the death-blow to the monopoly of midwifery by midwives.

Printing was discovered about the year 1453, but it did not reach France until 1470. Among the very first books printed, were those on medicine, but they were generally translations of the Greek, Latin and Arab writers. From the year 1530 every branch of medicine began to quicken with new life. Hitherto medical writers had been dwarfed by scholastic despotism, but now they dared to throw off the fetters of traditional allegiance, and to think for themselves. During the following seventy years, many original, and for the times, excellent works on obstetrics and on allied subjects appeared from the pen of male physicians. To specify those authors with whose writings I am most familiar:—

In 1530 Ludovicus Bonnaciolus of Ferrara published his *Enneas Muliebris*. In 1542, there appeared *De Morbis Mulierum* by Nicholas Rocheus of Paris. In 1544, *De Partu Hominis*, by Eucharius Rhodion. This book passed through many editions, and was translated into several languages; among them the English in 1598, with the title of “The Birthe of Mankinde.” The original is printed in black letter, without paging, and contains the oldest representation of an obstetric chair that I can find. In 1547, there was published at Venice with illuminated text, the *Practica Major*, by Joannis Michaelis of Savonarola. It exhibits, by the way, the rudest of all obstetric stools.¹ In the same year, Martin Akakia of Paris, wrote *Medici Regii de Morbis Muliebribus*. In 1550, Hieronimus Mercurialis published his work *De Morbis Mulierum*. In 1555 Maistre Nicolle du Hault, who had previously annotated the works of Hippocrates, published *De Generatione*. About 1556, Pierre Franco, a Provençal physician of much repute, issued a tract on Obstetrics; but he stole most of his ideas from Paré. In 1557 there appeared *De Affectibus Uterinis*, from the pen of John Baptist Montannus of Padua. In 1580, Louis de Mercado, physician to Philip II. of Spain, wrote *De Mulierum Affectionibus*; and the celebrated Jacob Rueff of Geneva, an excellent work, entitled *De Conceptu et Generatione Hominis*. From 1581 to 1585, there were published

¹ American Journal of Obstetrics; February 1872, pp. 664 and 666.

works of considerable merit by Francis Rousset, Jean Liebeaut of Paris, Jean le Bon, Albertus Bottannus of Padua, Felix Plater of Basle, and by Maurice de la Corde of Paris. In 1597, Israel Spachius, an industrious physician of Strasburg, collected the standard obstetric works of his day, and published them under the title of *Gynæciorum Harmonia*. After 1600 the number of obstetric works is legion.

These authors no doubt paved the way for the employment of male physicians; but in my opinion it was mainly the great weight attached to the name of Ambrose Paré. In 1551 this eminent man published a small tract on version, which attracted much attention. Twenty-two years later, when his name was a household word, he wrote his work on Obstetrics, which was translated into every European language, and soon became the text-book of all the schools. Thus far, the ignorance of midwives had been gently censured, but no effort had been made to dislodge them from public favor; but in 1587 a very remarkable work appeared from the pen of Gervais de la Touche, "Gentilhomme Poitevin," in which he bitterly attacked midwives as a class, and, urged, for the sake of humanity, that the practice of midwifery should be entrusted to men. The title is a very curious one, and as it fully explains the character of the book, is well worth giving in full:—

"La tres-haute & tres-souveraine science de l'art et industrie naturelle d'enfanter, contre la maulite & perverse impéritie des femmes, que l'on nomme Sages-Femmes ou belles-mères, lesquelles, par leur ignorance, font journellement perir une infinité de femmes & d'enfans à l'enfantement: à ce que desormais toutes femmes heureusement & sans aucun peril ni destourbiez, tant d'elles que de leurs enfans, estant toutes saiges & perites en icelle science."

This quaint book was dedicated to "all queens and princesses, to all dames and damoiselles of honor, and to all debonair matrons of chastity and of long-suffering" and no doubt had its weight in opening the eyes of the "long-suffering" public to the shortcomings of midwives. But the increasing intelligence of the community was undoubtedly the true reason why the practice of midwifery gradually drifted out of their hands; and yet both Astruc and Louise Bourgeois missed it. The

former, who boasts in the preface of his work on midwifery, that he never delivered a woman, very naturally overlooked it. The latter, while imputing the cause to the wantonness and immorality of the women, unwittingly gives the true reason. For she advises that, in cases of flooding or of other dangerous complications, the midwife should send early for a surgeon, and not delay as long as possible, as many do lest he should get the credit of delivering the woman. This advice she excuses, on the ground that "extreme cases need extreme measures;" and as "I very well know from experience," she adds, "that if another midwife is called in, they go at each other tooth and nail (*se prendre de bec*), forgetting in their furious passion alike their patient and their duty." Therefore "It is far better to live at the hands of a bold and skilful surgeon than to die in those of an ignorant and rash midwife."

It would seem, then, that in proportion as people grew wiser by reading books and by having them to read, the ignorance of midwives became more and more manifest. The physician developed with the times, the midwife did not. The former wrote elaborate works on obstetrics, which the latter, with rare exceptions could not even read. What more natural than that intelligent women should prefer the teacher to the inapt pupil; should place their lives in skilled hands, than in those that were unlettered? What more inevitable than that the male physician, who was hurriedly sent for in cases of emergency, or was kept in waiting in an antechamber for such an emergency, should, despite tradition, prejudice and religion,—should, in spite of himself, for it was long deemed dishonorable for him to practice midwifery,—ultimately usurp the place of the midwife by the bedside of the woman in travail? The battle between knowledge and ignorance is never a drawn one; either Christian must die or Apollyon give way.

PELVI-PERITONITIS.

A CLINICAL STUDY.

BY

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Middleport, O.

EVEN since the graphic description of the symptomatology and pathology of this quite common disease by different gynæcologists, abroad and at home, among whom we may cite T. Gaillard Thomas as prominent, many practitioners and some writers still fail to recognize it as a disease separate and distinct from pelvic cellulitis. That it is often a complication of the latter, and *vice versa*, there can be no doubt. Nor can there be any reasonable doubt among those who have studied the clinical history and pathology of pelvi-peritonitis, without the prejudice of preconceived opinions, that the pelvic portion of the peritoneum may become inflamed without the inflammation extending to the pelvic cellular tissue, and without the peritonitis becoming general.

Why can we not have a circumscribed inflammation of the peritoneum as well as of the pleura? Who has not often observed at post-mortem examinations that a pleuritis has at some time in the past existed, and that it only extended to a small and well circumscribed area of the pleura? And who, during the lifetime of a patient, has not been able to diagnose positively the existence of a pleuritis not general, but limited to a small and well-defined portion of the pleura? Again, as mentioned by Thomas, we may have pneumonitis without necessarily having pleuritis, and *vice versa*. So we can have inflammation of the pelvic portion of the peritoneum without the morbid process extending either to the pelvic cellular tissue or the pelvic organs. As in the thorax the inflammation of one tissue is liable to extend to the others within the cavity, so an inflammation of any important organ or tissue within the

pelvis is liable to extend to some or all of the others within the pelvic cavity. Hence the frequent complications that have led to doubt as to the existence of the disease in question as a disease separate and distinct from others, especially from pelvic cellulitis. I believe it is generally granted that inflammation of the pelvic cellular tissue is far more apt to extend to the pelvic peritoneum than the reverse; hence it is that we so often find peritoneal inflammation existing when we find cellulitis present.

Another cause for the confusion that has existed, and still to some extent exists, is the fact that the disease under consideration has been differently named by different authorities; thus it is called perimetritis, parametritis, peri-uterine phlegmon, etc. I have preferred the name of pelvi-peritonitis as being the most correct, and the one chosen by Profs. T. Gaillard Thomas, C. D. Palmer, and others, and as being the one most expressive of the nature and extent of the disease; the other names indicating that the inflammation involves also the uterine structures, which is not necessarily the case in true pelvi-peritonitis.

The disease is an "inflammation involving the peritoneum covering the female pelvic viscera, and limited to it" (Thomas.) It is not, therefore, that form of peritonitis which, from some exciting cause, begins in the pelvis and becomes general, and is known as metro-peritonitis, and which is a disease of much greater gravity. Pelvi-peritonitis in its purity does not involve any structure but that portion of the peritoneum above described, but its complication with other pelvic diseases is common.

The symptoms and pathology of the disease were distinctly and prominently brought before the profession by Bernutz, in 1857, in a work published in connection with Goupil at Paris. It was then, I believe, first set forth as a distinct disease.

It is distinguished by severe and sharp pain within the pelvis, excessive hypogastric tenderness on pressure, especially when the pressure is made downwards and backwards; rigors accompanied with or followed by fever, nausea and vomiting; often considerable cerebral excitement manifested by delirium; facial expression very anxious, sometimes pinched; pulse fre-

quent, quick and hard ; often much ischuria and rectal tenesmus, both being sympathetic. The decubitus is not constant, while the paroxysms of pain—which are frequent—are most severe ; the patient usually lies on the side with the limbs well flexed on the body, which latter is curved well forward ; but when the paroxysms are off, the dorsal decubitus is most frequently maintained. At this early stage the heat of the vagina is increased by several degrees above normal, and the pelvic roof is still soft, but very tender, and seems fuller than in health. In a few hours, or a day or two at most, the pelvic roof becomes much more tense and firm, is extremely sensitive to pressure, especially to conjoined pressure, and careful manipulation discloses a greater fulness and sensitiveness at some point—generally at one side or posteriorly, sometimes anteriorly—than had previously existed. By this time the roof may become very hard in a portion of its extent. If not, it soon will present an astonishingly hard sensation to the finger on pressure, being not inaptly compared to a deal board. Either a well-defined tumor will be felt in some portion of the pelvic roof, or the whole roof becomes very firm and tender. The fulness above described is caused by the extravasation of lymph, at first, of course in a fluid state, but as this becomes organized, the pelvic roof becomes harder and harder as a result. Through this board-like pelvic roof projects the vaginal portion of the cervix uteri, as fixed and immovable as though moulded around with plaster of Paris. Sometimes the cervix is tender ; usually pain is only produced by an attempt to move it with the finger, when the pain is found not to be in the cervix or uterus, but in the inflamed peritoneum around it.

Most authorities speak of a well-defined tumor usually to be felt at one side of the cervix, of the size of an orange, or larger, and usually at the most dependent portions of the peritoneum. In most of the cases coming under my observation, there has been found a firm, hard and smooth pelvic roof without there being a distinct tumor. It is true, however, that the roof may be hardened at one side only, giving the sensation of a tumor on that side ; but in every instance in my cases the hardening process, by the organization of plastic lymph already thrown out, has gone on until the whole roof

became nearly alike hard. This plastic roof is always very tender and painful in acute cases, and is so only in a less degree when the disease becomes chronic. Through the course of an acute attack, the constitutional disturbance continues to be active and quite severe. There is often a frequent recurrence of rigors accompanied by sharp pelvic pain, often spasmodic in character, and followed by fever, which in turn may be followed by free perspiration. It is true that many of these phenomena are present in pelvic cellulitis, but there are a few well established landmarks which indicate decided differential points between the two diseases. In pelvi-peritonitis, the tumor, if felt above the pelvic brim, is seldom large, is more apt to be central, is hard and exceedingly tender, while in cellulitis the tumor is nearly always distinct and lateral; tender, but more of a doughy or boggy sensation is given to the fingers. The intra-vaginal symptoms are very widely different. In distinct pelvi-peritonitis the roof is always hard and firm—board-like—in a part or its whole extent, while in distinct cellulitis such is not the case. In the former the cervix is perfectly immobile, in the latter not. The tenderness above the brim is in the former nearly always central; in the latter lateral. There is not nearly as marked a tendency to suppuration in distinct pelvi-peritonitis as in cellulitis. I have never had charge of a case that resulted in suppuration. Cellulitis often does, or is very liable to result in the formation of a pelvic abscess. In cellulitis there is no necessary displacement of the uterus, while in pelvi-peritonitis the uterus may often be found crowded to one side, or late in the disease drawn to one side by the contraction that has taken place in the tissues, while the system is trying to dispose of the organized lymph. In the former there is no tendency to periodic relapse; the nausea and emesis are not excessive as a rule; the facies is less anxious, the pain is not so distinctly paroxysmal, the patient apprehends little danger, while the reverse of all this is generally true in the latter. It must not be forgotten that these diseases very frequently complicate each other. The above description and differential points refer to cases that are distinctly one or the other.

The pathology of pelvi-peritonitis is the same as that of inflam-

mation of serous membranes elsewhere, and need not be rehearsed tediously here.

The causes are also the same as those causing other forms of pelvic inflammation, and inflammations of other serous membranes.

Illustrative of this disease, I will relate a few of the well-marked cases that have come under my care in practice, giving a few passing remarks on the way. The first was one where the disease had become very chronic before coming under my notice.

CASE I.—Mrs. X. æt. 33, of lymphatic temperament, mother of six children ; is of short, heavy build, flesh soft and flabby, is very anæmic, formerly of excellent health, but was unfortunate in becoming inoculated with syphilis by her husband, of which, however, she was not aware. For this disease she had been very irregularly and imperfectly treated. She called at my office May 13th, 1875, complaining of an excessive mucopurulent leucorrhœa, and continued severe, heavy, pelvic pain, and costive bowels. I found the tongue thick, pale, flabby, printed by the teeth, and covered with a heavy white fur. The pulse was 84, with a quick irritable stroke, not forcible. She informed me that, soon after my attendance upon her, two and a half years ago, (at which time she was suffering from secondary syphilis) she became pregnant, and miscarried at about the sixth month ; that she had severe hemorrhage, becoming greatly reduced, and upon getting around contracted a severe cold, immediately afterwards taking her bed, and suffered extremely with what her physician said was inflammation of the womb. This attack kept her in bed most of the time for three months. Has been in feeble health ever since. She menstruates regularly and excessively, suffering with severe sharp pain every time the menses appear, and as long as they continue. A vaginal examination disclosed a very lax mucous membrane, discharging freely a mucopurulent secretion. When the finger reached the pelvic roof, it was found to be very hard and wood-like, and quite tender, the cervix drawn a little to the left, completely immobile, os patulous, not especially tender. The uterine sound disclosed the fundus inclined anteriorly and laterally to the right. Diagnosis, chronic pelvi-peritonitis, compli-

cated with excessive anæmia and menorrhagial dysmenorrhœa. I prescribed for her the following:

R Tr. gentian comp.

Spti. rectificatus āā ̄ ij.

Fl. ext. nucis vom. ̄ i.

Ferri citratis ̄ i.

Syr. aurantii ̄ ̄ iv.

Misce. Sig. tablespoonful before each meal.

Also

R Hydr. bichloridi grs. ij.

Resin. podophyl grs. i.

Pulv. aloes Cape grs. xvi.

Pulv. capsici grs. iv.

Misce. ft. pilulæ xxxij.

Sig. Take a pill after each meal.

Ordered the hypogastric region to be well covered with Tr. iodine decol. every day, and the use of a vaginal suppository of mercurial ointment and aqueous extract of opium every night at bed-time, the vagina being first thoroughly cleansed with a *hot* solution of tannin. injected with a Davidson's syringe. Under this treatment, in one month she had gained very considerably in general health and comfort, the vaginal discharge was much diminished, also the pelvic pain. She had suffered less pain and less discharge at the last menstrual period, consequently came through it feeling much stronger than on former occasions; her bowels had become regular. The treatment was continued two months longer, with little change of medicine or application, but a change from the town to country life among the Virginia mountains was recommended and complied with. At the end of this period there was great general improvement. The local pelvic suffering was quite relieved in every particular, and the woman looked herself again. A vaginal examination showed the pelvic roof still very hard, uterus fixed, less tenderness. I now placed her on the sulphide of calcium grs. iij, hydr. proto-iodide, one-tenth of a grain, three times a day, in pill. with request to report again in two weeks. To my happy surprise, at the end of this time

there was a great change in the pelvic roof. Though there was still considerable induration, the roof was much more pliable than two weeks before. Treatment continued another period of four weeks, when the entire disease seemed to be removed. The uterus was considerably lower than normally, but a pessary was not introduced till a few weeks later, on account of a possibility of its causing a return of the trouble. This case proves one of two things, either that the treatment was illy directed, or that the disease, when it reaches the chronic stage, is very obstinate and difficult to relieve. It has been generally observed by most authorities on this subject, that such cases are of long and tedious duration, often outwinding the patience of the afflicted woman, and, not unfrequently, trying the endurance of the attendant also.

This lady stated that she had taken no precaution to prevent pregnancy, yet it had not occurred, nor has it taken place since first afflicted with this disease. Pelvi-peritonitis is a fruitful source of sterility, in consequence of the morbid condition produced in the ovaries by the deposition of plastic lymph around them, which often permanently binds them too firmly for all time to permit their performing their physiological functions during the act of coition. Besides, it seems not improbable, that a morbid process so violent or long-continued, does or may produce disorganization of the ova to a sufficient extent to prevent their perfect development. This, too, may be fortunate for the woman; for where strong pelvic adhesions occur between the uterus or its appendages, and the surrounding peritoneum, which in turn has its folds firmly glued together, often glued to the intestines, bladder or walls of the pelvis, impregnation would be a mishap, for the expanding uterus would be so firmly bound down that its natural increase from gestation could not be easily, sometimes not at all, accomplished. A few cases where death has occurred to the pregnant woman, caused by such adhesions, are already on record from responsible authorities.

Whether the sulphide of calcium was the active agent in removing the chronic induration in the above case, at the stage in which it was given, or whether it was due to the great improvement in her general health, I am not fully prepared to say. Such an inference may be drawn, yet we cannot overlook the

fact that mercury was continued throughout the whole course, and that iodine externally was applied a large part of the time while she was under treatment.

CASE II.—Mrs. K. æt. 37, mother of one child now eighteen years old, claims to have had no abortions up to the present one, is of spare build, nervous temperament, the subject two or three times a year of epileptic paroxysms; has from one to three at each attack, for which she has taken largely of potass. brom. almost daily for four years past. She aborted Sept. 22, 1874, which, I think (but do not know) was purposely induced. I did not see her until the 28th, when I found she had been losing much blood, was very anaemic, facies anxious, pelvic pain very sharp and spasmodic, great vesical tenesmus and irritation, very tender over the central hypogastric region, where the enlarged uterus could still be felt. She was having frequent rigors, followed by fever, the thermometer registering 103, pulse small, wiry, 112. Stated that "all of the after-birth had come away three days before," and she had gone out on the morning of the 28th, in the cold damp air, getting thoroughly chilled before returning to the house. I found the vagina exceedingly hot, pelvic roof very tender, soft and puffy, and I thought I could detect the existence of fluid above the vaginal walls; uterus not especially tender, os patulous and normally situated. Order hot hop poultice to the lower abdomen, gave opium largely to control the pain, and as far as possible prevent the inflammation from progressing. Strict quiet enjoined, and diet to be nourishing and such as would leave but little residue.

On the 29th I found her with a temperature of 103, pulse 120, quick, irritable, small; tongue pointed, very red, countenance pinched and anxious, severe headache, frequent paroxysms of pain of a keen, cutting nature in the pelvis, decubitus left lateral, limbs drawn up a little; abdomen very tender just above the pelvis, and in the left region, slight tympanites, vagina hot, slight sanguineous discharge, left and posterior cul-de-sacs full, tender; and thought it positive that there was considerable fluid just above the roof. I continued the opium, with one grain of calomel and quinia sulph. grs. iv. every six hours.

On the 30th, the general condition was quite the same; had less severe pain; temperature 102.5; pulse 110°; urine had been

quite free, vesical tenesmus causing no further trouble. The pelvic roof was now becoming quite firm on the left and posteriorly, is very tender, decubitus dorsal, limbs very little flexed, tympanites distinct, not extensive. Treatment continued, except the mercury, which was left off.

October 1st. General condition little changed, pelvic roof now very firm and hard, tender posteriorly and to the left, right side not so tender nor hard, but full. Her strength seemed to be rapidly on the wane, she loathed food, stomach irritable, had vomited some, causing great pelvic distress. Treatment continued, stimulant added, to have milk, toast, bread—soup, animal broths, &c., as much as she could take and retain, and to use pounded ice freely, blister to the hypogastrium $4 \times 6''$.

There was little change for the next week except a marked relief from general suffering; but she was still very much prostrated, suffering at times much pelvic pain and also pain in the left thigh, inner side. The whole pelvic roof meantime had become very hard, the cervix being crowded a little to the right, where it was perfectly immovable; tenderness rather less on pressure. To make a long story short, from this time on for the next eight weeks there was a gradual amelioration of the general suffering and some continuous improvement in strength. But the local trouble was less yielding, though the pelvic pain had become infinitely less than formerly, the pelvic roof however remained as firm as ever. During this period, she had been plied with tonics, alteratives, good diet, and anodynes when necessary; local applications of iodine, mercury, &c., The patient was now comparatively comfortable while in bed, but could not sit up or attempt to walk without suffering severe pain and feeling greatly exhausted. The menstrual discharge now made its appearance slightly, accompanied with extreme pain and increase of all the trouble for two days; after this subsided there was little if any noticeable change in the situation.

I now removed all alterative remedies, continued the general tonics and good diet, and put her on calcium sulphide gr. iv. every six hours, using pledgets of cotton soaked in glycerine, applied to the roof of the vagina. This application caused a free watery discharge from the vagina. A week later, the indura-

tion was evidently disappearing; and in still two weeks more the pelvic roof had become as soft and elastic to all appearance as it ever was, except a small portion in the posterior cul-de-sac, which hardness finally disappeared. Both of these cases were distinctly those of pelvi-peritonitis; certainly showing histories and symptoms widely different from pelvic cellulitis. The natural tendency to suppuration would have ended—in all probability—in either of these cases in the formation of abscess long before resolution occurred. The tendency to the formation of pus in purely pelvi-peritonitic cases is not marked. I have never seen one result in the formation of an abscess. On the other hand cellulitis or cellulo-peritonitis of the pelvis do dispose to resolution by the formation of pus, often forming large and dangerous abscesses.

The tendency of pelvi-peritonitis to become chronic is so well marked, that it has been noted by most of writers on the subject, and seems to be a well-established clinical fact. There is also a well marked tendency to monthly relapses in many cases, especially among those who have an occasional light attack about the time of menstruation, caused by exposure or otherwise. This affords a solution of the difficulty of readily and quickly disposing of them, even where the attack is not severe. Two years ago, while treating a young lady, naturally of good health and constitution, for uterine disease, I found the pelvic roof very firm, quite tender and unyielding, and the uterus bound firmly down, not being more than one and three-fourth inches from the vulva to the os, the fundus being laterally displaced and somewhat retroverted. Her clinical history was that of dysmenorrhœa for the two preceding years, caused by severe cold at first, but the pain returning regularly always thereafter at each period. Here was evidently a case where repeated attacks of pelvi-peritonitis had resulted in permanently fixing the uterus, and probably destroying the ultimate functions of the reproductive organs for all time to come.

In another instance, a married lady came to me for pelvic "aching" from which she had been constantly suffering for several years. She had been married for six years, had never been pregnant, nor had she (as she stated) tried to prevent such an occurrence. I found the uterus greatly prolapsed, being one inch from the vulva to the cervix, as firmly fixed and immov-

able as though surrounded with plaster of Paris, the pelvic roof being hard and tender. She gave a history very similar to the one above-named, only the pelvic trouble had existed much longer. This case ultimately yielded to a general alterative and tonic course, with local applications of glycerine on cotton, with frequent warm water vaginal douches, though the course was long and tedious. She has never become pregnant. Several similar cases have come under my observation. In no instance have they become pregnant. Most of them have remained uncured, unless they have each submitted to a long and tedious course of treatment both systemic and local.

CASE III.—Mrs. L. æt. 28, mother of three children, youngest three years old, aborted from a fall. She was of good health, strong and active, nervo-sanguineous temperament. Went out prematurely; and a needless exposure to cold, with wet feet, on the eighth day after the accident, caused severe pelvic pain of a sharp, cutting and spasmodic character, accompanied by rigors and fever, facies anxious, at times delirious. Treatment, opium internally, and hot stupes of turpentine locally. The next day the pain was less severe, but she still suffered greatly when not fully under the influence of the anodyne. The vagina was very hot; pelvic roof soft, tumefied, tender in the extreme, uterus not very sensitive. Two days subsequent to this time I found the posterior cul-de-sac hard and firm, tumor as large as a good sized orange, filling the space and crowding the cervix forward. The remaining portion of the roof, though firm and tender, was not quite so hard. The tumor was very sensitive. The hypogastric region very tender centrally, some tympanites, febrile excitement high, occasional rigors and attacks of pain, urine free, but there was considerable vesical tenesmus. Temperature $103\frac{1}{2}$.

Treatment. Opium grs. i to ij, as often as required, also hydr. chlor. mitis gr. i., quinidiæ sulph. grs. vi. every six hours, blister to the hypogastrium $5'' \times 6''$, and glycerine and tinct. opii, equal parts, applied every four to six hours on cotton, to the roof of the pelvis. The general suffering soon subsided, so that moderate doses of opium kept her quite comfortable. The quinidia was continued for a week, the hydr. chlo. mitis being omitted after the second day. She was also given potass. iod, grs. x., every six hours, after the first four days, the glycerine and landa-

num being continued, and occasional douches of warm water given. The tumor remained very hard and tender for three weeks, when it gradually became more consistent. It was not, however, entirely removed until the ninth week, when the pelvic roof was found to have become soft and elastic throughout its whole extent, though her general health was not fully up to the normal standard. She was now placed on a general tonic, after which she soon became to all appearances perfectly well, the whole course from first to last, while she was under treatment, being quite three months.

I have purposely omitted the mention of any efforts to regulate the action of the bowels during the treatment of acute cases. This must be done with great caution. In fact, for several days after the acute symptoms set in, I have deemed it best to keep the bowels closed, and not permit any action for the first four or five days, and then to accomplish their movement by repeated small doses of sulphate of magnesia to render the ejecta soluble and easy to pass. This plan is made still better by giving small enematas of warm water, every two hours, soon after commencing the salts.

The reason for preventing peristaltic action during an acute attack, especially in its early stage, must be apparent to all.

CHLORAL HYDRATE IN OBSTETRICS.

BY

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THE use of anaesthetics in obstetrics has grown into great favor within the past few years, and notably within the last decade.

Many practitioners, submitting to the importunities of their patients to "give them something which will render them unconscious of their pains," permit the inhalation of chloroform or some of the ether compounds for hours during a single labor. The application of anaesthetics in these cases has given rise to

serious studies : and from a number of observations made in the several stages of natural labor, it is shown that in chloral hydrate, we have a remedy less objectionable, and very nearly as effective as its more powerful relative, chloroform.

Dr. Franco Mozorrra has collected the observations made in fifty cases wherein the chloral was employed, being enabled from his personal experience to analyze the details. He arrives at the following conclusions.

1st. The hydrate should be pure, otherwise it may be dangerous to use it, or, at least, it may be without any true therapeutic effect.

2nd. In women, during labor, the chloral first acts as a calmative, thus producing sleep, and considerably diminishes suffering.

3rd. The uterine contractions continue during the sleep caused by this agent : They are shorter, less frequent, pretty energetic, and generally the duration of the labor is shortened by the influence of the chloral.

4th. The anæsthesia produced by chloral may be sufficiently complete, and render the woman quite unconscious, even during the expulsive period, and for some time afterwards.

5th. Given in small doses, chloral sometimes causes agitation, which ceases when the dose is increased.

6th. The remedy may be given at any period of labor.

Dr. Playfair, in a recent discourse upon this subject (*London Lancet*, February 1874), makes the statement that chloroform, as an anæsthetic during labor, shows a tendency to do more than is desired ; that, while it diminishes the pain, it at the same time lessens the force of the uterine contractions, thus prolonging the labor. He also objects to its use, on the ground that it predisposes to post-partum hemorrhage.

Chloral, on the other hand, he regards as superior to chloroform in many respects ; notably in the fact that, while it diminishes their painfulness, it does not seem to diminish the strength or frequency of the pains.

Dr. Playfair claims that chloral recommends itself more emphatically by reason of its being applicable at a period when chloroform is scarcely to be thought of, towards the close of the first stage of labor, before the complete dilatation of the os, and when the sharp and grinding pains produce more suffering,

and are less easily borne than the more propulsive pains of a later stage of the labor.

In women of a highly developed nervous organization, such as constitute a very considerable portion of our patients of the higher class, while the cervix is yet undilated, or the membranes not ruptured, the pains are severe, but short and ineffectual, chiefly limited to the back, and producing little or no effect in dilating the os, he has found chloral especially valuable. To administer opiates in quantities sufficient to procure sleep, as has been the custom in this class of cases, was to suffer the disadvantage of arresting the labor and losing time; while on the other hand, if chloral be employed instead of opiates, the same refreshing sleep is induced, without any suspension of the pains or protraction of the labor.

Also, in cases where there is rigidity and spasm of the cervix, or where the latter is thin and rigid, with a sharp edge, the chloral is exceedingly useful. Soon after it has taken effect, the author has observed, not infrequently, a thin os which had remained unaltered in character for many hours, dilate rapidly, far more so than when chloroform is inhaled for this indication.

His mode of administering the drug is as follows. He orders a six-ounce mixture containing a drachm and a half of chloral.

When the pains are becoming severe, which is not usual until the first stage of labor is approaching completion, he gives one sixth part of the mixture, that is fifteen grains. This he repeats in twenty minutes. The subsequent administration must be regulated by its effects. Usually after the second dose, enough has been taken to bring the patient under the influence of the remedy sufficiently. It is seldom necessary to give more than a third dose, and the author quoted has never given more than a drachm of the chloral in an entire labor. By lessening the quantity after the second dose, and increasing the intervals between their administration, a full and sufficient effect can be kept up for many hours. It need not interfere at all with the use of chloroform, that may be inhaled when the pains grow strong and forcing, just as if chloral had not been given.

About five years ago I was in attendance upon a young

woman, in her first labor, who, almost from the very first pain manifested such a restless and sensitive condition, and gave such complete vent to her feelings in cries and moans, pleading with nearly every breath for the privilege of inhaling chloroform, that I felt it a necessity to administer some drug to procure for her some degree of quiet.

At that time I had never heard of chloral being used as an anæsthetic in obstetrical cases, but feeling that it would not be proper to commence the administration of chloroform in the midst of the first stage of labor, where, from the thin, sharp, and dense condition of the os uteri, I was led to infer that some hours would elapse before the second stage would be reached; I concluded to try the chloral, hoping that if I failed to procure sleep, the fact that I was making the effort to relieve her suffering would to some degree allay her irritability. Mixing twenty grains of the chloral with about an ounce of sweetened water, I gave it, and at the end of twenty minutes, finding the patient partially under the influence, I gave fifteen grains more, when complete anæsthesia supervened. I watched the case closely, and found the uterus contracting at regular intervals, showing a good deal of strength, and gradually dilating the cervix. By repeating my chloral in fifteen grain doses, every 45 to 60 minutes, I was enabled to keep up a sufficient anæsthetic effect until the strong and forcing contractions of the second stage were reached—a space of about four hours.

Since that time I have repeatedly used the chloral and have been well satisfied with the results obtained.

In a number of cases in which I have employed this drug, where there was a thick and rigid os, or again where the os was thin, with a sharp, dense edge, and not dilatable, wherein the pains were mainly in the back,—short, severe, and producing but little effect in dilating the cervix; I feel entirely satisfied that I have been rewarded with a more speedy relaxation and dilatation of the cervix, a more speedy completion of the labor, while at the same time, the patient was rendered more comfortable.

I prepare the chloral for use after the following formula:

Hydrat. Chloral..... ʒ ii

Syrupi Simpl.

Aqua font..... āā fl. ʒ ss

In this form an ounce of the mixture, a quantity sufficient for any given case, can be put into an ounce bottle and carried in the vest pocket.

Of this mixture I give one drachm. (that is, 15 grains of the chloral,) in a wine glass of water at a dose, repeating the dose every 20 minutes until its soporific effect is produced. In exceptionally severe cases, with strong pains and a robust constitution, I give a drachm and a half for the first dose, in order to procure a prompt effect from the remedy.

It is occasionally necessary to follow with chloroform, toward the last stages of labor; but for the classes of cases heretofore named, and during all the earlier stages of parturition, I most heartily recommend the use of chloral hydrate where an anæsthetic is indicated.

BLOOD-LETTING IN PUERPERAL ECLAMPSIA;—PATHOLOGY AND THERAPEUTICS;—THE OLD AND THE NEW.

BY
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“There is a path that leads to truth so surely, that any one who will follow it must needs reach the goal, whether his capacity be great or small. And there is one guiding rule, by which a man may always find this path, and keep himself from straying, when he has found it. This golden rule is:—Give unqualified assent to no propositions but those the truth of which is so clear and distinct that they cannot be doubted.”—*Descartes—Huxley.*

EXPERIMENTAL PHYSIOLOGY AND THE MODERN PATHOLOGY OF ECLAMPSIA.

In the following paper we propose to examine, more or less cursorily, some of the modern views in regard to puerperal convulsions, as we find them expounded in a few of the more recent dissertations on the subject.

The convulsions of pregnancy and childbed occur under circumstances which characterize the advent of no other class of serious nervous perturbations. Concomitants dependent upon the peculiar general condition of the subject, the special condition of the blood, the state of the patient's nutrition and of her organs of elimination, serve to embarrass the explication

of phenomena which, in other forms, are most readily comprehended. It is not surprising, then, that several of these concomitants—influencing the convulsive manifestations as they often do—are not rarely mistaken for the proximate cause, or regarded as constituting the disease itself. Plethora, spanæmia, hydræmia, anæmia, besides many toxic conditions, the most prominent of which is uræmia, have each in their turn, served as the specific theoretic condition, underlying and evoking the convulsive phenomena which characterize eclampsia.

Theories, predicating the essential identity of uræmia and eclampsia, have brought puerperal convulsions into intimate etiological relation with Bright's disease of the kidney. These last relations are indeed striking and also of the very greatest importance. The general phenomena of Bright's disease, as regards the vascular system, the nervous system, the toxæmia—and often too, the aberrations in the renal secretion, as to constituents, volume and specific gravity, present themselves as a totality arising out of conditions of the body, if not identical with, certainly very analogous to, those existing in many cases of puerperal convulsions. For these reasons, it would seem unavoidable, that any course of investigation, seeking to trace the phenomena, in either case, back to their original sources, should cover ground in many respects not altogether dissimilar. And yet, the two tracks—parallel for awhile—at a certain point naturally and necessarily diverge into separate lines of both interest and research. The convulsive phenomena of Bright's disease are but the occasional evolvement from the condition of the kidney and blood-system, (capillaries and blood); while, on the other hand, the frightful convulsions of childbed—whatever may be the hidden causal basis upon which they may be founded—form, so to speak, “the head and front of the offending.”

Not always, but sufficiently often, has scientific opinion—we might say, scientific demonstration—located this etiological starting-point, in part or in whole, most plausibly in the kidney;—and sufficiently analogous are many of the attending symptoms—even outside the renal system and the albuminuria—as cedema, giddiness, blindness, sparks, flashes,¹ somnolence,

¹ From which the term *eclampsia* has originated.

&c., to render perfectly legitimate, if not unavoidable, the recognition of their very close relation. Indeed, there have been established in the minds of all, bonds which indissolubly associate them in every reasoning process, by which we would essay to study their individually complex and mutually intertangled phenomena. The widely differing prominence, however, of the two classes of phenomena, as characterizing each of the two diseases, of course now determines the investigation into distinct and separate lines. The majority of modern pathologists, while they by no means overlook the conditions of the kidney, so often aberrant during pregnancy, either in structure or function, nor the blood-system and its toxic conditions—still naturally, and we think very properly, carry their deepest researches into the penetralia of the nervous system, as the theatre in which the astounding drama—too often a tragedy—of puerperal eclampsia is enacted.¹

The renewed activity given to vivisections some time ago, (about 1850,) by Dr. Claude Bernard, and scarcely allowed to flag for a moment, by Dr. Brown-Séquard, seems in the last ten years or more, to have experienced a most decided exacerbation, in which ingenuity in devising, and cunning dexterity in performing, the operations have been sometimes perhaps the most notable characteristics of the experimental era comprehended in that period. A number of other distinguished gentlemen have also been prominently engaged, with varying degrees of success, in one way or another—whether by mechanical or electrical stimulation, by arterial ligations, or by destructive processes, in most commendable and arduous efforts to de-

¹ Among other more lengthy, and sometimes less meritorious discussions, it affords us pleasure, though differing entirely from him in many of his conclusions, to refer to the report of Prof. J. Ford Prioleau to the South Carolina Medical Association, at a recent meeting, as a comprehensive though compendious presentation of the more prominent views of certain modern pathologists and practitioners. This author reviews the earlier observations of eclampsia, not wholly neglecting any shade of opinion from Hippocrates to the present time. The doctrines of irritability, of plethora, and of the opposite state, anæmia, of the various toxic and other conditions of the blood, as uræmia, are clearly though briefly reviewed; but it is to the more recent experiments and observations upon the nerves and nerve centres that our attention is most hopefully directed, with the object of evolving the more modern ideas as to the pathology of eclampsia, as well as the guiding principles as to its treatment.

termine the endowments of various cranial nerve centres. Much of their attention has been given to the convolutions of the cerebrum; and while the observed effects of stimulation have appeared to themselves, and to some who have tested their processes, somewhat remarkable, we must say that some of their *conclusions*, if we understand them aright, appear to others even still more so. While Dupuy and Carville are said to seriously question some of these results, these multifarious experiments seem, in the minds of some, to have completely dethroned the heretofore recognized motor ganglia, as the controllers or instrumentalities of muscular activity. For an exposition of this calamitous schism, we here quote briefly from the report of Dr. Prielean: "Especially to Dr. Marshall Hall is the profession indebted for the evidences which confirmed the theory of the diastaltic action of the nervous system, which knowledge has been the clue for the explanation of so many, and often so apparently divergent phenomena, applicable to both the healthy and the diseased condition. The carefully exact and laborious researches of such profound thinkers, skilful vivisectionists, and learned physiologists have deservedly influenced the minds of the profession. Among numerous facts relating to the nervous system, they had apparently established the localization of the centres concerned in motor energies, finding them in the masses of nerve matter, consisting of the corpora striata, the thalami optici, the crura cerebri, the pons varolii, the peduncles of the cerebellum, the lateral portions of the medulla oblongata and the anterior columns of the medulla spinalis. These portions of the nervous system have been so generally received as the seat of motor influence, and consequently the parts from which convulsive action can only [?] take its origin, that we are almost confounded to find, at this day, such assertions questioned, and that the motor forces could be engendered by any other portions of the nervous system."

Thus, it would appear, there is a tendency in the minds of some to transfer from the ganglia at the base of the brain, and from the motor tract, the seat of motor influence, so long held to reside in these parts, and to locate it in the surfaces of the convolutions. For our own part, while we are willing to admit that a series of experiments upon the cortical surface of the brain may have been valuable in evolving facts as to the endow-

ments of the convolutions generally, and also as to the specific influence of particular convolutions upon other portions of the nervous system, *as will-centres*, we see nothing whatever in any of these experiments to invalidate the results established by Marshall Hall and others, or in the least to justify the very grave conclusions apparently arrived at by the distinguished gentlemen who have been so ingeniously engaged in them. Indeed we cannot recognize any difference whatever in the phenomena evoked by most of these experiments on the convolutions, and the results of very many accidents and cerebral irritations which have been, from time immemorial, the facts of common observation, and which occur so entirely in accordance with well recognized principles of nervous physiology, that no process of reasoning is ever entered upon for their elucidation. "There is perhaps," says Dr. Marshall Hall, "not a point on the general cutaneous surface, in which tetanus, an excito-motor effect, may not originate,"¹ and the convulsive phenomena of an epileptic kind, so constantly observed to result from spiculæ of bone, from punctured fractures, from small clots, or from spots of congestion, acting as irritations on the cortical surface of the convolutions, have exactly the same mechanism as the experimental irritations; and only differ from the mechanism of tetanus, in that, in the one case, a centripetal sentient nerve is the excitor, while in the others the irritation is transmitted to the motor centres by a less elaborate and more direct process of centric reflection. The aptitude of ganglia and nerves of special sense or function to manifest their peculiar kind of activity under either artificial or morbid irritation, is familiar to every one. The eye will experience subjective flashes of light, and the ear subjective sounds, on the excitation of the nerves and ganglia endowing these organs with their respective special functions. Nothing within the range of experimental physiology to us would seem more natural or more to be expected, than that irritations of the vesicular covering or of the fibrous basis of the convolutions—be it of whatever kind it may—should be responded to by involuntary contractions in the muscles, through molecular changes propagated along the fibres connecting the convolutions with the true motor ganglia

¹ *London Lancet*, April 1857. Letter on "Excito-Secretory Function of the Nervous System."

and motor tract. This surface is the organ so far as we can locate it, of *will-force*; these fibres, the avenues of *will-transmission*; and these ganglia at the upper end of the spinal cord, are the habitual recipients and executors of *will-mandates*¹ As the retina responds normally to rays in the evokement of luminous impressions, and the tympanum to vibrations in the evokement of sound, so does the convolution-surface of the brain, as it is thought, respond to the will in the evokement of voluntary muscular activity: and, on the other hand, as artificial stimuli can evolve subjective light and subjective sound, so in the case of the will-organ—as observation and pathology have demonstrated—when an artificial or a morbid excitation is substituted for the will, do we ever find the train of phenomena simulating, as nearly as possible, the results produced by the natural stimulus; that is, evoked *muscular action*, but *without the will*;—literally, involuntary contractions; in this case, called, “convulsions.”

No variety of opinions as to the alleged seat of the will, can materially affect our statement in regard to the interpretation of the recent experiments of Hitzig, Ferrier, Bartholow, and others, resulting in the transference of motor energy from the basal ganglia to the convolutions. It is more in accordance with what we know to be the normal result of will-action in the convolutions, to suppose that the *experimental* stimuli exciting spasmodic movements propagate molecular action along the connecting fibres to the basal ganglia in the same manner, that it is habitually propagated by the *intellectual* or *will* stimulus, in awakening these motor instrumentalities in the ordinary voluntary acts, than to admit the new theory attempted to be founded on these experiments.

And yet it is held that “this recent advancement of our knowledge of the functions of the hemispheres enlarges our views as to what may be the seat of convulsion, and furnishes us also with an insight into one of the many direct causes, by giving as evidence that an irritation applied directly to the

¹ Dr. R. B. Todd, in expressing himself on the subject of The Will, remarks, “that the cerebral convolutions with the fibres that connect them with the corpora striata and optic thalami constitute the centre of *intellectual action*.” *Cyclopædia of Anatomy and Physiology*, London, 1847. Article “Physiology of the Nervous System,” p. 723, E.

cortical portion of the brain may be as efficacious in the production of a fit, as can irritation directed to the medulla itself."

There can be no doubt that, in pregnancy and during parturition, conditions of the vesicular covering of the convolutions frequently exist whereby the convulsions are initiated, though we cannot agree that they arise from irritations transmitted *invariably* in this direction. "Metastatic labor" is a term used by one of the most philosophic of not very recent writers, Dr. Power, to indicate the discharge of the "*vis nervosa*" in other directions than in the production of contractile energy in the uterus. No one aware of the term can fail, in the observation of certain cases of puerperal convulsions, to have suggested to his mind that to change the phrase a little, a convulsion is in some respects, what might be termed a *distributed labor pain*. Few cases there are which can be regarded as apt illustrations; but, in these, the convulsion invariably returns soon after the beginning of a uterine contraction which has proved abortive; and, on the other hand, convulsion seldom takes place after the full establishment of the uterine action. There are convulsions belonging to the almost repudiated class, known formerly as those of "the hysteric kind:" and which, when occurring after labor, are almost invariably initiated by a uterine contraction or "after-pain." The purely reflex phenomena present themselves in all degrees of activity, and are not always purely convulsive. The reflected irritations may evoke sensations as well as spasmodic actions; and hence the chill as well as the jactitation; which latter we observed to occur on one occasion, after labor, with a violence and prolonged endurance, well comparable to the convulsions of true eclampsia, while the consciousness of the patient was not in the least impaired. They affected the muscles of the body *laterally*, and the alarm of the patient, a very intelligent lady, was most excessive. The after-pains were quieted by forty drops of tincture of opium per anum, and the convulsions ceased promptly on the subsidence of the uterine irritation which unquestionably evoked them. It is true, that at the present day, the interest excited by eclampsia often leads us into more recondite and therefore more attractive fields of study than those presented by the easily-read page of reflex nervous action; but may not such transition cases as the one just related, we would

humbly ask, serve as links which, if carefully studied, might complete the chain of facts necessary to the attainment of a more correct pathology?

The direct corollary from the modern interpretation of the facts of experimentation on the nervous system in relation to eclampsia, is to involve us in a discussion ending in positive denial of all former views as to depletion and repletion in the cerebral circulation. Though nearly all of the most distinguished teachers and experienced practitioners have expressly recognized the influence of plethora, congestion, and cerebral fulness, as the most common causes of convulsions, grounding upon them frequently the imperative necessity for venesection, there is no opinion now so opposed, almost with derision, as that in which convulsions are attributed to "a determination of blood to the head."

Great plausibility is apparently given to these adverse views, Many and pertinent and significant are the observations and experiments of able and distinguished investigators constantly adduced to substantiate the fact, that *excessive losses of blood produce convulsions*; and that the mode of death in extreme exsanguination is one in which convulsive symptoms are often the most prominent element of the struggle. By venesection, by arteriotomy, by compression of carotids, and by ligation—indeed, by every ingenious method in which the brain could be suddenly or slowly depleted to the extreme of anæmia, did the earnest and faithful investigators of this hopeful question energize in the elucidation of the truth. Trousseau's conclusion should have, perhaps, as weighty authority as any, even at the present time, though he did not seem to adopt entirely either depletion or repletion as the sole origin of the convulsions. "There are," says this author, "and I lay great stress on the point, two very distinct conditions in the attack of eclampsia or of epilepsy, whether idiopathic or symptomatic: 1st. A cerebro-spinal modification, unknown in its essence and its nature, which in a second abolishes all manifestation of animal life; of the two, this is by far the most important condition. 2d. A secondary congestion, which, although less important, may in some extremely rare cases be carried so far as to produce subcutaneous ecchymosis, cerebral capillary hemorrhage, and even meningeal hemorrhage."

Attractive as we might make this synopsis of modern views, we cannot occupy further space with opinions, however authoritative, so widely published as these are in books and journals at the present time. The accumulation of experiments, the variation in the vivisections, the deductive results arrived at, by the earnest investigators of the present era in this kind of research, have all tended strongly in one direction. They tend towards a pathology, the apparently accepted axiom of which is, that the cause of puerperal convulsions is ever to be found in *cerebral anæmia*; and ending in a precept of practice most legitimately deducible from that pathology, *that depletion in any form, but more particularly blood-letting*, and especially that which is applied with the view of *lessening the amount of blood in the brain, is irrational, unphilosophical and destructive*. As we have above referred to Dr. Prioleau's excellent report, and owe to it much of our apprehension of the modern views on the subject, we, in all fairness, quote his language in regard to venesection, as based upon them: "We desired to speak of the employment of the lancet. Its use will greatly depend upon the views entertained upon the etiology and the pathogeny of the fit; the time should have passed when practitioners adhering to routine and founding treatment upon the restricted field of personal experience, or upon the dicta of ancient masters who were ignorant of the physiological knowledge which we possess, employ venesection without question or discrimination in every case, as some continue to do. A personal experience is never progressive, nor is it likely to be, and is at best but a fallacious criterion of success. Our views should be more expanded. Derived from the collected information of the many recent obstetricians, each of whom is employed in the larger fields of observation and experimental knowledge; our practice should be based upon our knowledge of pathology, and upon the facts and the analogies which speculative and physiological research has contributed towards the elucidation of nervous phenomena. Speaking generally and in the most comprehensive manner—excluding exceptional instances of convulsions, which we all know occasionally occur, and which require a special treatment, an account of which we have been compelled to omit from consideration in this report—we would say, that the anæmia which generally accompanies pregnancy is the predis-

posing cause of eclampsia—the sudden arterial anæmia of the encephalon the immediate cause. Holding such strong opinions, we must be adverse to the use of the lancet.”

It is an observation not unfamiliar to those who note the progress of discovery, that neither in physics, nor in medical science, have the mere experimenters and observers of facts been always the best interpreters of their true significance. Arantius, Cæsalpinus, and Fabricius, had each, by patient labor, discovered that there were valves in the veins, which, by their inclination, marked the direction of the blood-current as plainly as the arrows of the geographer can mark on a map the course of any river. Yet Harvey, who did not discover these valves, but read more clearly their interpretation, perfected from the same facts the grandest discovery our science had known for centuries, entitling him to the appellation of “the Father of Modern Physiology.” May we not find it prudent to accept, without some hesitating circumspection, experimental results which assume to subvert views which have so long governed treatment in this fearful disease; and which have not, to say the least, been contradicted by the teachings of experience? May we not dare to suspect, that possibly some of the interpretations reasoned out by the ingenions and skilful vivisectionists, as the pathologic significance of the phenomena they evolve, may not always answer as the indices to a true pathology, or do to trust implicitly as the infallible guides to a safe and unquestionable practice? It is the warning voice of true Philosophy which tells us that “there is no error more prolific of mischief than that of connecting observed effects with causes which did not produce them.”

Most of the deductions upon which the anæmic pathology of eclampsia has been based, have been drawn from facts arising out of conditions not altogether identical with those in which convulsions take place in pregnancy and during labor. That animals bled to death, should have, sometimes, in their death-struggle, jactitations and contortions very nearly allied, say analogous to, even say identical with, those of the epileptic seizure in the human female; or that the sudden cutting off by ligature, or otherwise, the entire blood, or as much as possible, from any considerable region of the encephalic mass, will give rise, in animals, to convulsions—we perhaps will not just now deny. These experiments are very interesting, and perhaps conclusive, so far

as *these animals* are concerned, and quite suggestive for some of the purposes of reasoning in human physiology; but we know very well that the same effect, nor anything like it, is hardly ever produced in single or even double carotid ligations in man. It has been long acknowledged that the ligation experiments of Sir Astley Cooper on dogs could not legitimately be used, as we find them educed from and expounded in the modern works, to predict results as to human ligations of the carotids; the carotids being comparatively unimportant in the cerebral blood-supply of these animals.¹ Simultaneous double

¹ "Many experiments have been made by Meyer, Jobert and others upon the lower animals, with the view of determining the effect produced on the brain by the ligature of the carotid arteries; but the deductions from these are of no value whatever when applied to the human subject; for the simple reason, which appears to have been strangely overlooked, that in many of the lower animals on which the observations were made, as the dog and rabbit for instance, the common carotid arteries are of secondary importance so far as the cerebral circulation is concerned, being destined principally for the supply of the external parts of the head, the brain deriving its chief supply from the vertebrals; whilst in other animals, as the horse, the brain derives nearly the whole of its blood from the carotids, and but a very small quantity from the vertebrals. Hence, in one case the carotids may be ligatured without danger, whilst in the other their deligation is inevitably fatal."—ERICHCSEN, *The Science and Art of Surgery*, p. 541.

In therapeutics, we may say, such modes of study are still more fallacious. The experiments of Laborde, Guttman, and Eulenburg with bromide of potassium are condemned by Dr. Bill, because they were made on the lower animals. "No such experiments are of any value in therapeutics. Each tribe of animals has its own peculiar therapeutics—morphia acts on cats like strychnine." The present physiology is largely, and we suppose unavoidably, based on experiments made on the lower animals; yet we constantly find such variance of action, which, like those above referred to, deeply impress us with the necessity for caution.

Though, with many others, we have long been familiar with the popular report as to the harmlessness of certain active poisons in particular animals, we desired our friend Dr. A. A. Bell, of Madison, to procure and furnish to us an authentic statement of some striking facts which had come to his knowledge:

"MADISON, GEORGIA, July 19th, 1875.

"DEAR DOCTOR,—Dr. R. R. Harden, of Atlanta, writes that he has himself given to a medium-sized hound dog a *heaping teaspoonful of arsenic*, every other day for three times. It purged him some, but did nothing else but cure the mange which he had. I know it myself to be a popular remedy for mange in the dog. Have never heard of its doing any harm. The horse, sheep, and goat feed on *hemlock*. Farmers feed their young chickens on pulverized *nux vomica*, to kill the hawks.—Very respectfully yours,

"A. A. BELL.

"To Dr. Campbell, Augusta."

ligations are very apt to *kill*, but the patients die in the old-fashioned and naturally-to-be-expected way, with white softening from a rapidly-perishing brain; just as a healthy arm or leg would mortify upon the cutting off of its blood supply. They more often, we believe, die of paralysis than of convulsions. The symptoms arising from this cause, according to Erichsen, are immediate and remote—syncope, trembling, giddiness, impairment of sight and hemiplegia, are the immediate; paralysis, convulsions and death, are the remote results, marking the closing scene, after softening from impaired nutrition has taken place.

But even did we admit, for the purposes of argument, that these operations, had they been performed on man, and that whenever we bled one to death, or whenever we exsanguinated the brain of the human subject to an extreme degree, by any means whatever, epileptic or eclamptic convulsion invariably result—we could not, even then, admit the legitimacy of the comparison; for what we may venture to call the element of quantities—as brought into comparison whenever these observations and experiments are appealed to, with the view of establishing an identical, or even an analogous cause for the convulsions of child-bed—is entirely disproportionate and incongruous. The eclamptic woman can be in no condition at all approaching that of an animal which has been bled to death; for we very well know—whether the practice be right or wrong—that in the midst of this very set of phenomena, quarts of blood are often taken from the arm, certainly without always destroying life; and often with an effect, if injurious—as doubtless it often is—so inconclusive in its obvious results, as to leave on the minds of the majority of the most distinguished and experienced men in the profession, the opinion that the procedure is actually beneficial in arresting the very set of phenomena which these pathologists have recognized, as the proof most irrefragable, that cerebral *anæmia* is the cause and the condition of eclampsia! They do not all die, as we might most reasonably have supposed, but the enormous proportion of sixty-five per cent.¹ escape death, and actually get well!

On the other hand, it is argued against venesection, that ex-

¹ Leishman's System of Midwifery, p. 650.

perimental research upon living animals has shown, that the fullest degree of cerebral turgescence seldom or never gives rise to convulsions. A little reflection—simply recalling well-known principles—would have rendered elaborate vivisection on this point wholly unnecessary. Whatever may be the pathogenic influence exerted by a *moderately* increased activity or turgescence in the cerebral circulation during pregnancy, favoring *increased polarity* and *responsiveness* of the motor ganglia and of the motor tract, as well as of that immense excitor vesicular sheet spread over the convolutions, commonly known as the cortical substance, we will not here attempt to estimate. We are free to say, however, that it is *increased polarity and increased responsiveness to near, or to distant excitor impressions*, that we regard as the condition essential to the occurrence of convulsions during pregnancy and child-bed;—that it is indeed the eclamptic state.

We have regarded it as too long a settled fact in pathology, that cerebral blood-pressure, or any other kind of equable cerebral pressure, could ever have such an effect, to doubt for a moment that such a state of pressure is the very last condition that could give rise to convulsions. Such pressures upon the encephalon, by common consent and common observation, are attended by an exactly opposite condition—that of paralysis; for there are apoplectic states of the brain which are the *ipse morbus* of certain forms of paralysis.¹ Such turgescence, such congestion, blood-fulness, serum-fulness—essentially such pressure upon the brain, and not “exhaustion of nerve-force,” as often suggested, is the one grand essential in arresting the

¹ Long since, the influence of blood-pressure upon the brain had been carefully studied out, and as forcibly demonstrated as logic and language could disclose a physiological process. It was unquestionably proven that pressure of any kind, whether by blood or depressed bone, by pus, by serum, by distended vessels, or by extravasated clot, instead of giving rise to convulsions, invariably produce the opposite—paralysis. And yet vivisections and ligations are being diligently wrought to demonstrate this principle many years proven; viz., that extreme cerebral congestion does not produce convulsions. These repetitions are unprofitable. Is there no tangent at which physiologists may cast themselves out of this charmed circle in which they are performing so tiresome a round, and in which pathologists and practitioners must necessarily follow, as travelers guided by their pioneer? Science may be puzzled and embarrassed by re-opening settled questions in physiology, but it can never be advanced.

convulsion, and in obtunding, as opium does, or as chloroform may for a moment or two—the reflex irritability of the cortical surface, of the motor ganglia, of the motor tract, of the cutaneous surface, of the uterine nerves, and of the sensori-motor apparatus universally; which reflex irritability, from whatever source it may have taken its origin, whether from an irritative hyperæmic condition of the brain itself, or of the sensory or motor ganglia at its base, or from too much urea in the blood, we here repeat, is the one ultimate and essential cause of most puerperal convulsions. A moderate hyperæmia of the eye renders the retina exquisitely intolerant of light, convulsing the iris as well as the associate *motores oculorum*. Advance this to actual apoplectic congestion, the retina becomes irresponsive, and the reflex motor phenomena entirely disappear. It is well-known that active leeching, or opiates, would, in most cases, promptly have relieved this irritation. The parallel is obvious.

Of course we are fully aware that it will quickly be asked: If you regard “determination of the blood to the head” (and we have no objection to the honest old phrase) so essential, in a certain way, to the arrest of the convulsions, why is the drift of your argument such, that we recognize in you an advocate of the lancet? We answer that we are not so much at present the advocate of the lancet, as that we are asking to be recognized as the defender of the use that has been made of it in the past. We do not claim for it that indispensable value which, not very long since, we insist, was most justly accorded to it. We do not regard it *now* as the “sheet anchor”¹ in the treatment of eclampsia; but at the same time we do not wish, that its non-use and abandonment, as a sole reliance, by ourselves and by others, shall be regarded as the substantiation of doctrines in physiology and in pathology, which we consider as by no means legitimate; and which are gaining triumphant though undeserved practical vindication, almost solely, by the entirely groundless and unwarrantable interpretation of blood-letting in eclampsia, as it has been extensively practised in the past, and by, here and there, a few in the present.

We wish to break, in some degree, the force of that spurious argument which makes it a crime of ignorance ever to use it;

¹ Gooch on “Diseases of Women.”

and, if possible, to call attention to some of the fallacies which attach both to the observation of some of the experimental facts, as well as most especially to the momentous deductions evolved from them in regard to a remedy which, for centuries, under the most careful—we may say, the most jealous observation of the best medical philosophers, continued to be recognized as the accepted and most reliable resource then possessed for the control of eclampsia.

Blood-letting has ever been so decided and striking a measure, that when, in the progress and advance of therapeutics, other remedies of less exhausting influence, but possessed of sedative powers capable of controlling convulsions, began to come into use; each one of these was set up, not as an adjunct to, or as an improvement on, the good and well-tested reliance of so many years—not as a some-time substitute, not as an improvement simply—that is, a *better* method in the place of one acknowledged to have been *good*—but these remedies were brought forward as rivals and antagonists; uprooters of that which had so long been the recipient of confidence and favor. A new *modus operandi* was claimed for the new remedies which was entirely adverse to that of venesection, and which, if established, would prove that the practice of blood-letting was then, and had ever been, entirely ignorant and injurious. Venesection, at one time almost our only remedy, had been used so long and so freely and so generally, that, with all the praiseworthy attributes it had gathered, it had accumulated also, many sins. Among the thousands its judicious and proper application had saved from death, there were doubtless also many, who, by its improper employment, had been killed. The “new issue” of physiologists, pathologists and practitioners, began to condemn it with a wholesale condemnation. So far from seeking its many merits to disclose, they with ruthless and unappeasable rigor, drew all its frailties from their dread abode. The practice is now not only condemned as being inferior to other sedative methods of quieting convulsive action—as we freely admit it often is—but it is even insisted that it is not a sedative at all. It is not only argued that it is unnecessary to use it, now that we have equally good and less exhausting remedies, but that it never had been right to use it, no matter how destitute we may have been of means by which to quiet convulsions;—not only that its use is

never now right, but that it had always been totally and entirely wrong. And more than this ; with the refrain of an old chime constantly repeating itself in their minds,—*atonia gignit spasmos*—they enter what is thought to be the decisive field of vivisections and of mechanical and electrical irritations ;—they overturn and overturn, until they have established in some sort, a physiological basis for their condemnation of the lancet, which is so logical and so impregnable, that those who, at first, thought to question it, at once succumbed.

We now look with horror upon the ignorance of our forefathers ; and in shuddering wonder, we thank Heaven for the sixty five miracles which must have been enacted wherever any hundred pregnant or lying-in women had been subjected to blood-letting in eclampsia !

But their vivisections and the generalizations from them prove too much for common observation and for common reason to accept :—they lead unavoidably to conclusions, which, if extended, may presume a most frightful condition of anæmia, some forms of which may, even of themselves, “suddenly extinguish life.” In this state of extreme exsanguination, could the brain, from any influence whatever, bear the farther diminution of the amount of blood upon this uttermost minimum to which it is already reduced ? We are compelled to agree, if we accept their rationale, with the very extremest of those who radicate the convulsion in anæmia, that most assuredly it could not. Upon such strong deductions—all based upon physiological experiments—has the impregnable doctrine of the anæmia pathology been founded, and by these deductions has it practically gained an almost universal support at the present time.

But, as we have said, these experiments and generalizations in proving such extreme degrees of cerebral anæmia as the common condition of the brain in eclampsia, have proved too much ; and we might add it has been promulgated an entire age too soon. There are too many practitioners yet living who have witnessed the escape of too many women (said now to have been the subjects of such anæmic brains) after profuse and repeated bleedings. The sweeping condemnation of the once time-honored measure, while it saps the logical integrity of the reasoning of many we honor and revere among the dead ; also, most unfortunately for itself, aims a mortal blow

at the justness of the life-long convictions, and labors founded on them, of some few who are yet living. Some few who can remember, though perhaps they cannot reason;—who can count, though they may not be able to philosophize; and who can compare even though they would not presume to generalize. There are men now living, who still remember their own successes in that most destitute past, when only the lancet and a few minor adjuncts constituted their entire fund of resources even in the most frightful cases of eclampsia. With that “meekness of wisdom,” so characteristic of an enlightened advance in professional life, they hailed with cordial pleasure the achievements of later investigators. They have modified their method—they accepted improvements, bleeding less and trusting more, year by year, to anæsthetics; till, while they remember with gratitude the many mercies often accorded them out of the very depths of despair, through the direct and unquestionable instrumentality of the lancet, they have yet accepted the new and less exhausting remedies. But they have adopted them strictly as substitutes—only as less exhausting substitutes—for that which once had served them well, and which now, perhaps, they lay most tenderly and reverentially away. These venerable men—and some of them are not yet even gray—as we have said, can count, and they do count the number of escapes that were made by patients, who have been treated on the once successful, but now condemned and reprobated plan. They candidly acknowledge, too, that there is a difference in favor of the substitutes. They study the statistics which were expected to stultify themselves, and they magnanimously regret to find that the difference is so little. They examine the pathology which has guided to the principles of the new practice, and they are filled with wonder when they realize how little the noon has gained upon the dawn of science!

These men are by no means amenable to the charge of unprogressiveness in science. Though out of the sphere of the latter-day progress in vivisections and in experimental physiology, they yet look with hope and pleasure to this as the source whence is to come the confirmation of many of their own results of observation, as well as for the clearing of obscurities they could not in their own day penetrate. But when, instead of this, they find these experiments are to become the pro-

crustean bed upon which are to be laid established truths as well as possible errors;—when long demonstrated facts, as the functions of the brain-centres, are to be denied and uprooted, because they do not quadrate with modern interpretation of modern experimentation;—when absurdity appears in the main induction of this modern reasoning—they may, for a time, be forced to silence; but there they will still remain; and while admitting the converse, per force, they rise stronger than ever, with *epur si muove* as their final and indignant protest.

For vivisections, for all experimental demonstrations of physiological and pathological truth, we have the profoundest and most ineradicable reverence; but when the deductions evolved out of them lead to absurdity, we take refuge with the more reliable and far safer guides which are ever to be found in the teachings of experience and in the intuitions of common sense.

The idea of cerebral and centric anæmia seems every day to be extending its applications, and sometimes too, in accounting most strangely for very opposite kinds of phenomena of disturbed and morbid action. Not only are the jactitations initiating syncope and the convulsive struggles attending death in the shambles appealed to, in order to confirm the doctrine that all convulsions depend upon centric exsanguination; but now we find so high an authority as Niemeyer contending, that even the paralysis resulting from cerebral hemorrhage (apoplexy) is not due directly to the pressure of the clot, but that “it is attributable to the sudden compression of the capillaries producing anæmia of the brain substance.” In answer to this, we might adduce the fact that, unless he explains paralysis from depressed bone in the same way, his rationale will not hold good. Again, Professor Bauduy, of Missouri, remarks that “some maintain that reflex paralysis is induced by an anæmia of the blood-vessels in that particular portion of the cord where the irritation is reflected; hence, they call it a reflex spinal ischæmia.”¹

Thus we see how various are the phenomena attributed by the more modern writers to the instrumentality of the brain and its basal ganglia, when they may have been from any cause put in

¹ Lectures on Diseases of the Nervous System.—Philadelphia, 1876. p. 334.

a condition of lessened vascularity. Some attribute convulsions to this condition ; while others again, as just quoted, explain the diametrically opposite phenomena of paralysis to this same condition of anæmia. Animals that have undergone carotid ligation, animals that have been bled to death—persons in syncope, persons suffering from cerebral hemorrhage—all these various subjects are assumed, and probably correctly, to have anæmic brains and anæmic basal ganglia. According to some, these exsanguinated centres *convulse* the muscles ; according to others they *paralyze* them.

While we would not wish to raise any question which might unfairly disconcert the varied applications of this very accommodating theory of anæmic nerve-centres, there does occur to us one other familiar set of well-understood convulsive phenomena, in which it would surely seem that neither the anæmic brain nor the anæmic basal ganglia can exercise the least energetic instrumentality in the production of convulsions :—We refer to the very marked muscular jactitations following upon *complete decapitation*. We know full well, as every one else does, how Dr. Marshall Hall would most consistently interpret these contortions according to the principles of his “true spinal system ;” but should not their occurrence—the head being entirely severed—lessen our confidence in the so-called cerebral anæmia, relied upon as the cause of puerperal convulsions ?

ANIMALS BLED TO DEATH DO NOT DIE IN CONVULSIONS.

Having doubted for some time the legitimacy as well as the cogency of the arguments, so long and so often attributed to analogies drawn from the convulsions of slaughtered animals, we, during the preparation of this paper, began to cast about for some striking and convenient experiment which might test the value of the very strong assertions based upon what appeared to us rather imperfect, if not unintentionally perverted, observations. The most common and familiar example of such convulsions is that of the killing of the ordinary domestic fowl. In this case, however, the mind being impressed with the common idea that death by hemorrhagic exsanguination is always necessarily convulsive death, it was difficult to decide whether to the severing of the arteries, or to the simultaneous

severing of the cervical cord, was to be attributed most of the very marked contortions observed in such killing; for, in the majority of such instances, complete decapitation is the method adopted. There was reason, however, to believe that *bleeding* was *not* the cause of these same contortions, which, as we had often observed, are invariably produced by simply crushing or wringing the neck, without separating the head at all, or producing any effusion.

With this view, we determined to modify the mode of producing death, though we here distinctly reiterate our opinion as to the *non-conclusiveness* of such experiments on the lower animals, as illustrative of the effect of morbid cerebral conditions in the human female during the eclamptic seizure.

In our first experiment the subject was a full-grown and vigorous young fowl. We severed at one incision all the vessels, nerves and other tissues of the neck in front of the vertebral column, leaving uncut only the trachea. The blood was rapidly poured out. The fowl became weak and lay quietly upon its side, making no convulsive movement, and manifesting no disposition thereto until touched, when the actions were evidently of a voluntary character and produced by fear. Not being touched or approached again, there were no movements for a considerable time. Gasping soon succeeded; then an opisthotonic condition of the lower limbs and body; and lastly, a quivering and tremulous movement of the wings and wing feathers, not fluttering as in the succeeding case. The bird now on being taken up was found to be dead.

The subject of our second experiment was also a vigorous full-grown fowl. The feathers being removed from the back of the neck, we severed, with a delicate bistoury, the spinal cord about the middle of the cervical region, passing the knife carefully between the vertebræ. But a few drops of blood were lost. The most violent convulsive actions, jumping from the ground, fluttering with the wings, and indeed all those spasmodic movements familiar to every one who has witnessed complete decapitation, were the instantaneous result upon the cutting of the cord. These lasted some fifteen or twenty minutes, becoming somewhat less marked. Decapitation was now completed. After this, the convulsions continued a considerable time and then ceased. These spasmodic actions certainly

did not increase on the pouring out of the blood by the severing of the cervical vessels.

The difference in the effect of decapitation on various animals, if systematically observed, would be most striking. The widest difference, perhaps, would be found to exist between birds and reptiles : while the muscular contortions of decapitated birds are of the most violent and irregular character comprehending energetic movements over a considerable space the moment that section has nullified the inhibitory or co-ordinating influence of the encephalon ; the movements of decapitated reptiles, as the saurians and chelonians, are entirely different, being restricted as to their scope, and of a character very little differing, in precision and co-ordination, from true voluntary muscular acts. This we exemplified by the decapitation of two fresh-water tortoises shortly after the experiments above described ; in these there were no movements that partook at all of the character of convulsions. The fore-feet were pretty constantly drawn beneath the carapace, but the hind-limbs frequently executed movements outward and backward, sometimes touching the ground, lifting up the body, and in every respect closely resembling the motions exercised in walking. These seemed entirely independent of any response to reflex irritations caused by the touching or the pricking of the foot.

Dr. Bennet Dowler, of New Orleans, was engaged many years ago in some interesting studies on the nervous system of saurian reptiles, which are still familiar to many. The subject of his original and ingenious experiments was the great amphibian of the south-west ; and his object was, the investigation of "the seat of volition in the alligator." His results were of the most striking character, and demonstrated, that after removal of the cerebral masses, co-ordination under reflex irritations was so complete, and muscular movements so well concerted and apparently intentional, as to leave the question unsettled whether or not volition in these reptiles was an attribute of any of the constituents of the encephalon, or of some other portion of the nervous system. Such wide variance in the effects produced in the different orders of animals by removals and separations of the cerebral ganglia from their connection with the spinal centres, the true seat of convulsive action, should cer-

tainly lessen still more our confidence in all inductions relating to the human subject drawn from such analogies.

In conclusion, we would say, we have been much surprised at the importance sometimes attached by certain physiologists to the struggles of animals dying in the slaughter-house, in their attempts to elucidate delicate questions of pathology; for it must be remembered, that in most of these modes of death the cause producing it is often of a composite character. The animal is first "knocked on the head" to disable it, and then "stuck" to exsanguinate or "whiten the flesh." That under these circumstances there should be convulsions, is not at all surprising; but that careful physiologists and pathologists should attribute them in such a case to one only of the combined causes attending the death, and upon this assumption found principles for the decisions of practice in circumstances of dire and consummate extremity, must appear to any one as at least in some degree a failure in circumspection. If such slaughtered animals really die in convulsions, who can legitimately decide whether the crushing blow upon the head or emptying the vascular system of its blood, is the true and only cause producing them? We have seen that the domestic fowl at least can bleed to death, and yet no convulsive struggle characterize the cessation of life.

PUERPERAL WOMEN BLEEDING TO DEATH DO NOT DIE IN CONVULSIONS.

But let us confine our thoughts to the very subjects themselves—to the pregnant women impressed with all the conditions that coexist with eclampsia.—How is it with them when they suffer extreme exsanguination? Do *they* die in convulsions when this frightful casualty befalls them—when they bleed to death? The answer from every experienced practitioner would be a most decided *negative*. Fortunately the calamitous occurrence of actual death from uterine hemorrhage, whether ante- or post-partum, is an extremely rare one, since the modern hemostatic resources are so fully and so readily at hand;¹

¹ "Perchloride of Iron, in Hemorrhage after Labor." By Robert Barnes, M.D., &c., London. *Vide* "Obstetric Operations," Second American Edition; Chap. xxviii., pp. 449-458.

Although not directly connected with the present subject, we take occa-

yet nearly every one has witnessed the extreme exhaustion attending placenta prævia and other distressing conditions; and yet in the report of none of these cases do we find that eclampsia ever comes to close the scene. *Convulsions* are never thought of, even though "anæmia and the exsanguinated basal ganglia" must unavoidably occur to all philosophic minds who contemplate such conditions. Let us here close pertinently this much of our subject by a note from the *Transactions of the New York Obstetrical Society*: "Dr. Jacobi agreed with Dr. Thomas, that it is better to defer operating when the patient has become debilitated from repeated hemorrhages in placenta prævia, than to run the risk of the shock which an operation might produce. As to the mode of death, does the patient die from shock, or from anæmia of the already exsanguinated brain and medulla oblongata, unavoidably following the rapid evacuation of the uterine cavity by version and extraction? He is confident that the latter is the true cause. To this Dr. Thomas assented."¹ Can anything be found more pertinent to our subject—can anything more clearly *contradict* the anæmic theory of puerperal convulsions?

Thus have we shown, as it were, by gradual approach, the convicted fallacy of some of the long-admitted arguments upon which it has been assumed that puerperal eclampsia is the

sion to refer with much commendation to the paper of Dr. James Trask, of Astoria, N. Y., on "Injections of Tincture of Iodine into the Cavity of the Uterus in Hemorrhage after Delivery."—*American Journal of Obstetrics*, Feb. 1875, p. 613.

That the use of solutions of perchloride of iron, as suggested by Dr. Barnes, of London, has been instrumental in saving many lives, is most unquestionable. Its occasional failure and still less frequent disastrous results have not deterred the profession from using it, as a *last resort*, in which case only it seems to have been the intention of Dr. Barnes to recommend it. We have now, however, in the reported experience and researches of Dr. Trask, a most efficient and innocuous styptic which, while it surely excites contraction of the womb and uterine blood-vessels, thus promptly arresting the hemorrhage, yet never excites inflammation nor leaves any coagula, to produce irritation or by absorption, to lay the foundation of septicæmia. His formula is: "Tincture of Iodine, $\frac{5}{8}$ ss, water $\frac{3}{4}$ i. Inject into the womb." It is antiseptic as well as styptic. Dr. Trask's paper presents a valuable and scrupulously fair *summary* of the discussions relating to styptic methods, in post-partum hemorrhage.

¹ *Transactions of the New York Obstetrical Society*.—Reported by Paul F. Mundé, M.D., Secretary; *Am. Jour. Obst.*, Feb. 1876, p. 650.

result of cerebral anæmia :—First, that animals bled to death do not die of convulsions unless the cervical cord is severed or injured, or some injury done to other nerve centres ; and secondly, that pregnant women, in bleeding to death, *do not have convulsions*, even when “the exsanguinated brain and medulla oblongata” are distinctly recognised “as the cause of death.” This is an observation upon which we could not well improve—an argument made so fitly to our hands that we must not weaken it by a single word.

MODERN PATHOLOGY AND THE THERAPEUTICS OF ECLAMPSIA.

When the assumed condition of the brain in eclampsia—that of extreme anæmia—is considered ; that sixty-five out of every hundred subjected to venesection should escape ; it ought to be regarded far more wonderful—according to the reasoning of this pathology—than that eighty-nine should be cured by any other method of treatment. Blood was taken out of the body and out of the brain—in some of them largely taken—in order to cure the convulsion, the practitioner being ignorant of the fact that these very convulsions had their origin in an already deficient quantity of blood in the brain. The treatment had been predicated upon plethora of the cerebral blood-vessels, when in reality exactly the opposite condition existed—anæmia !

When to the experienced practitioner we say, there are some few morbid conditions for which we prescribe, in which all the elements of diagnosis, so to speak, can scarcely be said to have been supplied until the results of treatment have demonstrated the correctness of the assumed pathology, he will fully comprehend the remark. Let us see if the anæmic pathology of eclampsia, so confidently assumed, and based upon vivisections and the deductions of vivisectionists,—who, as we have humbly ventured to suggest, may not always reason so well as they dissect—let us see if this extreme exsanguination of brain is substantiated in the way referred to, by the results of treatment. Does the effect produced by remedies prove, or does it disprove, the existence of that condition of brain, which, to these pathologists, is the *ipse morbus* of eclampsia—the convulsions being but its most prominent manifestation ? In regard to the effect of venesection—the remedy of the past—it has been seen how

from the very first moment that we began to consider the subject of cerebral anæmia, down to the last word we could say about it, we but struggle upon the horns of a most perplexing dilemma. If the eclampsia of the past was extreme cerebral anæmia, then not one in a thousand who were bled should have escaped death ; and yet statistics prove, that sixty-five in every hundred got well under bleeding ! And with an equal confidence do we state, almost in the same words, the parallel proposition, that if the eclampsia of the present is extreme cerebral anæmia, not one in thousand, who are now subjected to chloroform, chloral and the bromides should be expected to escape ; and yet the statistics, admitted on both sides, prove that eighty-nine in every hundred do get well under anæsthetics.

The double dilemma to which we have thus we think very fairly been brought, by subjecting the anæmic pathology to examination, under the lens of a therapeutic as well as of a pathologic illumination, is truly disconcerting ; for in this awkwardness of discussion, we have fallen into the danger of unsettling our claim to that exact knowledge of the nature of the disease and of the rationale of treatment, of which we had supposed we were in such quiet possession. We do not see, in the turn which the discourse has taken, how we can escape from the entanglement into which we have so heedlessly fallen, except by a sacrifice perhaps of the gravest kind : Our pathology or our therapeutics must, one or the other, inevitably succumb. Had all the eclamptic women in the past died from bleeding whenever it was employed, or had all the eclamptic women in the present died from the use of those powerful depletants of the cerebral circulatory system—the bromides and chloroform—how triumphantly would our experimentally established physiology and our rationally deduced pathology have been vindicated. Or if the remedies which cured them had been brandy and strychnine and electricity, we could still claim the infallibility which we have asserted—but chloroform, the bromides, cerebral exsanguinators ! How can *they* arrest convulsions eventuating from an “ exsanguinated brain and medulla oblongata ? ” Verily, has Hahnemann at last been vindicated ?—*Similia similibus curantur !*

We see no possible way out of the prison-house now closing around us. Perhaps we may impugn the theory which alleges

the therapeutic action of the bromides to be that of cerebral depletants. The inevitable words are, "they lessen the amount of blood in the brain,"¹ and in this unquestioning faith, we rely upon them to cure grave diseases for which, in a former age, we medical men have been known to tie both of the common carotids; and they sometimes cure these diseases, when tying the carotids used often to kill. It is our principal remedy for epilepsy. We would fail at present in any attempt of this kind; the bromides hold the title to this alleged attribute from authorities fully as high as any that have asserted "that which is the essence of anæmic convulsions." And then there is chloroform, one of the very chiefest of all the anti-convulsives; one which, according to such unassailable authorities as Dr. Nélaton of Paris, and Dr. Sims of New York, kills, whenever it does kill, by producing cerebral exsanguination to such a degree and so quickly, that "the anæmia at once destroys life"—this assertion being further proved, in that by reversing the blood-statics, the blood by gravity is "determined again to the head," and the patient is recovered. We cannot revise the entire pharmacopœia even to save our doctrine. Indeed, too many things cure eclampsia. Quinine is said to act favorably, and that too is "a contractor of the middle coat of the blood-vessels,"² and a repeller of the blood from out of the brain. Even opium, in some of its forms, is thought by some to do good; but that also "contracts the arteries" and lessens the amount of blood in the brain, as well as in some other portions of the system.³ But after all—even

¹ Though we do not question for a moment, the truth of this rationale of the action of the Bromides and of several of the other anæsthetics above mentioned, we also recognize as correct the views in regard to their action expressed by Dr. Bill, (*Amer. Jour. Med. Sci. July, 1868*), viz.: that "*they are anæsthetics to the nerves of the mucous membranes.*" We have no doubt whatever, that much of their wonderful potency in arresting and preventing convulsions resides in this last attribute. Where the irritation is peripheral and the convulsions reflex, as in infantile convulsions or eclampsia from uterine irritation, the excito-motory action is arrested in its inception. We use the bromides largely in cholera-infantum to obtund the morbid sensibility of the mucous surfaces, which gives rise to the excito-secretory or so called "vaso-motor" action upon which, during dentition especially, this often unmanageable affection most frequently depends.

² The Rationale of the Action of Quinine, by Dr. Robert Campbell, then Demonstrator of Anatomy in Med. Coll., Ga., Augusta, will be found fully stated in *Southern Med. & Surg. Jour.*; Vol. xv. 1859, p. 569.

³ Treatment of Ulcers; by Dr. F. C. Skey, of London.

succeeding in correcting the false views in regard to the *modus operandi* of these remedies—what can we do with venesection itself? Even that did not kill as it ought to have killed. What can we do with sixty-five in a hundred who would persist in getting well notwithstanding the anæmic source whence the convulsion came, and most perversely, in spite of *tonia gignit spasmum*!—Not even for bleeding do they die near fast enough to vindicate the claims of science. The dilemma, it is said, ever forces a conclusion. Let us see—though not after the strictest method, perhaps:—notwithstanding the fact that we have not disproved anæmia of the brain and its gangliæ during the time the eclamptic convulsions are in progress, it would plainly be highly unphilosophical to conclude that such existing anæmia could possibly act as the proximate cause of the convulsions, when the very remedies admitted on all hands to be the most effective controllers of such convulsions are also the very agents which are, by all our experience, pre-eminently known to be such, as in their action produce and increase anæmia of the brain. We may conclude, then, that while anæmia to a considerable extent may sometimes exist at the time of the convulsions; and, if so, be increased by the use of the remedies which are applied to relieve them; still, inasmuch as such agents do relieve the convulsions, even though increasing the anæmia, some other cause than the anæmia must have been present to produce them; and consequently it must have been *this other cause* that was successfully combatted, and not the anæmia, when the convulsions were relieved.

In a former period in the history of medical reasoning, not very far back, it was attempted to found pathology upon the basis of physiology, and therapeutics, most strictly, upon the basis of the pathology thus deduced. Who can find fault with so orthodox a construction of the organic law of medicine? The formula was admired, approved and pretty widely accepted; but when examined, strange as it may appear, this commendable and what would at first sight appear, this only legitimate project of construction, had virtually to be abandoned. The first link was defective, and every portion of the chain gave way. Physiology, the foundation upon which the entire fabric was to have been reared was found to be imperfect and unreliable—not physiological *truth*, but physiological *deduc-*

tions and doctrines—hence the failure. Of course this interdependence is still the directing and controlling influence in all true progress, but only by slow advances and in restricted lines. There are many wide gaps and dim paths and dark places, through which pathological reasoning cannot guide us. Here personal experience, observation and phenomenal light—empiricism—must govern and direct us. Notwithstanding all that Marshall Hall, and Claude Bernard, and Brown-Séquard; and Hitzig and Ferrier, and Shroeder van der Kolk; and Radcliffe, Kussmanl and Tenner; and Dalton, Flint and Nothnagel; Fritsch and Jackson and Fournier, and many others, may have accomplished in regard to the nervous phenomena concerned in, or that can be applied to, eclampsia—notwithstanding what Hamilton, and Demaret, and Lyman, and Bright and Simpson, and Lever and Regnault; and Dubois and Cazeaux and Blot; and Litzman and Braun, and Segbert and Corel, and Ranvier and Barker may have accomplished in the toxæmic relations of the subject; and notwithstanding, still, the valuable labors and diligent researches and generalizations, such as we are now discussing,—notwithstanding all that has been done directly or incidentally to illumine the darkness and to dispel the uncertainty, still we dare not even now, deduce the pathology of eclampsia from the physiology which, from vivisections and other experimental methods of research, has been most recently promulgated in this and other countries. And on the other hand, even still less could we *begin to dare* to adjust our therapeutics to the teachings of that pathology, or to the rationale evolved out of the deductions of experimental physiology during the last decade of our own era.

We cannot then regard the pathology of eclampsia to have been very materially advanced, either in definiteness or in extent, by any of the recent experimental researches or the deductions from them, that we have thus hastily though we hope fairly examined.

In regard to methods of treatment for eclampsia, practice fortunately has not always been governed by the suggestions of the prevailing physiological, or again, even of the pathological views of the disease. For years, the doctrines of uræmia, and also of anæmia, have been predominant; and yet, as we have seen, whether forgetting the pathology they professed to hold,

or forgetting the *modus operandi* of the medicines they actually applied, the very men who would advocate the anæmic pathology, and—as an obvious corollary, abhor bleeding—seem all this time, with common-sense empiricism, to have been giving chloroform, the bromides, etc., in the very face of that obvious logical necessity, fore-dooming the patient on the still further lessening of the blood in the brain. Can we venture the opinion, in explanation of this incongruity, that the practitioners were not physiologists any more than were the physiologists, especially in such cases, ever practitioners; or did they know the wrong and yet the right pursue? In any event, how fortunate was it for the imperilled patient!

But it was not every one that pursued his practice in eclampsia on a method so loose of purpose as that he would violate pathological precepts and guides, albeit in doing so he cured the patient, without recognizing the breach. Even long since the prevalence of the doctrines recognizing anæmia, and also uræmia, as the pathological basis of the assemblage of frightful nervous phenomena characterizing the eclamptic condition, every now and then do we observe certain thoroughly philosophic, but independent practitioners, who, while they find no ground to deny the truth of these doctrines, still could not shut their eyes to the fact, that their own experiential knowledge and observation, as well as the statistics of practice, at least vindicated even venesection from that charge of danger and of fatality so often brought against it. Prof. B. W. Richardson, of London, is one of this class. Driven, like the rest of the profession, from the position that plethora, or any other demonstrable circulatory aberration, could be recognized—since the views of Karl Braun, at that time everywhere accepted—in the etiology of eclampsia, he seemed disposed to admit the presence of an irritant in the blood, as the cause inciting to the convulsions. He did not at the time deny that urea might be that irritant. Notwithstanding all this, Dr. Richardson too clearly understood, both from his own observation and from the history of venesection in eclampsia, that it was by no means to be relinquished. Its benefits could not be denied; so he set patiently to work philosophically to explain them. Hence we have the reconciling explanation peculiar to himself, and doubtless in many cases a fair expression of the truth, that if

he must relinquish plethora as the cause of convulsions, and no longer bleed for that, as uræmia is now the cause, *he still would bleed* in order to secure a rapid elimination of poisonous blood, uric blood, from the system.¹ This was indeed very courteously letting theory have some of the benefits and credit of practice, without in the least stultifying either his observation or the experiential facts of history.

Prof. S. D. Gross,² in most decided, if not irate terms, reviews and condemns the crusade widely entered upon against bloodletting. We regard his impatience as by no means unrighteous,—but we quote first some of his sage precepts, and then his running comments in deploring its abandonment. “In puerperal convulsions, attended with a plethoric condition of the system,” he recommends “copious venesection, promptly followed by the administration of a full anodyne, either alone or in union with chloral and bromide of ammonium, and the application of leeches to the temples and cold to the head.” “The plethoric condition of the system, so frequently met with in young, robust, pregnant women, is generally promptly relieved by the abstraction of twelve or fifteen ounces of blood; and certainly there is no more rational remedy in such circumstances, especially when redundancy of blood is accompanied by dizziness, vertigo, or headache. Thirty years ago there were but few women who were not bled once or twice during utero-gestation, on account of these symptoms, and I do not know that I ever heard of one that was injured by the practice.” “Certain forms of hysteria and epileptic convulsions, dependent upon congestion of the nervous centres,

¹ The removal of a mass of “poisonous blood out of the system” is not, we conceive, the true explanation of the benefit of bleeding in uræmic puerperal convulsions. It does far more than this; it relieves the engorgement of the kidney and rapidly restores its secretory function, and thus the entire system is purified by its natural depurator. Whatever may be our ideas as to cerebral plethora or cerebral anæmia, bleeding for renal plethora is a necessity unavoidable and imperative; for no other diuretic can act, till the kidney is relieved by bleeding of its paralyzing engorgement. The older practitioners bled women to relieve them of the dropsy of pregnancy. They knew nothing about uræmia, but none the less did they clear it from the blood when they, by venesection, started the kidneys to drain the redundant water from the tissues.

² Discourse on Blood-letting considered as a Therapeutic Agent. *Transactions Amer. Med. Association*. Vol. xxvi, p. 421. 1875.

and a redundancy of blood in the system, are generally materially benefited by venesection. The relief in the former of these affections is often prompt and permanent, as I can testify from personal experience." "If I wished to be satirical," continues Prof. Gross, "I should say that there are in our profession, as there are, indeed, in every other, two distinct classes of men, the thinking and the non-thinking. The former, whose number is exceedingly limited, accept every novelty, or great and sudden change, with suspicion, wisely concluding that the one ought not to be adopted until it has been fairly tested by well-conducted observation and experiment, and that the other should not be rejected without sufficient cause. The non-thinking man, on the contrary, eagerly lays hold of every novelty, and seldom stops to seek a reason for his new faith. He adopts it simply because his neighbor adopts it. Especially is this the case when the novelty, whatever it may be, has a distinguished parentage, as when it has received the sanction of a great name, or, perchance, if it had a transatlantic origin—Jones, Robinson, or Brown, in Europe, is always a greater man, indeed, far greater, than his namesake on this side of the water. The non-thinking man confounds progress with improvement. He does not weigh the pros and cons of a question; he takes a shorter route; sees things in a distorted light; assumes for granted things that he cannot comprehend; and jumps at conclusions. As the sheep follows the wether, so he follows his master, looks through his spectacles, believes in his infallibility, and swears by his authority. The more the assertion borders on the marvelous the more readily does he gulp it, so much easier is it to assume the truth of a proposition or statement, than to prove it by sound, logical argument, and inductive reasoning. I think I am not guilty of exaggeration in what I say. It really seems to me as if we were bereft of our senses. No sooner is a new remedy, a new operation, or a new method of treatment introduced to notice, than it is puffed into gigantic proportions, and invested with virtues as foreign to it as any other folly under heaven."¹

While this revered master and "Nestor of American Surgery" was thus urging the claims of blood-letting, and deplor-

¹ Dr. Gross, *op. cit.*, pp. 436-7.

ing it as a "lost art" on the floor of our national tribunal of the medical sciences, our distinguished colleague and valued friend, the late Dr. C. B. Nottingham, of Macon, was claiming our approval, before the Medical Association of Georgia, by an admirable essay in defence of a reasonable consideration for venesection as a remedy.

To the question, then, What is the essential condition of the general system and of the nerve-centres?—which can be said to constitute the pathologic basis of eclampsia, we have certainly found that neither recent experimental physiology, nor the deductions growing out of them, can give us a satisfactory and consistent answer when viewed in the light of clinical experience and the accepted principles of therapeutics. That congestion of the brain, that ureic poisoning of the blood, or that anæmia of the brain and its ganglia, can any one of them alone, or all of them combined, supply an answer to this momentous question—or that an answer at all can be given as to the profound and ultimate state which underlies the convulsive phenomena, are questions which, we fear, a later time and a more advanced method or investigation alone can answer. But there is left us one resource which, in the meantime, we may adopt, however humiliating the admission it involves;—however it may compromise and involve in doubt many of the deductions of experimental physiology, of experimental chemistry, of blood tests, of urine tests, and of other tests of other fluids and other secretions;—however inconclusive it may acknowledge vivisections and excitations of the brain, the nerve-centres and the nerves, whether they be mechanical, galvanic or toxæmic—can we acknowledge they have been in vain, but false lights that have deceived us? Perhaps not—*most surely not*—these investigations have taught us much; but certainly they have not taught us the pathology of perpetual convulsions.

The humiliating resource left us is that we must retrace our steps, take up again and be contented with the interpretation of phenomena we were a little time ago contented with; and base upon it, with our eyes open and without groping, a pathology which will be consistent in accounting for the symptoms; and one too, which in every particular is confirmed by therapeutic results. To another age let us relinquish the ambitious enterprise of elucidating the cause of the causes that give rise

to puerperal eclampsia. We think we may be said to have both witnessed and realized its failure for the present. Let us drop back into more superficial interpretations, if so they be called, and be content again to generalize, along with the other forms of convulsive diseases, that over-studied, over-discussed, and over-wrought member of a common class, which we call eclampsia. Why not recognize for eclampsia that cause, than which we can recognize no other for any form of convulsions? The simple statement of this would be *irritation*; whether centric, whether peripheric, whether both in consensual relation. Will any one pretend to say that we have advanced so far that we must expunge the term and its significance as the common cause of every form of convulsive action? If so, let him look at the confusion its relinquishment has made in the study of eclampsia, and how such relinquishment has retarded the establishment of fixed views as to its pathology.

The one condition of irritation existing as the proximate cause of the convulsion will be ever recognized by its manifestations. Its recognition as the cause will go farther than any other theory to conciliate conflicting views, in regard to the concomitant states of the system, in regard to the phenomena in eclampsia, as analogons to those of dentition, epilepsy, tetanus and other forms of convulsions—and in regard to the results of every form of treatment which has been found most efficient in controlling the spasmodic action of this and every other kind. Whether there is congestion or anæmia of the brain and motor gangliæ; whether some centric or peripheric irritation; whether the irritant exist in the form of urea or ammonia, or some zymotic poison, sophisticating the blood, and being carried by it to the motor and other centres; none will deny that any of these conditions existing in varying degrees are capable of furnishing the basis of eclampsia in the state of increased polarity, in which we have every reason to believe the brain and motor centres exist during pregnancy and labor. Should we attempt to examine the various causes we have above referred to as the instigators of the *irritation* (which some may say we superficially persist in recognizing as the *general* cause of convulsions,) and each one of which, as is well-known, has been made the characteristic of a distinct pathology—too much space would be occupied in the discussion. We might

say, however, that the selection of the relations of a single one would very nearly cover the entire class. Even such consideration, to be done with justice, would occupy much time. We refer to the relations of the blood and its circulation throughout the system, but more especially in the brain and motor ganglia, as influencing these instrumentalities in the evokement of eclamptic seizures.

There can be no doubt that any rapid or sudden changes in the quantity of blood circulating in the brain and ganglia, whether they be of diminution or increase, (unless to the degree of apoplectic oppression) are liable to give unsteadiness to muscular action, sometimes evoking convulsions.¹ Thus congestive apoplexy, in its inception, is most generally marked by convulsive action; inflammatory conditions of the centres or their membranes have often similar results; while not the less in syncope do we observe convulsive phenomena almost identical with those marking the beginning of apoplectic congestion. Thus, the changes in the volume of the circulation, either way, are constantly observed to supply the condition of *irritation* or *irritability*, which we consider necessary to the evokement of any kind of convulsive action whatever.

In addition to the changes above referred to relating to *quantity*, we know that the blood of the pregnant and the lying-in woman is often changed in *quality* by being filled with uneliminated materials, some of which are well calculated, even without alteration of volume, either way, to irritate the nerve-centres they may circulate in, and stimulate their activity, as strychnine would do—by sophisticating the blood on its way to the ganglia. *Irritation* then, in each one of these cases, however produced (be it the result either of modified quantity or modified quality of the blood supplying the ganglia) would, to say the very least, most probably give rise to convulsive phenomena.

¹ Some one has remarked that the blood, to the brain and nerve-centres, is, as it were, what the pendulum and weight are to the clock. It controls, steadies, and co-ordinates their action. Too heavy a pressure, like too long a pendulum, causes heaviness and slowness of action; while an inadequate supply, or its extreme diminution, like the removal of the weight—disconcerting entirely the action of the machine—renders irregular and uncontrollable all muscular activity throughout the entire system. There are doubtless such convulsions from such a cause; but from what we have seen they must be extremely rare, as accidents of the pregnant state.

The *eclamptic flash* is one of the most remarkable and characteristic indications to pregnant women of the approach of a convulsion. Turgid conditions of either the centric origin of the optic nerve or of its peripheral expansion, the retina, are the pathologic states in which subjective luminosity, whether in the form of sparks or flashes, have been found most often to occur in individuals other than those in the pregnant or puerperal condition. The circulation of the brain and that of the eye is so nearly a common one that the vascular condition of the retina, as determined by the ophthalmoscope, has become the criterion of cerebral vascularity. That luminous phenomena occur most frequently in persons of a full and plethoric habit would strongly favor the view that their constancy in puerperal eclampsia is due to a plethoric rather than to an anæmic condition of the brain.

Our own recent experience furnishes us with a case illustrative, in a remarkable degree, of the *reflex* character which subjective luminosity will sometimes assume. Rev. E. P. R., D.D., a learned and distinguished clergyman, had undergone ophthalmoscopic examination by Drs. H. D. Noyes and E. G. Janeway of New York. From his account, sub-retinal hemorrhage to a slight degree had been diagnosticated. He suffered no pain, the eye had a clear and healthy appearance, and vision, we think, was gradually improving. As his case progressed, however, he was constantly annoyed by luminous phenomena. These were especially troublesome at night after lying down in bed and when in a dark room. They were invariably evoked by *sounds*, whether near or distant, the ticking or striking of the mantle clock, and also the striking of the city clock, perhaps miles away, would be, as long as he was awake, invariably responded to, *each stroke by a decided flash*, in the affected eye!

There can be no doubt that here, the turgid condition of the retina, consequent upon the minute effusion or upon some limited congestion, had exalted its responsive excitability so that by the auditory nerve and by every other avenue of afferent excitation this increased polarity was made to manifest itself in luminous *discharges*. Thus do we interpret this case of simple reflex "eclampsia" restricted to and extending no further than the retina of an injured eye.

In applying the phenomena of this remarkable case to the illustration of the flashes which form a part of that more complex tableau, true puerperal eclampsia, may we not legitimately predicate that the polarity of both the brain and retina of the plethoric pregnant woman is exalted by the over-turgid condition of their common circulation; and that when a convulsion is excited by any external or internal provocative, as blood-poison, or a labor pain, it is initiated first by a discharge from the sensory ganglia, in the form of *flashes*, as the sound of the bell touched off the explosion in the case above described; and then that, in the same manner, came the discharges from the *motor batteries* at the base of the brain, and from the medulla oblongata, in the form of spasmodic muscular jactitations with loss of consciousness, the result of which last discharge we call convulsions? In answer to the above question we can only say that in addition to the force of argument to be drawn from analogy, therapeutics and experience seem both most strongly to justify our view: In the case of a pregnant woman, whose general fulness and cerebral disturbances had caused us much uneasiness, even up to the eighth month, we were told that during the morning "she had been complaining of flashes." Just as we entered the room, she exclaimed, "I am going blind!" She had torturing pains in her head, her face was flushed, and her speech slow, somewhat indistinct and difficult. We administered immediately forty grains of bromide of potassium; and, remaining with her, in less than an hour gave her thirty grains more. A third dose of thirty grains was given in a few hours. Her symptoms began to recede after the first dose, and during the evening they entirely disappeared. Three grains of quinine, alternated every two hours by twenty grains of bromide of potassium to the extent of—quinine, grs. xii., and bromide of potass., grs. lx., were given each day for the next two days. She, shortly after this, experienced the most profuse, and to her, alarming diuresis. Her swollen legs, and hands, and face shrunk in a remarkable manner. She experienced no return of her unpleasant symptoms; continued with 3 grs. of quinine and \mathfrak{D} i. bromide potass. every day till after the time of her labor at full time, which took place nearly a month afterwards. She was happily delivered of a healthy living child, without

having experienced a single eclamptic or any other unpleasant symptom.

Another lady, in her seventh month of gestation, was very plethoric, apparently; but, we believe, was also very anæmic—overfulness of watery blood. She suffered from dysuria, had vesical tenesmus; urine scanty, it was not tested. She was œdematous in face, hands and legs; she complained of “a pain like an iron band” around her head, which came on in the evening. She bore quinine badly from idiosyncrasy; the little we could give did not modify the pain. Bromide of potassium failed to correct any of the symptoms. Being called, under the apprehension that premature labor had begun, she said that “each time she had a pain there was a sensation of light.” Her extreme pallor and her feeble pulse disinclined us to resort to bleeding. “Flashes,” however, had been our signal for the most active treatment for over thirty years. Active treatment most always meant blood-letting, previous to the era of morphine, chloral, and the bromides. We now, however, with reluctance, prepared to bleed in the most careful and limited manner. An 8 oz. glass tumbler was placed in the basin to catch and measure the blood. Not more than a gill of blood had been drawn when the pain ceased and the sensations of light became less frequent. By the time the tumbler was nearly full, she stated that she was “entirely relieved.” She was somewhat faint in walking back to the bed, but was perfectly comfortable for many hours. The excretion of urine was without pain and the flow more abundant; but labor came on in about eight hours, and she was delivered naturally of a still-born premature infant. Neither during nor after labor did she experience any symptoms approaching those of eclampsia.

Twenty years ago, we believe, we would have bled this lady perhaps a week earlier; we also believe that bleeding at that period would have prevented the miscarriage and preserved the life of the fœtus; and we further believe that it died of uræmic intoxication, *which the diuretic effect of a moderate bleeding would have eliminated from the blood of the mother.* As it was, the bleeding was made early enough to stay convulsions in the mother, but not in time to prevent the fatal poisoning of the child. We verily believe we may sometimes know too much for the welfare of our patients! We must make

haste to unlearn some of our modern pathology, or retain it only for ornament.

The above will at the present time be regarded by some as rather a striking case; but in times not very long past, when bleeding was a more common, indeed almost the only remedy, such cases, with the exception of the small amount of bleeding required, were quite familiar to many.

But it may be asked, why in the pregnant condition particularly should we find this irritability, or predicate this reflex responsiveness of the motor ganglia to both internal impressions through the blood, as well as to external impressions through any of the sensory nerves? The cause of this condition we have used a term every day becoming less familiar to designate: "Increased polarity of the nerve-centres," if stated, would we believe be perfectly comprehensible; but, being none the less theoretical on that account, it is inadmissible except as an hypothesis. In the pregnant condition, the demand upon the woman's resources of nutritive supply are greatly increased. That she is, in certain particulars, temporarily endowed with a nervous apparatus correspondingly enhanced in power to answer such demand, the investigations of Dr. Robert Lee, of London, establishing the rationale of the increased growth and muscular power of the uterus, most manswerably demonstrate. Ganglia of the womb, before undiscoverable, became rapidly developed both in size and activity to a degree commensurate with the control and with the activities they are to give to the enormously-developed vascular, muscular and nutrient endowments of the gravid uterus. It is known that there are some other portions of the pregnant woman's animal economy awakened into an analogous exaggeration of temporary growth: the spleen becomes enlarged; the heart generally acts with an increased force; there is general plethora, although the blood may be thin; and ordinarily the adipose and connective tissue are obviously increased. With all this before us—the greatly enhanced nutrition everywhere, with this plainly indicated teleological plan by which increased nutrition and power, as in the womb, are made to rest upon a temporarily organized apparatus of nerve force, why may we not legitimately predicate an analogously increased nutrition and normally increased polarity and activity in the cerebral and basal ganglia of the encephalon, in order

that resources of nerve-force may be engendered in amounts adequate to the sensory, the motor, and the nutrient supply which the general system is, during that time, called upon to yield? Such, in the want of demonstrated proof, we can only say we believe is necessarily the actual and normal condition of augmented nervous apparatus for the production of an amount of nerve-force adequate to the augmented demand. This may be said to render the brain of the pregnant woman, and all the ganglia pertaining to her encephalon, so many highly polarized magazines of potential force, ever liable under certain circumstances to become actual and uncontrollable, whenever it is awakened into action by any of the imminent conditions of quality or quantity of the blood which we have described. Any sudden increase or diminution of blood—even though normal in its constituents—in the structure of these ganglia; the circulation in them of uric or ammoniacal blood; perhaps the coming to them of zymotoxics or any other accidental or occasional irritant; would any of them be fully sufficient—as it is indeed claimed for each in its turn, by etiologists of various special tenets—to touch off the eclamptic explosion, by centric irritation. And no less, as is sufficiently familiar to all, does *reflected* irritation, from any distant organ, set in train the identical role of phenomena,—the womb oftenest of all organs;—hence the expression of the philosophic and observing Dr. Power, “metastatic labor-pains,” which being interpreted by us, means *reflected and distributed uterine irritation*.

Arriving at this point in our examination of the subject and standing in the end upon ground, as it may be said, so little advanced beyond the status of a period now drifted into the past, we contemplate with hesitation, almost with regret, the vast field from which we have receded, and which we earnestly ask practitioners for a while yet to relinquish. As we return, for the sake of more certainty, to the simple and ever consistent doctrine of irritation, and then compare it with the position to which we had progressed in our misdirected study of eclampsia, we can estimate what a long and dubious journey we have made, very nearly in vain, so far as its true pathology has been advanced, or its therapeutics, as it could legitimately be based upon the present pathology. It is like a new country, or unknown land, into which the pioneers, the sappers and the miners, had

pushed forward, in which they had made valuable explorations, had learned much, and increased the knowledge of the world—but yet a territory which permanently could not be held. The older pathology based upon cerebral plethora, the newer one resting upon the undoubted existence of uræmia, and still farther the most recent of all, that of cerebral anæmia—are, each one of them, too incomprehensive to embrace all the facts and phenomena, therapeutic as well as pathologic, of puerperal convulsions.

While then these views and lines of investigation have added immensely to our knowledge of the concomitant conditions of the pregnant woman, in which eclampsia may originate; while, under some strange misconception, new and most valuable remedies have been added to our successful means of treatment during the reign of this doctrine, the very efficiency of whose remedial influence *subverts the doctrine*; while even the death-rate, through the instrumentality of these means, has been greatly reduced; and while the results of these very investigations strongly illustrate and confirm the true pathology; still it has gradually been every year becoming more and more apparent, that no one of these doctrines, nor all of them combined, can ever replace that more consistent, though less recon-dite pathology, claimed in *centric and peripheral irritation*.

In nearing the close of this discussion, we may state that some very high authorities still look no further than this even at the present day, for the pathology of eclampsia. Dr. Robert Barnes, of London, very moderately discusses blood-letting as a measure to be considered in the treatment of puerperal convulsions. Recognizing the inadequacy apparently, of some of the more modern views, he, in the plainest language contents himself with centric and peripheral irritation as the proximate cause of the attack. He advocates “measures for moderating the excess of central irritability as almost always useful. The most available of these,” he says, “is the induction of anæsthesia by chloroform.” Belladonna, the bromides and chloral are given the highest praise; while he proposes, as a still more rapid method of inducing anæsthesia to administer sometimes, in the future, the nitrite of amyl. We are much surprised to see that he then, almost reluctantly, approaches the subject of blood-letting. “We cannot yet,” says he, “discuss the treat-

ment of eclampsia without referring to the practice of bleeding. To advocate anæsthesia is practically to condemn venesection. * * * I am one of those who think there is more of fashion than of wisdom in the almost absolute oblivion of the lancet. But in this particular case, I do not regret the disuse into which it is falling. It is very easy to tell of cases in which bleeding has been followed by recovery, and of other cases in which other treatment has been followed by death. I believe I have seen distinct relief ensue upon moderate abstraction of blood from the arm, or by the application of leeches to the temples. And where there is distinct evidence of plethora with marked engorgement of the vessels of the face, it is judicious, I think, to apply eight or twelve leeches to the temples, but not to the exclusion of anæsthesia. In delicate women with a feeble circulation bleeding in any form should be rigorously condemned. And we must not forget that the process of labor is usually attended by a loss of blood quite as great as is good for the patient.”¹ Such measured admission of the occasional possible benefits of venesection—if indeed we can recognize it as an admission at all—in one of Dr. Barnes’ wide experience and superior judgment, we must admit, surprises and disappoints us. In the American edition of his invaluable work on Obstetric Operations, the American editor, Dr. B. F. Dawson, has added greatly to the value of the work, as we have it, by the introduction of an abstract of the views of Prof. Fordyce Barker from a former number of this journal;² and blood-letting is here forcibly and most judiciously advocated. After speaking with great discrimination of its value in general and cerebral congestion, in uterine and renal turgescence, Dr. Barker uses the following language, which to many must call up reminiscences of satisfactory results in their own experience. “It has seemed to me there is some liability to err in the neglect of blood-letting from the feeling that this measure should never be resorted to unless the patient is in a sthenic condition. But some of the most striking instances of its usefulness have occurred under my observation when the patient was extremely anæmic.”

¹ Abstract from Lumleian Lectures in *Braithwaite's Retrospect*. Part lxviii. January 1874; p. 222.

² *American Journal of Obstetrics*, etc. Vol. iii. No. 3. 1871.

Previous to the discovery of those powerful cerebral exsanguinators now known to us in anæsthetics, and to their application in eclampsia, we were in the habit of advising, in clinical lectures, that, "though some few cases of eclampsia may even be materially injured by bleeding, yet this minority is so small a one as compared with the vast majority who would be greatly benefited by it, I would therefore advise you, in default of any experience of your own, to be guided by that of others, and regard *every case of puerperal convulsions in an ordinarily plethoric woman as a case for some blood-letting*. Take from 8 ozs. to 16 ozs. from every one such as above described. If, on observing the pulse and other symptoms you find benefit rather than injury, take more, and even to 30 ozs., should the convulsion continue." We cannot say that even now we are prepared to modify very materially the above precept. Empirical as it may appear to some, it certainly seems to us much safer than the equally empirical rule sought to be enforced by a large number of modern teachers, that of *not bleeding at all*.

Prof. Joseph A. Eve, of this city, after an experience of nearly fifty years in the active practice and conscientious teaching of obstetrics, we are gratified to add, is in full accord with our own general precepts relating to blood-letting. This much honored and distinguished gentleman, to whose transcendent worth, both as a physician and a man, we have ventured to dedicate this unworthy contribution, though always accurately and profoundly familiar with every phase of both the pathology and therapeutics in this subject, and though remarkable for his deferential consideration of the opinions of others, has yet never at any time receded in practice from the high importance which, in the beginning of his long and useful career, he attached to blood-letting. Valuable instructions, based on large and intelligent experiences, too often perish and are lost to the profession with the sage who gathered them. Such as his most certainly should have a wider distribution and more permanent record than the desk or the lecture-room. For this reason do we epitomize and ask their publication with our own in this widely circulating journal of American obstetric experience.

We feel inclined to suggest in this place, hopelessly as it may appear to some: first, that the occurrence of eclampsia in a

pregnant woman will probably at no distant day be again regarded as *prima facie* evidence, not of an anæmic brain, but that that woman is the subject of *cerebral irritation*; either primary, depending upon *irritative centric hyperæmia*; or secondary, depending upon *obstructive renal congestion*, either or both of which conditions might have been averted, and of course with them, the consequent eclampsia, by a timely moderate blood-letting, had their existence been recognized and their dangerous influence duly estimated at the proper time. Secondly, that while we will not now distinctly advise that every eclamptic woman should be bled, we must unhesitatingly decide, in view of the almost uniform turgescence, either in the brain or in the kidney—this latter not promptly amenable to any other measure of treatment—that the omission of venesection from the treatment of eclampsia should ever be regarded rather as the well-considered and reluctantly admitted *exception*, than as the rule of practice.

CONCLUSIONS.

From the foregoing discussion, we think it will be admitted, that neither the recent investigations of experimental physiology, nor clinical observation, nor the results of therapeutics—taking any class of them singly, or all of them combined—can authorize us, at the present time, in recognizing either cerebral plethora or cerebral anæmia, or yet uræmia, or other toxic condition of the blood, as furnishing a uniform etiology for puerperal eclampsia. For, though any one or all of these conditions of the blood and blood-vessels may exist, and underlie and strengthen the true proximate cause, they should, all of them, for the present, be regarded but as influences, very frequently present, and when present, more or less tending to excite eclamptic perturbation; but that no particular one of them is specially necessary as an element in its production.

2. That the pathology and proximate cause of puerperal eclampsia cannot be safely assigned, in the present advanced though inconclusive stage of scientific investigation, as being even yet legitimately removed beyond that one, which may be generalized in its proximate etiological relation, and assignable to *every form of convulsive action*—the tetanic, the hysteric,

the infantile, the epileptic, or the anomalous; viz., *nervous irritation*. This irritation may be either centric or peripheric, or both combined;—that is, an exalted excitability of the reflex excito-motory instrumentalities, as they exist in receptive sensory nerves and surfaces, or in the motor or responsive ganglia, and in the motor nerves. For example, a distant excitor nerve—as in the uterus, or it may be a dental filament of the fifth pair—being impressed, awakens such molecular change in abnormally polarized motor cerebral, or motor spinal centres, as to evoke general convulsive action;—in the one case, the infantile convulsions of dentition may occur, in the other, the hysteric or eclamptic. Still further change the receptive excitor to a traumatic injury, and a tetanic convulsion may take place—the characterization of each being dependent on the determining concomitants.

3. That though this pathology, and the designation of *irritation* as the proximate cause of eclampsia, and of *all convulsions*, will be familiar to many as the manifest truism of a period almost past, it may well be recognized on that very account—seemingly paradoxical as it is—as a real and valuable advance; inasmuch as to accept it, will be to relinquish the conjectural, the uncertain, and the unknown, for the familiar, the well-established, the consistent and the safe. We make a real advance when we go back to take up and incorporate an undoubted fact, which we had forgotten or neglected or passed by, and which is one of the indispensable elements in the development of truth. If such pathology be but a truism, we must then be satisfied with truisms, until less inconclusive investigation ripens into consistent truth, the researches that have been recently so prematurely credited as our guides.

4. That the proximate cause of eclampsia having been recognized as one and the same, as that common cause of so many—of almost all morbid nervous phenomena—some controllable, some intractable, viz., centric and peripheral irritation, or exaggeration of reflex excitability—then the sole, the grand and consummate indication unavoidably takes precedence, as the precept and the object of all treatment—to *quiet and to subdue irritation*. The prophecy of diagnosis is here confirmed by the history of cure. The surmises of etiology are endorsed by the consistent results of therapeutics.

5. That irritation being recognized as the proximate cause of the frightful phenomena we have to combat in puerperal eclampsia, the well-known and universally acknowledged superiority and efficiency of opium and its preparations for controlling irritation, renders it the first, the most ready, and the most promptly effectual of all the means at the command of the practitioner. That its hypodermic administration, either alone or in combination with atropia, and its ready applicability by the rectum, still further entitle opium to unrivalled pre-eminence as the combatter and controller of, and the first application to be made in, eclamptic irritation of the motor ganglia and excitor nerves.

6. That next to opium, in its general applicability, and superior to opium in many specific cases—utterly indispensable in some—we should rank blood-letting as *the* sedative, upon which we may most confidently rely for direct and immediate influence in overcoming most of the forms of centric irritation and for subduing convulsive action—especially as it is found in the eclampsia of pregnancy and childbed.

7. That the statistics showing the death-rate in puerperal eclampsia—under treatment by bleeding on the one hand, and by anæsthetics on the other—while they go largely to exhibit the great value of these latter agents as the subduers of centric irritation, and to show that their *combined* power has been able to effect in modern times better results than venesection *alone*, or almost entirely unaided in the past;—still, these very statistics—even showing as they do, in some sense, an unfavorable comparison—go far to contradict the anæmic pathology of some modern authors, and to convince us, that venesection, so far from having been injurious, was, in this disease, a largely beneficial method of treatment.

8. That in the light of more recent investigations, a careful review of blood-letting as practised in the past, so far from resulting in its condemnation, confirms the value of venesection as a method of promptly sedating nearly all the causes of irritation upon which eclampsia depends. That while modern discovery has supplied to the treatment of eclampsia many valuable agents that control convulsions, independently of venesection; the very success of these remedies, while it plainly contradicts the pathological views upon which the condemna-

tion of venesection is founded, *should strongly re-establish our confidence in blood-letting*; as such success leaves it highly probable that it is by virtue of their being *cerebral exsanguinators*—by their lessening the quantity of blood in the brain, and by preventing “the determination of blood to the head”—that they, like bleeding, have been found to be most valuable remedies in puerperal convulsions.

9. That though hereafter—since we are in the possession of many valuable remedies of like action, and capable, by a similar *modus operandi*, of subduing eclamptic irritation, both centric and peripheral—the resort to blood-letting may not be so often necessary as formerly it was, as an impromptu and immediate direct sedative, venesection should yet by all means be retained among the most reliable of all our reliances. That even when a convulsion has been relieved by other means, or when the danger of an impending convulsion has apparently gone by, no woman, of plethoric habit especially, should be discharged by the medical attendant, until after he has carefully considered—in view of the tendency manifested, and the frightful danger just escaped—whether or not this should be one of the measures of that prophylactic after-treatment, so invariably the care and the anxiety of the thoughtful and faithful physician in charge of such a pregnant woman.

10. That venesection, when discussed in its relations to eclampsia, has been, and is now still held, too strictly to the consideration of its influence upon the encephalon and upon the vascular condition of the ganglia within the cranium; and upon these alone. That, though cerebral plethora may be denied by some, there can be no doubt of *renal turgescence*, in many cases—modifying the circulation and action of the kidney—causing albuminuria, and preventing the elimination of *urea*, which being retained and circulated in the blood, is often doubtless one of the causes of eclamptic seizure. Therefore, that venesection should in this relation be carefully and hopefully deliberated upon with reference to the “temporary,” but none the less, for the time, devastating Bright’s disease of pregnancy. That by the same *rationale* by which, among the older practitioners, bleeding became incidentally a diuretic in dropsy, so, in an analogous condition of these oppressed and crippled organs, may it become a rational method of restoring

diuresis to eliminate urea and other irritating products from the blood.

And lastly, in regard to the bromides, and to chloroform, and to chloral, and to quinine, and to applications of ice and cold affusions, and in regard, as well, to a large number of other remedies—some, like those first mentioned, being agents considered of the highest value—one *common therapeutic endowment* seems, in varying degrees, to be possessed by them all—that they are all, like venesection, nervous sedatives, *the subduers of nervous irritation* :—and, that falling into singular coincidence, by general consent, their therapeutic action, like that of venesection, is accompanied by one common physical result—that of lessening the amount of blood in the brain.

REPORT OF THE LYING-IN SERVICE OF THE NEW YORK
CHARITY HOSPITAL FOR THE YEAR 1875.

BY WALTER R. GILLETTE, M.D.,
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THE Report of the Lying-In Service of Charity Hospital, for the year 1875, is presented as a contribution to the statistics of such institutions. Heretofore, no detailed report has been submitted, for the reason that the service was but a limited one. The consolidation of the Bellevue Lying-In Service with that of Charity, in the latter institution, last year, was an event of considerable moment, and was looked upon by some as disastrous, inasmuch as it was so far removed from the city that many hardships were anticipated for the poor creatures who were obliged to seek it. That the step has not been detrimental to the patients, the report completely shows.

The Lying-In Service thus constituted is the largest in the country, and we expect that its reports will be looked for as important to the statistics of the obstetrical art.

In criticising or contemplating the results of this institution, we must bear in mind that its inmates represent the very lowest

of the poverty-stricken, unfortunate and criminal classes of this city and its suburbs. Hardly a patient comes to us who is not cast down in mind or body in some way. Thus forty-five per cent. of our inmates last year were unmarried women—women who had been seduced and betrayed, or prostitutes suffering with the varied forms of venereal disease. These constituted the most wretched and hopeless of the classes we had to treat. The remaining were women worn out with poverty and want, who entered our institution when forced by the necessities of their wretchedness, and who came to us not full of hope and courage, but rather in indifference or despair. We are allowed no selection of our cases. We are obliged to receive all who come before, during, or after labor, even though they may be already suffering from zymotic diseases; and we are obliged to consider the latter in our statistics along with those who come under more favorable circumstances. Our table—herewith submitted—represents the actual condition and results of our service as it transpired.

There were 613 patients under observation. Of these, 182 were unmarried primiparæ, and 75 were unmarried multiparæ. The former class were girls who had been seduced recently, and who were in the mental distress of their class. The latter class were for the most part prostitutes, and many of them drunkards. Almost all of this class were in some way suffering from the effects of dissipation and previous disease. Among the married, 84 were primiparæ and 222 multiparæ. This class of women only come to our hospital after they have exhausted all their means of livelihood, and have become reduced in health from want and care.

The nationalities have not been given, but the largest proportion were either Irish or Irish-Americans.

The average duration of labor was for the first stage 10 and two-fifths hours, for the second stage, 2 and three-sevenths hours, and for the third stage, 13 minutes.

In 64 cases, labor lasted over 24 hours.

Vertex presentations.—Of the vertex presentations it will be noticed that the second in frequency was the *right* occipito-iliac-anterior position—there having been 75 of these presentations—against 28 of the right occipito-iliac-posterior—the favorite second position in frequency of many authors since Næ-

gele's teachings. So far as these statistics may bear upon the settlement of the fact that the right occipito-iliac-anterior position is the second in order of frequency, it is just to state that our observations in these cases were not always taken at the beginning of labor, but at any time during the labor—consequently there can be no doubt that many of these positions, discovered when the vertex was in the pelvic cavity—or at the inferior strait, were simply conversions of the right occipito-iliac-posterior—such as occurs in the ordinary mechanism of labor with the head in this position.

Irregular presentations.—There were sixteen presentations of the pelvic extremity. Of these four were footlings, and twelve breech. Ten of these children were born living, and six were dead. Two of these latter were premature births, one was hydrocephalic and ascitic, one perished in a prolonged labor of twenty-nine hours, and one by delay of an arm-locked head.

The forceps were applied but once to the after-coming head. Their use is hardly encouraged in our hospital in these cases, inasmuch as we deliver with much greater ease and celerity, by making traction upon the head by the trunk, respecting with the greatest exactitude the pelvic axes.

Transverse presentations.—There was but one transverse presentation. The patient was a primiparous married woman, æt. 27, who was taken in labor January 17. The position was of the right lateral plane—right. The waters discharged almost at the commencement of labor. Version was attempted by external manipulation and failed. The combined external and internal method of Braxton Hicks also failed. Podalic version by the ancient process was effected, and the child was delivered—dead.

Face presentations.—There were four cases of face presentation. Two were of the right mento-iliac position and were delivered unaided. One was a mento-posterior position, and delivery was effected by craniotomy, and the erotchet. One was a brow presentation, which by forced extension was converted into a face, and delivered as usual.

Forceps deliveries.—There were eighteen forceps deliveries. Four were performed at the superior strait of the pelvis, the remainder at the excavation and inferior strait.

The conditions requiring this method of delivery, were powerless labor from unascertainable causes in four instances. Powerless labor from prolonged first and second stages in four cases. Tedious labor from rigid os, in two cases. Rigid perineum, in two cases. Justo minor pelvis in three cases.

Transverse narrowing of inferior straight, one case. To the after-coming head in pelvic presentation in one case, and one case delivered at Bellevue, sent to us without a history.

Five children thus delivered were born dead, one of which had undergone decomposition. One was born in suspended animation, and could not be resuscitated.

The perineum was lacerated to the second degree in two cases. In the case delivered outside, and sent to us without a history, the perineum was ruptured into the rectum. The rents were sewed up in each case by silver wire, but with generally unsatisfactory results.

It is the practice of our service to sew up all rents of the perineum immediately upon their occurrence. The results are hardly such as to encourage us in the continuance of the practice, for we never get complete union when the perineum is entirely torn through. In rents of the second degree we occasionally succeed. The superficial rents of the perineum seem to unite just as well when we treat them by the postural method of tying their legs together. The forceps used is by preference a combination of Elliot's handles with Simpson's blades; for the reason that the stop in the handle allows us to control the degree of compression. The variety of short forceps in use is the straight form, and these we employ only for encouraging rotation of the vertex or face positions requiring them. For purposes of extraction, the ordinary long forceps is adequate to the head in any portion of the pelvic canal.

Version.—Version was performed in four instances by the ancient podalic method.

In two cases it was for the relief of the child in prolapse of the funis. One child was born dead, the other living.

The third case was a transverse presentation, already related.

The fourth case was a placenta prævia, the details of which will be more fully related further on.

Craniotomy.—There was one case requiring craniotomy. This was the case of a married woman aged 32, in labor with

her seventh child. She came into the hospital suffering from the exhaustion of a terrible debauch, which had been continued up to the time of the commencement of labor. Examination revealed labor advanced well into the second stage; the presentation, face; and the position, mento-posterior. The chin was firmly wedged in the pelvis, and all attempts, either to rotate it into a more favorable position, or flex the head, were without avail. Auscultation revealed the foetus dead. Craniotomy was then performed, and the child delivered by the crotchet.

Induced labor.—There were two cases of induced labor. One was suffering with uræmic convulsions. The os was found patulous and dilatable. Uterine contractions were induced by introducing the female nozzle of a Davidson's syringe, and slowly injecting warm water, for an hour. Labor came on rapidly, and a living child was delivered in three hours.

The second was the case of a patient in whom was a dead foetus. She was eight months advanced in utero-gestation. Her health beginning to depreciate, labor was induced by dilating the os with Barnes' dilators. When sufficiently dilated, the membranes were ruptured, uterine action set in, and the labor terminated without difficulty.

Placenta Prævia.—There was one case of Placenta Prævia which terminated fatally to both mother and child. It will be related among the deaths.

Convulsions.—There were two cases of puerperal convulsions. One was in an unmarried, primiparous girl, aged 19 years. Albuminuria was detected three weeks before labor. Convulsions came on violently during labor, she having fifteen. She was immediately put under chloroform—wet cups were applied over the kidneys, forceps applied, and she was delivered without difficulty. Both mother and child did well.

The second case was that of a married woman, primiparous, aged 21. When first seen—Feb. 15, 1 P.M.—was semi-comatose. Had then three convulsions, urine albuminous and general anasarca. No signs of labor. Dry cups were applied to lumbar region, and a hot bath given. Two hours afterward she was improved. A hydragogue cathartic of elaterium was given. Also infus. digitalis, and potass. acetat.; after three doses of which she refused to retain it. Convulsions returned at 10 P.M., and

the patient passed into coma. Was wrapped in blankets wrung out in hot water, and chloroform administered with the effect of controlling convulsions. Labor was now induced. Convulsions returned at 11 A.M.; at 1 o'clock P.M. coma was profound, and there were indications of œdema of the lungs. Patient was partly cyanosed. Dry cups were applied over the chest and lumbar region with effect of greatly relieving the symptoms. From this time she steadily improved.

Retained Placenta.—There was one case of retained placenta in the case of an unmarried primiparous woman, twenty-eight hours in labor. It was removed by the hand in the uterus.

Post Partum Hæmorrhage.—There were nine cases of post partum hæmorrhage. The first was a married woman aged twenty-seven, in labor with her second child. Labor commenced at 3.30 A.M., and lasted just thirty minutes. The pelvis was a justo major variety. Hæmorrhage followed this precipitate labor furiously. The uterus would not contract—was evidently paralyzed by the shock of its efforts. It filled rapidly. The hand was introduced and the clots emptied. It was then held by the hand, ice was applied to hypogastrium and vagina, and brandy and opium administered. The pelvis was elevated, and after a few minutes, when these efforts had succeeded in controlling the flow, ergot was administered. Recovery was perfect. The patient stated that her former labor was exactly similar in its precipitateness and hæmorrhage. There was no laceration of the perineum.

The second case was one of labor prolonged to thirty-two hours. The shoulders and pelvis were delivered by traction, in order to relieve the semi-asphyxiated child. Hæmorrhage followed, but was controlled by ergot, grasping the uterus, and applying ice to hypogastrium and vagina.

The third and fourth occurred under similar circumstances, and were similarly treated.

The fifth followed a forceps operation, and was treated the same as the others.

The sixth was a precipitate labor, lasting but three-quarters of an hour. Uterus was distended with liquor amnii, which did not escape until after delivery of the child. The uterus failed to contract, and profuse hæmorrhage occurred. It was controlled by manual compression, ice, and ergot.

The seventh accompanied an attached placenta, and was easily controlled.

The eighth will be more fully detailed when we consider the deaths. The patient upon admission was immensely œdematous. The veins of the legs, vulva, and labia majora, were largely varicose. Hæmorrhage came on immediately after labor, and was wholly uncontrollable, although all possible means to check it were adopted.

The ninth occurred thirty minutes after a normal labor of eight hours' duration. The uterus relaxed, filled, and bled profusely. It was controlled by evacuating the clots, the administration of brandy and ergot, stimulating the uterus with ice, and manual compression.

The only hæmorrhages which we deemed worthy of the dignity of being called post-partum, were those in which the amount of blood lost was sufficient to affect the pulse and respiration. From the strict attention which is given to following the uterus with the hand as it expels its contents, and maintaining manual compression until the binder and compress are applied, and the invariable rule which is carried out of administering a dose of ergot immediately upon the delivery of the child, we hardly look for hæmorrhages of any severity.

Prolapse of Funis.—There were six cases of this accident.

In the first case, the presentation was vertex, position R. O. A. In the early part of the second stage of labor a small loop of the cord came down, and became caught between the pubis and the head of the child. It pulsated feebly. The pains were severe and rapid, and the head was well engaged in the superior strait. The cord was reduced; nevertheless the child was born asphyxiated.

The second case was a vertex presentation. The cord was detected before rupture of the membranes; and effort was made by postural treatment to get it to recede. The membranes burst prematurely, however, and eight inches of the cord protruded from the vulva. All efforts at reduction by postural and manual method failed, and as the pains were increasing in severity and effect, and the cord ceased pulsating, podalic version was performed: first pushing the head up toward the left iliac fossa. The breech and shoulders were delivered, but the head could not apparently be extracted by manual aid. Forceps were then

applied, and the child delivered asphyxiated. It was resuscitated, however, so that in an hour its respirations were normal.

The third case was owing to an excess of liquor amnii. The membranes rupturing, a loop six inches long was washed down. The woman was put upon her breast and knees, the cord pushed up and held in place until uterine contractions had sufficiently engaged the head to retain it, and the labor terminated naturally.

The fourth case is not noted in full. The child was born dead.

The fifth occurred in a twin birth. The second born was a vertex presentation with the cord prolapsed. Labor was rapid, the child small, and was delivered unaided and safely.

The sixth case occurred in a justo minor pelvis, and accompanied a presentation of the vertex, a hand and the foot. Attempts to replace the cord failed. The foot was then seized and version completed. The head was extracted with great difficulty, and the child born dead.

STILL BORN.

There were forty of this class, and the conditions accompanying or causing them were as follows:

| | |
|--|---|
| Children born undergoing decomposition (causes unknown)..... | 6 |
| Syphilis..... | 5 |
| Prolonged Labor..... | 5 |
| “ “ and Forceps..... | 3 |
| Unknown..... | 3 |
| Prolapsed Funis..... | 3 |
| Fatty Placenta..... | 3 |
| After-coming head (in breech presentations)..... | 2 |
| Calcareous Placenta..... | 1 |
| Placenta Prævia..... | 1 |
| Hydrocephalus..... | 1 |
| Injury from fall..... | 1 |
| Following Version..... | 1 |
| Cord wound around neck twice..... | 3 |
| Premature detachment of Placenta, (twins)..... | 2 |

Total 40

We come now to the consideration of the casualties. Of the five hundred and seventy mothers delivered at term, fifteen died.

The first case was a primiparous married woman, aged 27. She had been in labor several hours before she entered the hospital. Upon examination the os was dilated to the size of a quarter dollar; membranes unruptured. Presentation was made out to be transverse, position right lateral plane-right. Endeavors were made to perform cephalic version by bi-manual manipulation [external], but without avail. When the os was sufficiently dilated, podalic version was performed, and delivery effected. There was no delay in delivering the placenta, but the patient did not rally, although every effort was made in her behalf. She sank and died in twenty-four hours, apparently from exhaustion. No post-mortem was permitted.

The next case was that of a primiparous widow (seduced), æt. 22. Presentation vertex; position L.O.A. She was ill with pleuro-pneumonia when taken in labor. The labor was particularly distressing, lasting nine hours. She died forty-eight hours after. Post-mortem examination revealed pleuro-pneumonia of the right lung, and catarrhal pneumonia of the left lung.

The third case was a primiparous unmarried girl, æt. 19. She had been delivered in the city, Jan. 28th, 12 M., and when brought to the hospital, Jan. 29th, pulse was 120; temperature 104°. Abdomen tympanitic and tender. The urine retained, and a large slough in the left labium majus; peritonitis was well marked Jan. 30; and she died, Feb. 2, from general peritonitis, the result of want and exposure.

The fourth case was that of a multiparous single girl, æt. 25. The evening before labor commenced she was seized with a chill, and a sharp pleurodynia. This pain increased intensely, so that at the end of the first stage of labor she was suffering great agony. A hypodermic injection of morphine was given her, with evident relief. Auscultation revealed crepitant râles over the entire left lung. Labor progressed, and she was delivered of a living child in four hours. She died two days afterward of pneumonia. It is interesting to know that when six months advanced in pregnancy, this patient had semi-confluent small pox, and was an inmate of the Small Pox Hos-

pital. She was quite ill, but the process and product of gestation were in no way interfered with.

The following case is one of great interest. The patient was a colored woman, pregnant for the third time. She gave no history of trouble with her two former labors. She entered the hospital March 15, seven months and a half advanced in pregnancy. Two weeks previous, while sleeping, was seized with a profuse hæmorrhage. She had several hæmorrhages after this. She was anæmic and exhausted. Examination revealed the placenta centrally implanted over the cervix, but detached for an inch or so on the right side anteriorly. On the 18th, she had a drenching hæmorrhage, which threatened her dissolution. The house physician immediately introduced his hand, gradually dilated the cervix, and passed within the uterus at the point where the placenta was already partially detached, detaching it still further. Seizing the feet, he turned and delivered easily. The uterus contracted firmly, after the removal of the placenta; but hæmorrhage continued, and was found to proceed from a fissure in the cervix. This was checked with liq. ferri persulph. She did not rally, however, but died an hour afterward. A post-mortem examination revealed the thoracic viscera, liver, spleen, kidneys, stomach and intestines healthy. The uterus measured $9\frac{1}{2}$ inches in length, 6 inches in width, walls $\frac{1}{2}$ inch thick, weight 3 lbs. 10oz. Coagula were traced into the enlarged uterine sinuses. At the seat of fissure, coagula from liq. ferri persulph were found. A laceration two inches long was found in the cervix along the right posterior-lateral wall. A sub-mucous fibroid tumor, two inches in diameter, covered by a veil of decidua, was attached to the right posterior wall of the body of the uterus. An intramural fibroid was found at the juncture of the right Fallopian tube with the fundus one inch in diameter; also two more, one in the central portion of the fundus, and the other in the posterior wall of the body. Nine subperitoneal fibroids, varying in size from a No. 4 shot, to one inch in diameter, were scattered irregularly over the surface. Peritonic bands of adhesion were noticed in the pelvis. There was also a cyst of the right ovary. This is a peculiarly interesting case. The fibroid condition of the fundus and body of the uterus, no doubt prevented the implantation of the ovum at the normal

site, and precipitated it to the cervical zone, where it became attached. It is worth considering how often fibroid tumors of the uterus predispose to the condition of placenta prævia.

The next case of death, was in a woman who had phthisis. Two days before labor she developed pleuro-pneumonia. She was delivered of a healthy boy, and died two days afterward.

The following was a death from post-partum hæmorrhage. Patient was unmarried and a primipara—entered hospital May 22; had had pains since evening previous; legs and labia majora were immensely œdematous. The veins of leg, vulva and pars intermedia were largely varicose. Urine contained no albumen. Os rigid, and waters discharged. Presentation and position, vertex, L.O.A. Os dilated at 3-30 A.M., 23d, and a hand was found presenting beside the head; it was pushed up, out of the way. Pains were strong, chloroform was administered, and the child delivered without difficulty. When delivered of the placenta, profuse hæmorrhage occurred. Ergot was administered with whiskey, cold applied to the abdomen, ice introduced into the vagina, and the uterus held. The uterus failed to contract, and hæmorrhage continued. Liq. ferri persulph. was then injected into the uterus, but without avail; and despite every remedial effort the patient died in half an hour from syncope.

The next case was that of a primiparous woman who had a vertex presentation, with prolapse of the funis. Efforts were made by taxis and the postural method to reduce the cord, but without avail, whereupon podalic version was performed, and the child delivered dead. The mother promised recovery for five days. On the sixth the temperature ran up to 104°—the pulse corresponding—the lochia became fetid. Septic symptoms rapidly developed. The uterus did not present any marked enlargement; nevertheless, it was washed out with a solution of carbolic acid. A mass of broken-down blood clots came away, and with some effort a piece of stinking placenta was expelled. Immediately after this expulsive effort, the patient seemed to pass into the condition of shock, and died. This was a distressing case, as undoubtedly it was the result of carelessness upon the part of the party managing the labor.

The following case entered hospital June 23, complaining of

pain in the head and back, disturbed digestion and insomnia. Tongue furred and bowels constipated. Bowels were emptied and quinine given liberally. Examination of urine revealed nothing. June 25, labor began and terminated in seven hours—normally. June 26, the face, neck and chest were found very red. Morning, pulse 101; temp. $100\frac{1}{2}$. Evening, pulse 132; temp. 102° . Erythema deeper, of a bluish cast and dusky. Lochia normal, no abdominal tenderness nor tympanites. Pain in back severe, causing patient to scream out. Ecchymotic spots were now discovered over the face, neck and chest, ranging in size from a bean to a half dollar. Conjunctivæ ecchymosed, urine bloody. Catheterization at 10.30 drew a drachm of pure venous blood. Patient sank rapidly, and died at 1 p.m. Autopsy. Body well nourished, covered with ecchymoses. Epidermis exfoliating. Brain, lungs and heart normal. Spleen large, diffuent. Hemorrhage in pelvis of right kidney, otherwise kidneys normal. Stomach and intestines normal; so also uterus and appendages. A small ecchymosis on the peritoneum was found.

The next case was that of a married multiparous woman, who came into the hospital in labor, with a face presentation, chin posterior. She was in an almost moribund condition, having been upon a prolonged debauch of ten days' duration. She was intoxicated when labor set in. She vomited incessantly; pulse was feeble and rapid. Temperature 103. Pains were rapid and strong. The chin was jammed in the pelvis posteriorly, and could be neither rotated nor flexed. The child was dead. She was anaesthetized. Craniotomy, and extraction by crotchet were performed, without any unusual difficulty, but she sank and died five hours after the delivery.

The next was a prolonged labor due to rigid os. The patient was a feeble wretched creature, and pains were very inactive. Symptoms of exhaustion appearing, the forceps were applied at the superior strait, and a dead child born. She was troubled with constant emesis, and sank steadily for ten days, and died apparently from sheer exhaustion, despite all possible restoratives.

Case was a multiparous single woman, who was delivered of a living child after a labor of eleven hours. There was nothing peculiar about the case until the following morning, when she

became partially comatose. Pupils contracted, respiration sighing. Urine was scanty, albuminous, and bloody. Face purplish and dark. Purplish petechiæ were seen on the body and extremities. The conjunctivæ were also ecchymotic. She died comatose.

Case XIII. was a single primiparous girl, aged 19, who was delivered of a putrid child after a labor of fifteen hours. Two hours afterward had a chill. Pulse became rapid and feeble. Temp. 102° , and covered with profuse perspiration. The uterus was washed out twice daily. Quinine, opium, and stimulants freely administered, but peritonitis became marked and she died four days afterward.

Case XIV. was an unmarried girl, aged 19, a small, puny, weak, miserable creature, who said she had never enjoyed good health.

She had a tedious labor lasting 27 hours, and was very much exhausted at its termination. Temperature immediately after ran up and ranged between 100° and 105° for ten days. Had irregular chills, fetid breath, partial coma, a peculiar sweetish pyæmic odor from her body, and a collection of fluid in the knee-joint. Upon autopsy, pus was found in the knee-joint, uterine walls, ovaries, and peritoneal cavity.

Case XV. had been delivered six days, and was convalescent. On the sixth day she walked into the bath-room and washed herself. Coming out, she sat upon the bed, and after saying that something was "squeezing her head," fell back, gasped a few times, and expired.

Autopsy revealed thrombosis and embolism of left pulmonary artery, and a fatty heart.

Case XVI. was in labor ten hours, and delivered easily, though at the time there was noticed a globular mass in the left half of the abdominal cavity just above the pelvic brim, which was considered to be a fibroid tumor. The patient did well until Dec. 31, when she had a chill, and complained of great depression. Pulse and temperature ran up. Lochiæ became offensive. No marked pain. She rapidly declined and died. Autopsy revealed an intra-mural myo-fibroid tumor four inches in diameter, extending from the attachment of the ligament of the ovary to the vagina, on the right side. There was a laceration of the cervix uteri and vagina on the right side, cervical metritis,

abscess of right ovary, and parenchymatous degeneration of the liver and kidneys.

The last case was that of a poor creature, single, æt. 26, who had chronic Bright's disease, a mitral regurgitant murmur, and phthisis advanced to the third stage. She was in labor nine hours, and delivered Dec. 31st of a living boy weighing 9 lbs. She died Jan. 2d of dyspnœa from phthisis, and œdema of the lungs.

In reviewing this report, we cannot fail to be encouraged by our immunity from the septic diseases incident upon hospitalizing lying-in women. We attribute this wholly to our rigid system of cleanliness. If occasion permitted, we would detail our method of hygienic care; but perhaps that would be unnecessary, as the rules are thoroughly well known, and their importance appreciated by those entrusted with the management of this class of cases. We have made no detailed mention of the cases of premature labor, and abortions, as they were of no special interest. There were no deaths among the mothers in these classes, and the causes of their occurrence in the great majority of instances are purely hypothetical. No apology is made for the paucity of some of the histories, as it was not deemed advisable to burden the report with minute details of cases that present no very novel features.

CLINICAL CASES.

CEDEMATOUS [HYPERTROPHY OF THE PREPUTIUM CLITORIDIS, FOLLOWING CHANCROID.—REMOVAL BY GALVANIC CAUTERY.

BY

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THE following case is of interest, insomuch as it is an extreme illustration of a condition which, in a minor degree, is not very uncommon.

B. S., æt. 22, a native of Canada, and by occupation a prostitute since the early part of 1871, was admitted into the Charity Hospital, New York, Aug. 17, 1875.

Previous History.—In the summer of 1871 she had several sores upon the vulva, which were treated with nitrate of silver, and healed in three or four weeks. In September, 1863, she had a bubo, which suppurated for about five weeks. In January, 1874, she suffered from erysipelas of the face and scalp. About two weeks prior to her admission into the hospital she noticed several sores upon the vulva.

August 17, 1875.—Upon examination a large chancroid was found in the fourchette. The preputium was somewhat enlarged, and upon its under surface there was another large chancroid.

January 3, 1876.—At this date she came under my care. The prepuce had now increased to a pedunculated mass, nearly as large as the fist, with a pedicle five inches in circumference. Its under surface was occupied by a chancroid about one and a half inches in diameter, exhibiting no tendency to heal. The whole tumor was painful upon pressure, so much so as to render a thorough examination impossible without an anæsthetic. The right labium majus was somewhat enlarged and covered with hard tubercles, chiefly on the posterior portion. The fourchette and neighborhood was the seat of an indolent chancroid, a little smaller than the one under the prepuce. From the period of her admission to the present time she had received the usual hospital treatment, viz., cauterization with nitric

acid, followed by iodoform. The appearances presented at this time are shown in Fig. 1.

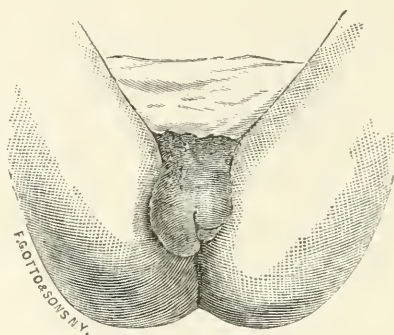


Fig. 1.

January 13.—The previous treatment was continued until to-day; when, finding no improvement, I thoroughly cauterized the sores with the solid nitrate of zinc.

January 17.—Chancroids beginning to heal. Zinc repeated.

“ 20. “ improving. “

“ 24. “ “ “

“ 27. “ nearly well. “

To-day a little of the secretion from the sores was inoculated upon the thighs.

January 31.—The inoculations failed to “take;” and, as the sores were almost well, it was decided in consultation to remove the enlarged mass by galvanic cautery. Present Drs. Otis, Chamberlain, Cleveland, Frankel, and Fox, and members of the house staff. After etherization, a platinum loop was placed around the neck of the tumor and heated by the cautery battery. Constriction of the mass was then commenced, and continued for 1’30”, when the wire broke, necessitating a readjustment, which being accomplished, constriction was continued with greater care, as the size of the loop diminished, and with frequent opening of the galvanic circuit as the heat became too great. At the end of 2’30” the pedicle was completely severed without the loss of a single drop of blood. After the effects of the ether had subsided, the patient experienced a great deal of pain in the parts, and a large urticarial wheal appeared upon the left arm, near the shoulder. Morphine in full doses was ordered.

February 1.—The parts still very painful, and there was a loss of about a drachm of blood.

February 2.—Parts swollen, painful, with free but offensive discharge. Ordered carbolized douches.

February 3.—Less discharge and swelling, but very painful.

February 7.—Still painful and not looking well. At upper angle of the wound, commencing ulceration. Ordered solution of potassio-tartrate

of iron to the parts. This application increasing the pain, it was replaced by curson oil.

February 11.—Since using the oil the pain is very much less, and the swelling has diminished. The wound looks healthy everywhere, and is commencing to heal at the edges.

February 21.—Is looking very well and healing fast. The chancroid in the fourchette is also much smaller.

February 29.—The wound is looking well, but all applications are painful; the right labium is still enlarged.

April 3.—During the month of March I was absent from the hospital, but upon returning at this date found the right labium somewhat larger than a month previous, and presenting the aspect shown in Fig. 2.

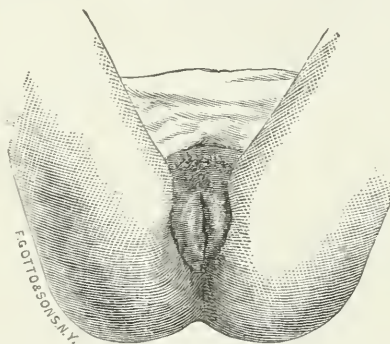


Fig. 2.

The wound has assumed the characters of an indolent chancroid, and the sore in the fourchette has enlarged and presents the same characters.

June 15.—The sores are still indolent and unhealed, though smaller than at any previous time. The right labium still enlarged, as shown on the cut.

Microscopical Examination.—Portions of the enlarged prepuce were hardened in alcohol, sliced, and the slices stained with picro-carminate of ammonia. Upon examination, the *stratum corneum* was found to be thin, but the *stratum Malpighii* greatly thickened. The *papillæ* were long, and infiltrated with small, round cells. Most of the *papillæ* contained dark granules like pigment. The tissue of the corium was richly infiltrated with round cells, some in clusters, others diffused. The blood-vessels were greatly enlarged and surrounded with cell-sheaths. There was no young tissue, or other evidence of organization discovered.

Remarks.—Moderate enlargement of the labia and of the prepuce are by no means rare accompaniments of chancroids of these regions, and usually subside spontaneously with the healing of the lesion. In this case, however, the œdema was

excessive, and the venereal ulcer large. Each reacted upon the other; the ulcer continued to act as an exciting cause of the œdema, and the œdema prevented healthy reparative action in the ulcer. It seemed, therefore, best to remove both by operation, and, as the part appeared to be very vascular (as it proved to be upon microscopical examination), to employ the cautery loop in preference to the knife. Both ulcers were supposed to have lost their virulence, but such proved not to be the case, as chancreoid action became apparent not long after the operation, the pus from the lower chancreoid infecting the surface of the wound. Indocility on the part of the patient, and her lack of thoroughness in making proper applications to the diseased parts, were among the causes which rendered the healing process more tedious than it might otherwise have been.

A SUGGESTIVE GYNÆCOLOGICAL CASE.

BY

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For some weeks after putting herself under my treatment for "womb disease," which I found to be corporeal endometritis of long standing, and ovaritis with dysmenorrhœa, the lady whose case gives occasion for the following remarks had made progress which I thought exceptionally good. But while doing so well, she had an attack of biliary colic; and this put a sudden period to her favorable progress.

Two days after the untoward event, while she yet suffered the usual after-effects of the passage of a gall-stone—pain and tenderness about the epigastrium with disturbance of digestion—I made examination of her genitals. The introduction of the speculum, and other necessary manipulation, gave her considerable pain; and I found that since my last examination—at the time of which the vagina was entirely free from inflammation, the uterus and diseased ovary very little tender, and the leucorrhœa slight, and consisting of little else than mucus—all had

changed; the vaginal mucous membrane was now very red, humid, and tender; light pressure upon the womb and ovary gave much pain, and there was an abundant discharge of bloody pus pouring from the os, which was now more than usually patulous.

The connection between this very decided change for the worse in my patient's condition and the biliary trouble is sufficiently evident, I think, not to require that I should give here the anatomical and physiological reasons why we may readily refer such a response on the part of the pelvic organs to irritation set up in the tissues of the bile ducts and the duodenum; and it is sufficient merely to call attention to the very frequent dependence of hæmorrhoidal congestion upon the same cause, and to the fact that whatever will produce fulness of the hæmorrhoidal veins must, of anatomical necessity, similarly affect those of adjacent organs.

Believing that this case presented in a very forcible manner a condition which is present more or less markedly in a large proportion of all the cases we meet with in gynæcological practice, it was to me very interesting, and strikingly illustrative of the importance of looking well to the state of the organs of digestion while we treat diseases of the uterus and its appendages. The very frequent association of dyspepsia with chronic inflammation of the uterus, and the *impossibility*, I believe I am right in saying, of getting good results in cases where only local treatment is used, growing out of such association, are well-known. The case under consideration, in affording an instance of irritation, which, though severe, was of but comparatively short duration, yet producing effects upon the pelvic viscera that lasted a month, enables us in some measure to judge of the effects produced by the less severe but more abiding congestions which enter essentially into the pathology of the commonest forms of dyspepsia. And it is a most reasonable inference that congestion of the mucous lining of the stomach or intestines, which acts, under otherwise normal conditions of the body, simply as a predisposing cause to uterine disease, will, after its establishment, tend strongly to keep it up, and to thwart the best devised and most skilfully carried out local treatment. I believe this unfavorable condition is much more frequent than is generally supposed, and enters as a most im-

portant element into the difficulty we meet with in curing chronic corporeal endometritis. A very common error seems to be that the dyspeptic symptoms we so often meet with are regarded simply as a consequence of the uterine trouble; and local treatment is relied on, with the hope that they will pass away when their cause is removed. The very unsatisfactory results of local treatment used solely are a sufficient commentary upon this theory. Facts in my case indicate very clearly that we should look upon the pathological state giving rise to dyspepsia as a far more important complication—that we should consider it causative, if not of the uterine trouble, *ab initio*, at least of its obstinate resistance to treatment; and while it is not for a moment doubted that organs connected with the digestive process may suffer along with all others in the general impairment of health, that is usually caused by uterine disease of long standing, the purpose of this paper is to show that in adopting a plan of treatment, another order of events must be looked to—that in which the dyspepsia is, in an important degree, primary.

No one would think of trying to relieve a patient suffering from hepatic congestion by removing any hemorrhoids that he might have; but it is a matter of very common experience that remedies addressed to the former trouble generally prove, if not sufficient treatment for the latter, at all events a most useful adjuvant.

CASES FROM THE NEW YORK STATE WOMAN'S HOSPITAL.

REPORTED BY
JOSEPH D. ANWAY, M.D.,
House Surgeon.

I.—UTERINE FIBROID, GASTROTOMY, DEATH.

DORA GYVES, æt. 42, single, native of Ireland, was admitted to the service of Dr. T. G. Thomas, at the Woman's Hospital, June 10th, 1873. History: Menstruated first at fourteen years of age: was always regular up to one year ago, when the flow began to appear at irregular intervals, and finally disappeared.

The duration of the discharge was always three days, and the amount was scanty. She has never had any leucorrhœal discharge until quite lately, and now has it only occasionally, and in very small quantity.

Six years ago she noticed that she "was growing stout," but did not pay any particular attention to it until three years later, when she consulted a physician and was told she had a tumor.

Two years ago she had an attack of pain in the right side, attended by fever, and was confined to bed for two weeks. This tenderness and fever followed an examination by several physicians. With the exception of an occasional pain in the right side, and the inconvenience of being so large, she has been quite well since the illness two years ago.

During the first few months after the discovery of the growth, it did not increase very rapidly, but during the past year the change in size has been quite marked. Her bowels have been generally constipated. Micturition has been frequent, but not painful.

Present Condition.—The patient is unable to lie on her back owing to pain in the lumbar region, which is very severe when she is in that position. She can, however, lie on either side, but better on the left. She sleeps very poorly. Her appetite is good, and she has no trouble at present with digestion. Is able to be up and about the ward. Resp. 20. Pulse 92.

Physical Examination.—The tumor, which is of great dimensions, is hard to the touch, and feels rough though not irregular. Here and there soft places were thought to exist. Into one of these the hypodermic needle was introduced, but only a few drops of fluid were drawn. The uterus could not be reached by digital examination per vaginam. The measurements of the abdomen were as follows: Circumference, fifty-two (52) inches; ensiform appendix to umbilicus, thirteen (13) inches; umbilicus to pubes, fourteen (14) inches; anterior superior spine of ilium to umbilicus, sixteen (16) inches.

Diagnosis.—The diagnosis of *uterine* fibroid was made.

The patient, on account of the great discomfort which the tumor occasioned, urgently requested operation, but a consultation being held by the visiting staff of the hospital, it was decided not to run the risk of removal, and she left the institution.

The following Spring she returned to the hospital, and requested that an operation for the removal of the growth be performed. She was again informed of the dangers, but still she insisted on its removal, and she was finally readmitted, May 15, 1875, for that purpose. Her condition was found to be about the same as when discharged in the Winter. The tumor had not increased in size to any extent, but the patient's condition was very wretched.

Dr. Thomas now called a consultation of Drs. Emmet, Peaslee, Markoe, and Peters, and, after careful consideration, operation was decided upon.

Operation. May 18th.—The patient under ether, the operation was begun by Dr. Thomas at 3.05 p.m. Drs. Walker, Hunter, and B. Emmet assisting. An incision was first made through the skin and subjacent tissues, down to the peritoneum. This incision was in the median line and about four (4) inches in length, extending from pubes up. When the blood had ceased to flow from the cut surfaces, the peritoneum was picked up with forceps, a small opening cut with scissors, into which a director was passed, and the membrane divided throughout the extent of incision. The hand was now introduced and the tumor found to be everywhere adherent, but the adhesions could be easily broken. The incision was extended to the ensiform appendix, which pointed directly forward from pressure of tumor. The adhesions on all sides were broken with the hand or cut with scissors, and during this process it was found necessary to ligate several quite large vessels. The tumor, having been cleared of adhesions, was forcibly held up by two assistants while the aorta was compressed by another. A long needle carrying a double ligature was passed through the upper part of the apparent pedicle (found afterwards to be the lower part of the cervix uteri) and through a projecting nodule of the tumor itself. The ligature was tied on either side, and the pedicle divided above the ligature. Dawson's clamp was first used to secure the pedicle, but afterward that of Spencer Wells substituted for it. The abdominal cavity was now carefully sponged out, clots removed, and all bleeding points tied. The pedicle was now further examined and found to be too short to bring outside, therefore another double silk ligature was passed through the middle of the pedicle lower

down, tied on either side as before, and the clamp removed. That portion of the pedicle above the last ligature was cut off, this time the division taking place through the upper part of the vagina, and the remaining portion fastened into the abdominal incision by means of long pins. Thomas's drainage tube was introduced, and the wound closed with twenty (20) silver wire sutures, Peaslee's needle being used.

Time of operation thirty-three (33) minutes. Weight of tumor (49) forty-nine pounds.

Pulse during the operation was about 96.

The patient was removed to her room at 4 P.M.; came out from under the influence of the ether nicely, and seemed bright. There was no vomiting; complained of some pain, and Morph. Sulph. (Mag. Sol.) M vii. was given hypodermically. The tube was examined and a clot of blood drawn out. She soon began to show signs of collapse, the pulse being very feeble, though about the same in frequency.

The surface was rather too cool, and hot bottles were ordered to sides and feet of patient, and brandy to be given every five minutes.

She soon rallied.

8 P.M.—Pulse 112 and stronger. Temperature $100\frac{1}{2}$. She complains of some pain in the abdomen, and is sweating quite profusely. Morph. Sulph. (Mag. Sol.) M vii. given.

11 P.M.—Pulse 128. T. 101. Patient is bright and feels comfortable; has slept considerably. Morph. Sulph. (Mag. Sol.) M x.

May 19th, 4 A.M.—P. 132. T. $101\frac{1}{4}$. Has had Morph. Sulph. (Mag. Sol.) M x. by mouth during the night; feels very comfortable now; has slept most of the time; complains of considerable wind in stomach, with eructations; has had only brandy and water thus far to drink. Now ordered cold milk punch instead.

10 A.M.—Morph. Sulph. (Mag. Sol.) M x. by mouth. The abdominal cavity washed out by Dr. Thomas; quite a large quantity of clotted blood came away.

1 P.M.—P. 140. T. 102. Resp. 20. Patient has slept considerable this morning; vomited a little water from ice taken; has considerable pain. Morph. Sulph. (Mag. Sol.) M vii. hypodermically; ordered $\frac{3}{4}$ i. brandy in divided doses in the

twenty-four hours, and in addition the $\bar{3}$ i. doses of brandy and milk every hour.

5 P.M.—P. 152. T. 103. Resp. 16. Has considerable pain still. Morph. Sulph. (Mag. Sol.) M vii.—subcutan. The pulse has gradually grown weaker and the patient though weak is very bright; ordered all brandy possible. It is evident that internal hemorrhage is destroying the life of patient.

8.50 P.M.—P. 146. T. $103\frac{3}{4}$. The patient being under the influence of ether, Dr. Thomas opened the abdominal wound, sponged out the cavity, and finally secured a large bleeding vein at a fold in the transverse colon, closing the abdomen again very rapidly. At commencement of operation the patient was pulseless at the wrist. A vein in the right arm was with some difficulty opened, and an attempt to transfuse directly from the arm of Dr. S. B. Jones, by means of Aveling's apparatus, which could not be made to work. Finally some blood was drawn, defibrinated, and $\bar{5}$ iii. thrown into the veins of the patient. During the operation the foot of the bed had been raised, and brandy enemata given. The pulse after the operation was 158 scarcely to be counted.

May 20th, 4 A.M.—Patient vomited some after operation; has had nothing by the mouth, but has taken and retained brandy enemata every hour since; has rallied somewhat; appears bright; pulse 144 and very weak. Morph. Sulph. (Mag. Sol.) M.vii. subcutan.

8 A.M.—P. 140. T. $103\frac{1}{2}$. Patient has slept well since four o'clock; says she feels quite comfortable; pulse thready.

May 20th, 9 A.M.—A clot was found in the drainage tube and sucked out, also a sancer full of fluid blood from the cavity. Morph. Sulph. (Mag. Sol.) M.x. given on account of pain. Her body is quite warm, but her hands and feet are quite cold. Ordered hot water in bottles to hands and feet. Patient very weak, but bright. Compress applied over site of hemorrhage and tightly bandaged. The pulse remains about the same: bleeding evidently continues, as dark fluid blood can be sucked out of the abdominal cavity.

3.45 P.M.—In the presence of Drs. Markoe, Peters, Peaslee, Sims, and others, Dr. Thomas again attempted to transfuse blood into a vein of the left arm, but the vein was evidently obstructed higher up, as a varix formed when the blood was

injected. The left saphenous vein was then opened after some delay, and about $\frac{3}{4}$ i. of defibrinated blood was thrown in. No perceptible change in the pulse followed this operation.

6 P.M.—Pulse 148. Resp. 28. Complains of feeling faint, and is apparently dying. Takes and retains large quantities of brandy by the mouth.

7.45 P.M.—*Died*.

II.—RETROFLEXION.

Miss B——, æt 23. Single. Admitted to Woman's Hospital, December 1, 1875. Service of Dr. T. Gaillard Thomas.

The patient first menstruated when 14 years old, and for four years was perfectly regular. There was no pain connected with flow; the time was four or five days; and the amount was normal. When eighteen years old she received an injury in the following way. While washing clothes she attempted to lift the stick used for stirring the clothes while boiling, laden with clothes, from the boiler to a tub. In so doing she slipped on the wet floor, and fell backward on her left side. She felt something "give away" at the umbilicus, but continued her work until the next day, when she was obliged to go to bed. During the whole summer that followed, she was unable to put her right foot to the ground, owing to a sensation as of "something pulling" in the right iliac region. Her troubles were ascribed to an injury of the right psoas muscle. From the time she fell until the present, she has had severe pains at periods covering one week before the flow, and lasting all through it. During the autumn following the injury, she was able to walk about for a short time, but was soon laid up again, and could only go about with the aid of crutches. The pain has always been located in the right iliac region, and has always been made worse by walking—in fact, she has never been free from pain since her fall. The time of flow is now only one day, and the amount is small and light colored. Last spring the pain at menstrual periods was so intense as to cause her to faint, and again three weeks before coming to the hospital she suffered in the same way.

Phys. Exam.—The cervix looks upward; the sound passes backward and downward two and three quarter inches.

Treatment.—December 2d.—The uterus was replaced and a Cutter's bulb pessary introduced.

December 3d.—The pessary has caused considerable pain, and was removed in consequence.

December 9th.—A Thomas's retroversion bulb pessary was introduced to day—one having very little curve—in fact nearly straight.

December 16th.—Patient has worn the instrument without any discomfort and is able to walk about with ease. Says she has not felt so well since her troubles began.

December 30th.—The instrument which the patient has been wearing has accomplished all it can ; and one a little larger, with more curve, which will throw the uterus over into the position of anteversion, was introduced.

January 6th.—The last instrument introduced does what was required, but is a little too broad, and causes some pain ; therefore it was removed, and one a little smaller in every way substituted for it.

January 13th.—Uterus in position ; can walk with perfect ease.

January 27th.—Patient has not menstruated during the last eleven weeks, but has some uneasy feelings about the abdomen as though she were going to.

February 12th.—Patient has been waiting for the menstrual flow to appear, but as it has not, she was ordered to go home, with the hope that change of climate would bring it on.

March 1st.—A letter from the patient informs us that the "flow" appeared the week following her return, and for the first time in five years she was free from pain during it.

TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

Reported by PAUL F. MUNDÉ, M.D., Secretary.

Stated Meeting, March 7th, 1876.

DR. B. F. DAWSON, *in the Chair.*

ARTIFICIAL LEECH.

DR. SKENE exhibited an instrument devised by Dr. W. W. Reese, of Brooklyn, for leeching the cervix. It consists of a glass tube, in which runs a hard rubber piston, from the bulb of which can be projected a spear-shaped knife by pressure on a knob in the handle; by then drawing back the piston suction is made on the incised spot. The glass tube permits the amount of blood extracted to be accurately measured, and the force of the suction can be regulated at the will of the operator. The instrument is a very convenient one, and the best one made thus far. It entirely supplies the place of the natural leech and has none of the troublesome, time-costing attributes of the latter. Besides, it is not followed by irritation of the cervix, as is the bite of the leech, and no infiltration of the tissues ensues; neither does the hemorrhage continue after the artificial leech.

DR. MUNDÉ asked Dr. Skene whether he really thought that leeching, either by the natural or artificial article, is beneficial. There have always been some doubts as to the value of the procedure; and Schröder, in his late work on Diseases of the Female Sexual Organs, infinitely prefers frequently repeated scarification to leeching, which, he says, is at once followed by an increased afflux of blood to the uterus.

DR. SKENE said that in using local depletion of the uterus he had always made a distinction in the class of cases chosen: 1. In mere hyperæmia of the uterus, to relieve congestion and hyperæsthesia, he had always found free scarification the most beneficial. 2. In engorgement of the uterus, corporeal metritis and endometritis, leeching is better and more enduring. His practice has been to limit leeching to the period immediately after menstruation, applying one leech at that time; if indis-

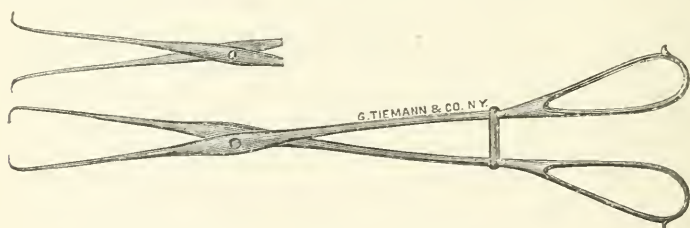
criminatedly used, it will increase, instead of diminish, the hyperæmia. Scarification should be made once or twice a week. He fully believes, moreover, in the existence of such a pathological condition as uncomplicated hyperæmia.

DR. BYRNE said, that the question of the effects of leeching had been discussed by the London Obstetrical Society a number of years ago. He, for his part, had never applied a leech, and only scarifies occasionally. He has watched cases of uterine disease treated by local depletion for a long while, and has never seen the least benefit from that treatment, except in simple hyperæsthesia of the uterus.

DR. H. T. HANKS exhibited a

TENACULUM FORCEPS.

The accompanying cuts represent a pair of tenaculum forceps particularly useful for steadying the cervix uteri during



operations on this part. They consist of two tenacula, about $8\frac{1}{2}$ inches in length, with a loop-like handle, held together by a rivet similar to that in a disjuncting pair of scissors. Between the rivet and the hook-ends, each tenaculum is slightly bent, so that the points, when the instrument is closed, do not slip past. The little rubber ring serves as a spring to close the points.

In order to use the instrument as represented in the section cut, the points must be sprung apart, laterally, in closing, and the rubber ring drawn down to the notches in the handle.

We thus have an instrument that can be used in four different ways :

1. As two separate tenacula.
2. As a forceps for steadying one or both lips of the cervix in operating for lacerations.
3. As a local depletor. By means of one point within, and the other without the cervical canal, and firm pressure at the handles, considerable blood may be quickly removed.

4. For steadying the cervix, when introduced within the cervical canal, as in the section cut. In this respect they are similar to Noeggerath's forceps.

This instrument, in its different capacities, has served me well for more than a year.

Stated Meeting, March 21st, 1876.

The President, DR. T. G. THOMAS, in the Chair.

The President appointed a committee, consisting of Drs. Byrne, Hunter, and Ward, to draw up suitable

RESOLUTIONS ON THE DEATH OF DR. CHARLES F. RODENSTEIN,

late a Fellow of the Society. The committee reported the following resolutions, which were unanimously adopted:

Whereas, This Society has learned with deep regret of the death of Dr. Charles F. Rodenstein; be it

Resolved, That this Society recognized in the late Dr. Rodenstein an active and valuable Fellow, whose regular attendance at its meetings, costing him a journey of many miles, was a proof of his professional zeal and constant interest in the welfare of the Society.

Resolved, That the death of Dr. Rodenstein is a loss to the profession of an able and energetic physician, and to society of a gentleman of large culture and genial disposition.

Resolved, That the Society tender to the bereaved family of Dr. Rodenstein their heart-felt sympathy in their affliction.

Resolved, That a copy of these resolutions be sent to the medical journals of this city for publication.

Signed, JOHN BYRNE,
JAMES B. HUNTER,
CHARLES S. WARD.

THE CERVICAL TAMPON IN ABORTIONS.

Referring to a case of abortion with twins at the ninth week, reported by Dr. C. C. Lee, in which the vagina was tamponed, contrary to Dr. Lee's usual custom—DR. WM. M. CHAMBERLAIN said that he had recently been practising tamponing of the os uteri with a sponge or sponge-tent, instead of merely filling the vagina with cotton; and would like to ask, not whether the plan was new, for that it was not, but whether it was a practice generally accepted as advisable.

DR. HUNTER said that it is recommended in Barnes's work on "Obstetric Operations."

DR. BARKER said that it had been taught for twenty-five years in New York.

DR. THOMAS said that he had employed it for a long time, and asked whether there was not danger of septic infection from a sponge-tent.

DR. CHAMBERLAIN said that he always used the carbolized tent, with which he thought there was no danger.

DR. MUNDE said that a very convenient and efficient tampon, both for the vagina and cervix (first suggested to him by Dr. Harwood Wakeman, of this city), is a simple roller bandage of a size to suit the case, which is introduced through a large cylinder speculum, or with Sims's speculum, and the centre rollers pushed forward by the finger into the gaping os. It should not be too tightly rolled, of course. It can be removed by merely seizing the end of the roller and drawing it out.

DR. HANKS said that he had never tamponed in his life, although he had seen very many cases of miscarriage during the earlier months. He had always been able in case of need to dilate the cervix with Barnes's dilators or the finger, and detach the whole ovum or placenta before leaving the house.

DR. FORDYCE BARKER read a paper on

VAGINAL HERNIA, OR ENTEROCELE,

based on a case sent him by DR. CLEMENTS, Surgeon U. S. A.¹

DR. NOEGGERATH said that in his long gynecological practice he had met with only two cases, really two and a half, of this affection; that is, two cases of complete enterocele and one case in which only a portion of the rectum had passed into the vagina and was covered merely by mucous membrane, the muscular fibres having separated. This last case caused more distress than the other two. As for the anatomical relations, they have been but little described. In one case he thought that the mucous membrane had also been ruptured, and that the hernial tumor was covered only by peritoneum; this patient had been confined nearly a year previously. He is surprised that this accident is not more frequently met with, as its occurrence is certainly favored by the normal inclination of the pelvis. He thought Dr. Barker's paper one of exceeding interest and importance, as it contained data which, to his knowledge, had never before been collected.

¹ Already published in June number.

CASE OF TRUE PUERPERAL DIPHThERIA.

DR. M. A. Pallen said that he had just come from a case of puerperal diphtheria, which he considered a very interesting one. About two weeks ago he delivered a woman of a healthy child with the forceps for inertia uteri. Mother and child did well, and were returned to the care of the attending physician. Dr. Pallen heard nothing from them until this morning, when he was sent for to see the child. On his arrival he found it dead from diphtheria, which had been diagnosed only twenty-four hours before. The mother, who was up and had been nursing the child, apparently presented no other symptoms than a feverish, excited manner, which was attributed to grief at the sudden loss of her child. This evening he was called again and found the woman moribund. Her whole vulva and the vagina, as far up as he could see through an ordinary bivalve speculum, were covered with diphtheritic membrane, the first appearance of which had been noticed at 11 A. M., soon after his morning visit.

DR. LEE said that this case was an exceedingly interesting one, as an example of the extreme rapidity of diphtheritic toxæmia. He would ask Dr. Lusk whether there were any diphtheritic genital deposits during the epidemics of puerperal fever at Bellevue Hospital.

DR. Lusk said that there were some such, but only on the genital organs, and no child or attendant was attacked.

DR. NOEGGERATH said that he was convinced the time would come when we shall distinguish sharply between diphtheria and the genital deposits called "diphtheritic." Dr. Pallen's case certainly is one of *true genital diphtheria*. But in hospital wards where puerperal fever rages, with so-called diphtheritic genital deposits, no child or attendant is ever attacked. Would the same be the case in a ward occupied by diphtheritic children? Assuredly not. We never see the ordinary sequelæ of diphtheria—paralysis or nephritis—after so-called puerperal diphtheria, the pathological conditions of which latter are entirely different from those of true diphtheria.

DR. Lusk said that he had quite recently been informed by letter from Strasburg that Prof. Von Recklinghausen declares the microscopic appearances of true diphtheritic membrane in the fauces and so-called diphtheritic deposits on the genital organs to be identical.

*Stated Meeting, April 4th, 1876.**The President, DR. T. G. THOMAS, in the chair.*

DR. S. BEACH JONES reported the following

CASE OF ACUTE PUERPERAL SEPTICÆMIA TREATED WITH INTRA-UTERINE INJECTIONS OF CARBOLIC ACID AND WATER—RECOVERY OF PATIENT.

During the summer of 1874, a patient presented herself at the Clinic for Diseases of Women, at the College of Physicians and Surgeons, with the following history :

Mrs. —, born in the U. S., at present residing in New York, aged 34 years, had been married but a few months. During the last two months, her menses, which previously had been very regular, had not appeared. She was suffering from digestive difficulties, especially nausea and vomiting, which were markedly more troublesome in the early part of the day. On examination the left breast presented the early signs of pregnancy, the right one was almost wholly obliterated—the nipple entirely so ; the effect, as she stated, of a very severe scald from boiling water, received in childhood several years before. Examination of the uterus confirmed the opinion already formed, that the patient was advanced in pregnancy.

Being apprised of the state of affairs, she expressed a good deal of surprise, and said that she had been informed by her family physician in the country, who had presided at her birth (as well as by non-professional persons,) that such an event would be impossible, because she had been subject to a periodical hemorrhage from the vulva during the first month of her life, which recurred for several successive months at regular intervals of four weeks, and lasted for a few days at each appearance. The condition of her right breast, and her mature age, gave her considerable solicitude at the prospect of becoming a mother. Some time after my first seeing her at the Clinic, she consulted me at my office, and requested me to attend her in her expected confinement, which I agreed to do.

On the 4th of May, 1875, I was called to attend her, and on arriving found that labor had begun. It proceeded without anything remarkable, excepting that it was a "tedious" one, and she was safely delivered of a healthy female child about 3.30 P.M. As soon as the placenta was delivered, and the uterus well contracted, before applying the bandage, I strapped the remains of the right breast firmly with strips of belladonna plaster, completely covering the cicatrized gland.

The milk came abundantly in the left breast at the proper

time, and from it she nursed her child; the tension and pain at first felt in the right one subsided after a few days without causing any further annoyance. The belladonna plaster was renewed once after the first strapping. I saw her daily until the eighth day, everything going on well, the lochia passing through the ordinary changes in color and quantity, and then told her I would see her again on the tenth day, after which it would be unnecessary for me to visit her. On May 14th (the tenth day after delivery), I saw her about 9 A. M., and found her complaining of abdominal pain and difficult micturition. The lochia were scanty and of a fetid odor, temperature 103, pulse 107, and irritable in character. I injected ℥vij. Magendie solut. hypodermically, applied flax-seed poultices to the hypogastrium (which was slightly tender), drew the urine with the catheter, washed out the vagina with warm water and carbolic acid, ordered her to be kept quiet and free from pain by morphine, and confined her diet to fluids. In about six hours I saw her again; she was feeling little or no pain, had no appetite, a dry tongue, slight tympanites, temp. 105°, pulse 114. The injection of carbolic acid and water was again repeated, the nozzle of the syringe being carried as high up as possible, the urine drawn by the catheter and ℥vij. Mag. sol. again injected hypodermically. At 9.30 P.M. the temperature was 104.5°; pulse, 108; the same treatment was continued, and directions left to keep her quiet with the morphine, p. r. n.

I went home and was about to retire, when I received an urgent summons to return immediately. Upon reaching my patient I learned that soon after I left her she had been seized with a chill which lasted about half an hour, and was so violent that she shook the bed on which she lay. She was in a stupor, from which she could only be roused by a loud voice to answer questions with intelligence. She said she suffered no pain, and was "comfortable." The face was flushed, the pupils natural, no particular tenderness over the abdomen.

I placed the thermometer in the axilla and counted the pulse, which was beating at 150 per minute (counting round the full minute twice, to insure accuracy). It was now 11.40 P.M. I immediately gave her ʒi. of brandy, with an equal quantity of water, and examined the thermometer, which showed a temperature of 107.3°. Taking a flexible male catheter, and attaching it to a Davidson syringe, I carried it up to the fundus uteri and thoroughly washed out the cavity with a solution of carbolic acid in water, at a temperature of 99°. The odor of the fluid washed out was not especially fetid, and was mingled with some bright red blood. I stated to the friends the alarming condition of the patient, and told them that I thought she would not live

until the morning, at the same time asking if they desired a consultation, which they declined, and left the management of the case in my hands. At 12 midnight (twenty minutes after the first dose of brandy, and less than that after finishing the washing out of the uterus), the pulse had fallen fifteen beats per minute. $\frac{5}{8}$ ss. brandy was again administered. At 12.35 A. M. (May 15th) the pulse was 123 and the temp. 104.1°.

At 1.35, pulse, 115; temperature, 102.8°; patient sweating profusely.

At 2.15 A. M. I left her with a pulse of 115 and temp. 102.6° without pain or any discomfort. By 9 A. M. the same day I again saw her—pulse, 105; temp. 101.8. Had spent a quiet night, sleeping (without any opiate since 9.30 the previous evening) nearly all the time, and she remained all day in about the same state, with the exception of a rise in the temperature to 103.1°, with a pulse of 116 in the evening. Milk punch was given in moderate quantity throughout the day, and the vaginal injections continued. On the morning of the 16th (12th day after delivery and about 36 hours after the chill) the temperature was 99.5° and pulse 80.

From this time she continued steadily to improve, and her case presented nothing further that was remarkable. Both mother and child did well; the patient not long afterward left New York to reside in one of the neighboring cities, where, with the exception of an ulceration of the cervix uteri, which caused some pain in the back, and difficulty in going upstairs, she regained her usual good health.

All the observations of pulse, temperature, and treatment are transcribed from notes taken at the bedside at the moment. The thermometer was carefully tested to determine its accuracy.

FÆTUS PAPYRACEUS.

DR. HENRY F. WALKER showed a specimen from the practice of Dr. Wm. H. Van Wyck, who had attended the lady from whom the specimen came in a perfectly normal confinement six weeks previously. On examining the membranes, a thickened portion was noticed, which was taken for calcareous deposit. On more careful examination, however, the supposed deposit was found to be a fœtus papyraceus, the embryo, of about $2\frac{1}{2}$ months, being flattened out to the thickness of $\frac{1}{8}$ ". The umbilical cord extended to the place of insertion of the entirely atrophic placenta. The membranes were attached to the vertex of the fœtal head. The atrophied twin was situated nearly opposite the normal full-grown placenta on the uterine surface of the decidua reflexa.

DR. WALKER then demonstrated the specimen of a uterus still containing the placenta, and read the history of a

CASE OF TRUE DIPHTHERIA OF THE CERVIX UTERI,

which he had observed in the practice of Dr. Van Wyck:

I first saw Mrs. M. with Dr. Van Wyck, of this city, on Thursday, Jan. 27, 1876. The history of the case is in part the statement of symptoms observed by Dr. Van Wyck, and in part a narrative of the case under our common observation.

Mrs. M., aged thirty, was pregnant for the third time, and had advanced to the commencement of the ninth month without unfavorable symptoms. In both of her previous pregnancies there had been renal trouble with uræmic poisoning. In the first, the child was born dead at full term, after several convulsions on the part of the mother, who remained unconscious for forty-eight hours after delivery. In the second, there was albuminuria, anasarca, uræmic headache, and premature expulsion of a dead fœtus at $7\frac{1}{2}$ months. Dr. Van Wyck had watched the patient with care during her third pregnancy, but she presented no symptom of uræmic trouble. He was called to see her on Wednesday, January 19, 1876, and found her complaining of a sense of pressure, or of uterine descent. He examined digitally and found the head of the child presenting, the cervix dilated to the size of a Spanish dollar. The patient had had no pains, and no mucous or aqueous discharge. Dr. Van Wyck sent her to bed, and ordered perfect rest in the supine position, and prescribed morphia. With this relief from downward pressure, the cervix uteri regained its tone, and next day all symptoms had ceased; the cervix would barely admit the distal phalanx of the finger. Dr. Van Wyck did not see the patient again till Tuesday, January 26th. At that time the patient complained of distress referred to the womb and vagina, of much bearing-down pain, and of incontinence of urine. Her temperature was between 99° and $100\frac{1}{4}^{\circ}$ during the day, and her pulse from 110 to 115. Examination by the finger showed the cervix greatly thickened and apparently impacted between the contained head of the child and the pubic bones. There was no dilatation, however, of the os. The cervical walls seemed at least an inch and a quarter in thickness. Dr. Van Wyck ordered the patient to lie with her hips raised on pillows, to make the child gravitate toward the fundus uteri, and gave suppositories of morphine every three hours. The next day he found the condition of the cervix unchanged. With the speculum it appeared of a deep venous red color, which was the color also of the vaginal walls. These were œdematous,

and so markedly so that there seemed to be a cystocoele. The tint was like that seen in the cases of suppurative tonsillitis. Besides this peculiarity of color, and the appearance of œdema, there was a white patch of exudation on the posterior lip of the womb, which he regarded at the time as diphtheria. Dr. Van Wyck applied a single leech to the cervix, which drew well, and in part relieved the appearance of congestion. On Thursday, the day following, I saw the patient, with her physician; the whole cervix uteri was covered with a patch of membrane, which extended a little upon the vaginal walls. This was not very firmly attached, and could be removed without leaving a bleeding surface. The vagina was œdematous, but not quite so deep a red as on the day previous. It looked as though affected by erysipelas, brawny. At this visit I heard the foetal heart. The mother's strength seemed good, her temperature 100° , her pulse 112. She took nourishment quite well. Pledgets of lint saturated with carbolyzed glycerine were kept applied to the cervix, the patient was kept in bed, morphia was given as required, and brandy ordered. I did not see her again till Saturday.

On Friday A. M., pulse and temperature about the same as previous day. Friday evening, temperature $99\frac{1}{2}^{\circ}$, pulse 150, which Dr. Van Wyck regarded as due to excitement from having too much company during the evening. Local appearances were the same, but the patient showed marked diminution of strength. Dr. Van Wyck listened, but did not hear the foetal heart. The mother now felt confident that she perceived foetal movements. Saturday, 11 A. M., Dr. Van Wyck found her with pulse 110 to 115, temperature $100\frac{1}{4}^{\circ}$, apparently better than the day previous. When he saw her at $4\frac{1}{2}$ P. M. he found her moribund. I saw her again with him at 8 P. M. She seemed to be dying of blood poisoning inducing asthenia. She died at half-past eight; and, as she expired, Dr. Van Wyck performed ventro-hysterotomy and delivered the foetus. It had evidently died before the mother, as the skin was detached from the limbs in the efforts used. The uterus was removed for preservation. The membrane seemed not to extend into the canal, but only to involve the cervix. The case I take to be one of diphtheria, destroying by its toxæmic effect, and making its local manifestations in an unusual place.

Dr. CHAMBERLAIN questioned the propriety of applying a leech to the cervix, as was done by Dr. Van Wyck in this case to relieve inflammation, as the bite of the leech was likely to be followed by engorgement and parenchymatous inflammation peculiar to the wounds inflicted by that animal.

Dr. REYNOLDS maintained that the leech had done good in

relieving the congestion, and he could not see what harm it could have done.

DR. WALKER thought that systemic infection from the abraded surface might take place.

DR. WINSTON asked whether physicians visiting diphtheritic patients should attend cases of labor at the same time.

DR. THOMAS thought that it would certainly be careless to visit a labor case immediately after a diphtheritic patient. He also considered the application of a leech to a diphtheritic cervix rather hazardous practice.

DR. NICOLL asked whether leeches to the cervix would not excite uterine contractions.

DR. MUNDÉ said that he had seen one case during the first months after he commenced paying special attention to gynecology, where the patient was sent to the clinic in Würzburg with the diagnosis of "chronic metritis." Accepting this diagnosis without question, and pregnancy not being surmised, four leeches were applied to the cervix, and the patient sent home the following day. A few weeks later, she returned, saying that labor-pains had commenced almost immediately after her return home (a distance of not more than three miles), and that she had speedily miscarried at three months. That this miscarriage was induced by the irritation caused by the leech-bites and their suction, seems highly probable.

DR. T. G. THOMAS read the history of a

CASE OF DERMOID CYST OF THE OVARY

removed by him at the Woman's Hospital ten days previously:

Mrs. Margaret Oertel, æt. 39. Married 22 years. Twelve children, seven abortions. Youngest child seven years old. Sick seven years. First menstruated when 14 years old—always regular, and had no pain. Seven years ago noticed a "lump" in her right side, about the size of a pear. Says she was nauseated most of the time, and had all the symptoms she usually had when pregnant. She increased in size slowly, and at the usual time "felt life." She was told at this time by a physician, who examined her, that she surely was pregnant. Eight months later, while exerting herself, a large quantity of water came away by the vagina, and for the next twenty-four hours had bearing-down pains, as in labor. Says she was very much smaller after this, and could feel the growth in the right side more distinctly. Since then the growth has been very slow. Her general health has remained very good. Says she has had con-

siderable pain in the right hip. Menstrual periods have been more frequent of late, coming now once in three weeks.

March 16.—Small amount of yellow fluid drawn from tumor with hypodermic syringe, and found to contain fat and epithelium in large quantities.

Diagnosis: *Dermoid Cyst of Ovary.*

Operation, March 25th. Time, from first incision till tumor was removed, 8 minutes; till insertion of last suture, 17 minutes; till patient was removed from the room, 22 minutes. Weight of tumor, 5 lbs.; fluid, 4 lbs. Patient had, previous to operation, quin. sulph., gr. x., morph. sulph., gr. $\frac{1}{4}$, every night for three days, also pil. cathart. co. No. 1, every eight hours.

Pulse and Temperature after Operation.—March 25—7 P.M., pulse 120, temp. $99\frac{3}{4}$; 11.40 P.M., pulse 96, temp. $100\frac{1}{4}$.

March 26—6 A.M., pulse 100, temp. $100\frac{1}{2}$; 10.10 A.M., pulse 100, temp. $102\frac{1}{4}$; 11.10 A.M., pulse 120, temp. 103; 2 P.M., pulse 128, temp. 104; 3 P.M., pulse 128, temp. $104\frac{3}{4}$, ice bag applied; 5.15 P.M., pulse 120, temp. $104\frac{3}{4}$; 6.56 P.M., pulse 120, temp. 104; 9 P.M., pulse 120, temp. $103\frac{3}{4}$; 10 P.M., pulse 120, temp. $103\frac{3}{4}$; 12, pulse 120, temp. 104.

March 27—1 A.M., pulse 120, temp. $104\frac{1}{4}$; 12 noon, pulse 130, temp. $104\frac{1}{2}$; 2 P.M., pulse 138, temp. $104\frac{3}{4}$; 7 P.M., pulse 152, temp. $106\frac{1}{4}$; 8 P.M., pulse not countable, temp. $105\frac{1}{2}$. Died 9.36 P.M.

Patient was kept under the influence of opium as much of the time as possible after the operation.

Autopsy. Cause of death, Peritonitis.

DR. DAWSON asked whether some of the fluid did not enter the abdominal cavity and cause the rapidly fatal peritonitis? Would not intra-peritoneal injections have been beneficial?

DR. THOMAS said, that 20 hours after the operation he opened the abdominal cavity, passed a catheter down to the bottom of Douglas' cul-de-sac, and withdrew barely 20 drops of fluid, showing that there was nothing there to wash out.

TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

Reported by W. H. H. GITHENS, M.D., Secretary.

Stated Meeting, January 6th, 1876.

DR. J. L. LUDLOW, *in the Chair.*

The following officers were elected for 1876 :

President, Dr. John S. Parry.

Vice-Presidents, Dr. J. L. Ludlow, Dr. Ellwood Wilson.

Secretary, Dr. W. H. H. Githens.

Treasurer, Dr. D. Murray Cheston.

Curator, Dr. W. H. Parish.

Publication Committee, Dr. Wm. Goodell, Dr. John H. Packard, Dr. James V. Ingham, Dr. Roland G. Curtin.

Council, Dr. Lewis D. Harlow, Dr. H. Lenox Hodge, Dr. R. P. Harris, Dr. Chas. H. Thomas.

Stated Meeting, February 3d, 1876.

DR. J. L. LUDLOW, *Vice-President, in the Chair.*

DYSMENORRHEA FROM GOUTY OR RHEUMATIC DIATHESIS.

DR. J. L. LUDLOW made some remarks in reference to a form of dysmenorrhœa not depending upon any malformation of the uterus, or deviation or change from the proper position, but upon a gouty or rheumatic diathesis.

These cases may not show any evidence of the disease at the time, but on tracing back their history, there is found evidence of this gouty or rheumatic diathesis. He instanced the case of a patient who had been for a long time quite healthy. There was no mechanical obstruction. She suffered intensely with her menstrual periods, the uterus became engorged and tender, and the flow was clotted in character. He placed her upon treatment intended to relieve the gouty tendency, anticipating the period by eight or ten days.

During the period he found apiol capsules valuable, preceding them for days by the use of iodide of potassium, wine of colchicum, etc.

In cases depending upon deviations of position of the uterus, he found most benefit from the use of the apiol capsules, using two or three a day.

He had been surprised, some years ago when at Capon Springs, in Virginia, which enjoy a reputation for gouty disorders, at the number of ladies who came suffering from uterine disorders. They would come feeble and unable to walk, but would improve gradually in health and finally go home well. In these cases he attributed the favorable result to the beneficial effects of the water upon the rheumatic trouble which existed.

Guaiaecum was a favorite remedy of Dr. Dewees in similar cases.

Dr. A. H. SMITH remarked that he had used apiol capsules with good results. The beneficial action when it occurs is generally prompt.

When the patient feels the twinges, showing the effort of nature to establish the flow, the capsules will bring it on in about three or four hours.

He regarded apiol as the only true emmenagogue. He knew nothing else which would produce the desired effect so free from collateral influences.

If it does not produce its good effects speedily, it probably will not at all.

He gives a capsule containing π v. every three hours; usually from twelve to fourteen hours will be sufficient, if there is any effort on the part of nature to establish the flow.

He had very rarely found dysmenorrhœa attended with organic trouble which would not yield to radical measures.

Dr. LUDLOW remarked that in the case cited he had given apiol alone without any effect.

Dr. W. T. TAYLOR asked whether the lady was married or not. Had Dr. Ludlow met with cases in married ladies after the period of child-bearing had commenced?

Dr. LUDLOW replied that the lady was unmarried. He had seen many cases in married ladies.

Dr. A. H. SMITH remarked that Dr. Taylor's question reminded him of the case of a married lady, twenty-seven or twenty-eight years of age, who has suffered intensely from dysmenorrhœa since she was sixteen years of age. She began menstruating at fourteen. At seventeen she was attacked with symptoms of uterine inflammation. Since that time she has passed masses of membrane. At periods of time lasting from five or six days to two weeks in duration, she suffered from in-

tense agonizing pains. The amount of membrane passed would vary according to the length of the period of pain. If only a few days, a small amount was expelled; if two weeks, a larger mass came away.

On one occasion, he found a large mass which looked like an abortion, but the lady assured him that it could not have been, for she had menstruated six weeks previously, and there was no subsequent opportunity for pregnancy to occur. Before her marriage she had also passed a mass quite as large.

He gave the mass to Dr. Jenks, who assured him that it was placental tissue, but the lady insisted that it was of just the same character as the mass passed before marriage. It had the appearance of a mass thrown off from the cavity of the uterus.

In this case he has found that if the patient takes an apiol pearl as soon as she feels the pain of the menstrual nismus, she passes no membrane; if, however, she allows the pain to come on, the membrane comes away in masses.

Apiol always arrests the pain and the formation of membranes. The flow passes away as an ordinary discharge. The action of apiol in this case is very striking. If it is given at the beginning of the nismus, it brings on menstruation quickly without pain or membrane.

DR. PARISH gave the history of a young lady whom he had treated for dysmenorrhœa. Her father was of a marked gouty disposition, with concretions in his joints. The lady had been treated by mechanical means for an ante flexion of the uterus, and was under local treatment for the dysmenorrhœa. Upon the administration of iodide of potassium and guaiacum, the patient's condition improved so much that she refused further local treatment.

Stated Meeting, March 2d, 1876.

DR. J. L. LUDLOW, *Vice-President, in the Chair.*

VARICOSITY IN THE FEMALE SEX.

DR. J. L. LUDLOW referred to a subject which, he remarked, was apparently trifling, but really the cause of much discomfort to a class of people who are obliged to stand at their work.

These people complain of what they call rheumatism, but the trouble is really caused by a condition of varicose veins. These veins are small in size and deeply seated, and can be felt as prominences by passing the hand over the limbs. Small in amount as they are, they give rise to a great deal of irritation.

In females he has noticed this condition causing great aching

of the limbs, especially in getting about after a confinement. He generally orders in these cases bandaging of the limbs and rest.

In some of these patients the veins are not positively varicose, but have a tendency to assume the varicose condition.

DR. GOODELL remarked that he had met with some of these cases, which refuse to yield to treatment.

Ergot well administered will sometimes act beneficially by its effect upon the muscular coats of the vessels. He thought this condition was sometimes due to sub-involution of the uterus, causing it to press upon the veins, thus producing the varicose condition or capillary enlargement resembling the small marks sometimes seen upon the abdomen.

DR. H. LENOX HODGE remarked that there is another cause, sometimes of a nervous character, for the aching pains in the limbs. Frequently, in cases of misplacement of the uterus, there is an aching which is referred to the lower extremities; whereas the true seat of the cause is really in the spinal cord, or even in the brain. In these cases, therefore, the pain is relieved by treatment directed to a part distant from its apparent seat.

DR. LUDLOW remarked that the cases he referred to were those of internal varicose veins.

PUERPERAL CONVULSIONS.

DR. W. T. TAYLOR related the history of a case of puerperal convulsions in a young woman seven months pregnant. When he first saw her, she was completely anasarcons, head, body and limbs. She had frontal headache, etc., and albumen was found in her urine. Diuretics and other appropriate remedies were given with apparent good effect. In the evening she fell into convulsions. Five or six Swedish leeches were applied to the temples, and venesection was attempted, but without success. In the course of two or three hours she became partly sensible. Ether was then administered, and finally the convulsions ceased. She was better next day, and in a week was well. She has since been delivered safely of a dead fœtus of seven months' development.

He narrated this, because in similar cases the induction of labor is recommended generally. In this case he did not think it necessary, and he had a favorable result.

DR. GOODELL remarked that the question is a very difficult one to answer, and must be decided by each physician for himself by the necessities of the moment. In cases like the one mentioned, where there is no uterine action, the physician is to be guided by the effect of his treatment upon the convulsions.

The last five cases which he had seen, in which there was an opportunity to administer medicine, hydrate of chloral acted wonderfully. In two of them 3 i. given by the rectum acted perfectly. In the third case 3 ij. were necessary, 3 i. not controlling the convulsions perfectly. In the fourth and fifth cases the convulsions were absolutely controlled by the drug. It is a valuable remedy in cases in which there is no apoplectic complication.

Stated Meeting, April 6, 1876.

DR. J. L. LUDLOW, *Vice-President, in the Chair.*

DR. LUDLOW announced the death of Dr. John S. Parry, late President of the Society, on March 11th, at Jacksonville, Fla.

DR. ISAAC E. TAYLOR'S NARROW-BLADED FORCEPS AND CRANIOCLAST.

DR. WM. GOODELL exhibited to the Society the narrow-bladed forceps of Dr. Isaac E. Taylor, of New York.

This instrument can be applied, on account of its narrow blades, to cases where it is necessary to deliver while the os uteri is but partially dilated. Also, where the head is so high up and so oblique that it is impossible to carry one blade of the forceps as high as the other, the instrument may be locked temporarily by making use of additional holes on the female blade for the insertion of the pivot. When the os is sufficiently dilated and the head brought down, the blades may be readjusted and the pivot placed in the middle hole.

DR. GOODELL also exhibited the cranioclast of Dr. Taylor. By its use the bones of the base of the skull may be crushed, which cannot be done with the cephalotribe.

DR. LUDLOW commended the use of the old colpeurynter in effecting the dilatation of the os uteri, as it stimulates the action of the natural bag of waters. He preferred it to other modes of dilatation.

PLACENTA DIVIDED INTO TWO PORTIONS BY A MEMBRANOUS SEPTUM.

DR. ELLWOOD WILSON presented the placenta of a patient, with the following history:

On March 30th he was called to see a lady in the eighth month of gestation. She exhibited no special signs of distress, no hemorrhage, but was anxious on behalf of a sick child. He saw her again on April 1st, at 3 P.M. She was seized with frequent sharp pains. The vagina felt cool and moist, and the os

was dilated slightly. On careful examination he felt the margin of the placenta, and passing the finger through a mass of clots, he felt beyond what seemed to be the membranes. Piercing these, there escaped the amniotic liquor tinged with blood.

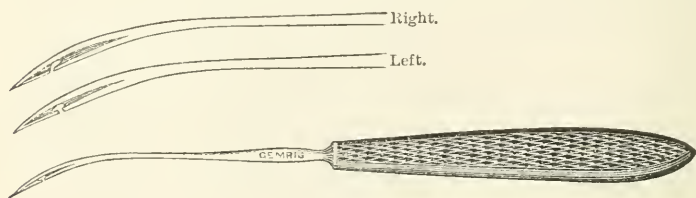
To stimulate the uterus and arrest hemorrhage, he gave ergot, and brought the head down upon the placenta, and when ergotism was fully produced, he applied the forceps and delivered the child. There was a complete placenta prævia, but the placenta was separated into halves, one to the right and the other to the left of the os, and connected by membrane. It was this membranous connection that he had punctured, and through which the child was born.

DR. ESHLEMAN remarked that he believed cases of placenta prævia were more common than generally supposed. He thought that very many cases of abortion attended with great hemorrhage were really cases of placenta prævia, either partial or complete.

In reference to cases of slow dilatation of the os uteri, he liked the use of the colpenrynter, but not so much as formerly. He thought the child's head was the best dilator, and had long been in the habit of applying the forceps within the uterus, and dilating the os by gentle traction, being careful to guard the tissues from injury by shielding the blades with his fingers.

NEEDLES FOR IMMEDIATE PERINEORRHAPHY.

DR. REUEL STEWART exhibited two needles which he had devised for the purpose of sewing up lacerations of the perineum



occurring during labor. The peculiarity of the needles is in the notching of one edge of each needle. The needle first introduced has its edge notched $\frac{1}{2}$ inch from the point by a slot extending backward toward the handle of the needle. The ligature, being placed in the slot, is carried by the puncture through one side of the torn tissue.

The second needle is now introduced through the opposite portion of the torn tissue. It is notched by a triangular notch,

the apex being at the edge, and the other angles running acutely, one toward the point and the other toward the handle of the needle. This needle, being introduced, crosses the first needle, and carries the loop of the ligature from the slot by its forward motion, the loop falling into the lower angle of the triangular notch. On being withdrawn, the loop slips into the upper angle, and is brought out with ease.

The Secretary then read the following case of

POST-MORTEM CÆSAREAN SECTION—DELIVERY OF A LIVING CHILD
TWO HOURS AFTER THE DEATH OF THE MOTHER. BY DR. P. A.
VEROUDEN, FERBORGH, NETHERLANDS.

To the Philadelphia Obstetrical Society.

In the *Physical Gazette* of 6th of February, 1876, I read a passage taken from the AMERICAN JOURNAL OF OBSTETRICS, in which it was mentioned, that Dr. Kelly, in the session of the Philadelphia Obstetrical Society of 3d June, 1875, has communicated the delivery of the dead mother of a living child.

It will certainly not be disagreeable to your honored society to possess more observations of that kind, and therefore I have the honor to communicate the following:

A certain woman, named M. Peters, dwelling at Ulft community, Gendringen, at the distance of an hour from my dwelling, was treated by me; she suffered of a pulmonary consumption. She was thirty-five years old, mother of several children, and in the sixth month of her pregnancy. There I posed the prognosis very dangerous; her spouse entreated me, that if his wife came to die, I would take care, as much as possible, that the child of which she was pregnant, could it not be saved, should receive at least the holy baptism.

Unexpectedly I received the news that the woman had suddenly died in an attack of hæmophthæ.

At my arrival two hours after the death of the woman, I perceive still distinctly with the stethoscope the tones of the heart of the fetus. The death of the woman, of which was not to doubt here, was not yet exactly affirmed, and then the corpse was laid on a pallet-bed; and now I did *lege artis*, as if I *in vivo* operated, the *Sectio Cæsarea* by the abdominal section.

I had the satisfaction to give birth to a living fœtus, which was yet not six months of age. It was brought prudently to the parish church, received the holy baptism, and lived still several hours after that ceremony.

A perpetual memory is made up, on account of the singularity of the fact, a piece, which is found in the archive of the

church government at Ulft, decanery of Groenlo, province of Guelderland, in the Netherlands.

P. A. VEROLDEN,

Member of the Physical Council for the Provinces Guelderland and Utrecht, etc.

FERBORGH COMMUNITY, Wisch Prov., Guelderland, in the Netherlands.

DR. WM. T. TAYLOR then read a history of a case of

METRITIS FOLLOWING LABOR.

About the first of this year I was engaged to attend a carter's wife, residing in a house which might be called a tenement, for a German barber and his family used the front part for shop and dwelling, whilst my patient and her husband occupied two rooms in the back part of the same. Her bedroom was situated over the room which she used as kitchen, dining-room, and parlor, and was heated by a stove-pipe coming through the floor, from the aforesaid room.

She was pregnant with her first child, and her gestation was completed on February 25th, when I was called to see her about 11 P. M. On examination I found the os uteri very high up in the pelvis, small and not dilatable; nor was there any "show;" the pains although severe, would move up and down the back, preventing her from sleeping. Having given her an anodyne I left her, and found her very comfortable on the following day, but at midnight I was again summoned, when labor had commenced. Several hours elapsed, however, before I could diagnose the position of the child, which was a vertex, with the occiput presenting anteriorly in the second position of Baudelocque. As the pelvis was narrow and the promontory of the sacrum encroached on the pelvic cavity, the head descended very slowly.

About 5 A. M., as the expulsive pains were weaker, and the woman getting tired, I concluded to apply the forceps. Previous to doing so, I told the parents that there was some risk to the child's life, when they requested me to baptise it. Having performed that ceremony, the woman was placed on her back and brought to the edge of the bed; her knees drawn up and supported by two of her friends. Having oiled my left hand, I introduced it through the os and slipped on the female blade of the forceps over the right side of the child's head, the fenestra covering the portal bone; in a similar manner I introduced the male blade on the left side of the head over the parietal bone, pressing under the blade the upper part of the left ear.

Having the head firmly within the grasp of the forceps I locked them, and lashed them together with a towel, so as to produce some compression, and obtain a better hold of the handles; when, during a pain, I made traction and rested during its cessation. Bracing myself firmly against the bedstead I drew with *all* my strength. The forceps were not kept lashed during the whole time they were applied to the child's head. In about twenty minutes the head was drawn slowly and safely through the pelvic cavity. As it reached the perineum, I removed the instruments and assisted the passage through the soft parts, with my hands supporting the perineum as was taught me by Professor Hodge. The child, a female, was delivered at 5½ A.M., and the placenta easily removed by drawing gently on the cord and twisting it as it advanced, at the same time placing my left hand on the abdominal uterine wall, and squeezing it down towards the pubes.

The delivery occurred on Sunday, Feb. 27th; and before leaving I told her sister (for she had no nurse) to darken the room, keep it quiet, and allow her to sleep. About noon I found she had urinated, her skin was warm and moist, her pulse 78—she had no after-pains, but the lochia were scanty. She had slept a little and was quite comfortable.

At 4 A.M. on Monday, 28th, I was summoned from my bed to visit Mrs. F., who was suffering great pain over the pubic region, with tenderness on pressure, having a hot dry skin, great thirst, quick respiration, and a pulse of 130, and no lochial discharge. On inquiry I learned that she had been visited during the day by some of her friends, that some whiskey had been drank in honor of the arrival of the young heiress, and that a general jubilee had been kept up until near midnight; when she was seized with a chill, followed by pain in the abdomen. They applied warm cloths to her, but as I had lost my sleep on the previous night, they did not send for me until the time stated. I had the abdomen bathed with a liniment of turpentine, sweet oil and laudanum; then covered with a thick layer of raw cotton, and a bottle of hot water applied to the pubes. An anodyne febrifuge was given, and at my next visit she was better, for the skin was moist, her pulse 100, and the pain relieved. I ordered some milk to be drank, with oatmeal gruel for nourishment, and was well pleased with her condition. But at 2 A.M. of Feb. 29th, I was again summoned to her bedside. She had had a severe chill, followed by a fever; for her skin was again hot and dry, her pulse 140, her respiration quick, her abdomen tender on pressure and tympanitic. The lochia were arrested, and she was in great pain, so that her condition appeared quite serious. An injection of castor oil and turpentine was given, which

brought away a copious stool. A dozen foreign leeches were applied to the abdomen over the uterus; and as it was difficult to obtain mush poultices, a thick layer of raw cotton was put on the bleeding surface to keep it warm, and bottles of hot water again applied to the pubes. A pill containing 1 grain of calomel, 1 grain of opium, and 2 grains of quinine, was given every two hours. During the day she took milk and beef-soup occasionally, with warm tea *ad libitum*. At 4 A. M. her pulse was 120, her skin was moist, the pains had subsided, and the tympanites had disappeared; but she had not slept. That night, however, she dozed occasionally for half an hour at a time. During the next 24 hours, the pulse fell to 100; the respiration became natural, the pains ceased, and the lochia became more profuse. I continued the quinine in 2-grain, and the opium in $\frac{1}{2}$ -grain doses every 2 hours, omitting the calomel. She had several evacuations during the night.

March 2d.—Continued the pills at longer intervals. The milk was secreted to-day, and the babe put to the breast, which it drew quite vigorously.

March 3d.—She rested well last night. Her pulse had fallen to 80; the lochia were still profuse, being increased by the raw cotton which was kept on the abdomen. Stopped the opium, but continued the quinine in small doses.

March 4.—Her pulse 76; respiration normal, pain and soreness all gone. From this time she improved very rapidly, and in another week was able to resume her household duties.

DR. DE FOREST WILLARD reported the following case of

SPURIOUS HERMAPHRODITISM, CAUSED BY COMPLETE HYPOSPADIA,
WITH CLEFT OF SCROTUM, IN A MALE INDIVIDUAL.

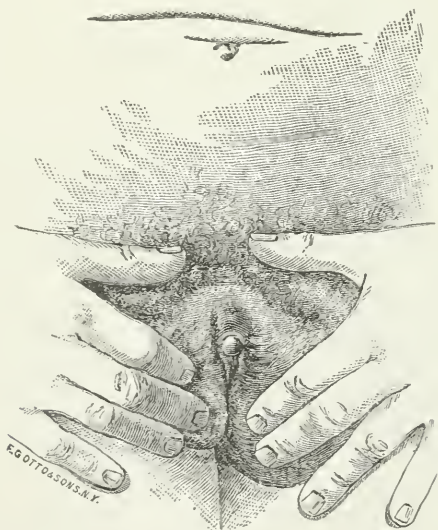
The person presenting this deformity, although an undoubted male, yet bore sufficient resemblance to a woman to pass as one for nearly thirty-five years of his life, during all of which time he lived, worked, and slept with females as one of their number.

Although at the present time no difficulty exists in regard to the determination of his sex, yet before attaining the age of 25 such a mistake might readily have occurred, from the fact that the testicles had, until then, occupied a position just below the external abdominal rings. At the present time, however, one of them has completely descended, and from the constant stretching to which the skin is subjected whenever the man separates the parts in urination, the left scrotum has become quite pendulous.

During his life as domestic, cook, waiting-maid, etc., in Germany, he discovered that his sexual desires were only

awakened by females, and with constantly offering opportunities he found, that being differently constructed from his companions, he was able to give a certain degree of satisfaction, both to himself and to them, the penis, during its erect state, becoming about an inch in length, and of the size of one's little finger. With this he was able to effect entrance, and he soon produced a discharge of semen at each coitus.

He remained in female apparel until the beginning of the civil war; when, desiring to enter the army, he donned male attire, but was rejected by the surgeon. Soon afterward he married one of his former companions, and remained with her for seven years, when she left him, seeking a divorce on account of his deformity. Has since continued as a man, working as cook, etc. Four years since, beard appeared, and has now a slight mustache, with thin chin whisker. Is over 45 years of age, has well-marked masculine features and voice, with robust strong frame and contour, full height and size and weight.



At first view, no signs of penis—a mons well covered with hair, which points upward at centre, but very slightly, toward umbilicus. The scrotum is entirely cleft, so as to gave the appearance of labia, while the upper commissure of these scrotal folds corresponds almost precisely with the similar commissure in the female. In these folds of skin upon either side lies a testicle, the left one hanging much lower than the right, and being of considerably larger size. The right one is soft,

and about two-thirds as large as an ordinary testis. Upon separating these plicatures a diminutive penis comes into view, its corpora cavernosa, and glans and prepuce all being present, but the corpus spongiosum is entirely wanting. The opening of the urethra is directly beneath the arch of the pubis, as in the female, requiring the pulling aside of the scrotum at each act of micturition. This opening will admit the little finger; and, from the infundibuliform conformation of the parts leading to it, would undoubtedly have been dilated by constant coitus to such extent as to be capable of receiving the male organ. The semen is discharged from this opening during his acts of cohabitation, but being so far from the extremity of the penis, is never thrown into the vagina of the female.

The glans is grooved upon its under surface, as are also the corpora cavernosa, this furrow being lined by a florid membrane, which constantly secretes a thin mucus, bathing the parts in moisture.

Folds corresponding to labiæ minora are present, lined by mucus-secreting follicles. The penis, when erect, protrudes as before mentioned. He states that his breasts at puberty were very much larger than at present. Has never had any menstrual disturbance.

The man evidently belongs to the first order of spurious hermaphrodites; that is, his general sexual configuration, from imperfect or abnormal development, approaches that of the female sex. The majority of cases of so-called hermaphrodites are simply females with an enlarged clitoris; but, in the male, this deformity occurs, according to Simpson, from three causes: extroversion of bladder; adhesion of penis to the scrotum; or, as in the case under observation, from hypospadiac fissure of the urethra, scrotum, and perineum. The clitoris of the female not unfrequently greatly exceeds in length the penis of this man, cases being recorded by Haller (*Él. Phys.* tom. vii. part ii. p. 81) and Arnaud (*Dissert. sur les Hermaphr.*, p. 372), in which its length was seven or eight inches; and Chabart mentions one, evidently pathological, twelve inches long.

Many cases are on record, however, where a hypospadiac cleft has given rise to such great deformity that even such astute diagnosticians have been arrayed upon opposite sides in their opinions as to the sex of the individual, (*vide Dissert. sur les Hermaphr.*, p. 298, Arnaud, and Gericht, *medic. Abhandlungen* Bd. i. S. 177.)

The existence of such a deformity is readily explicable when the manner of formation of these parts is studied. About the second month of embryonic life, a transverse division of the perineum begins to form and divides the cloaca into the anal cavity

behind, the uro-genital sinus in front. After a time the opposite sides of the cloacæ gradually approximate, and throw out folds which unite to form the pelvic portion of both male and female urethra. The process of median union, however, does not go on in the female, the primary perineal fissure remaining in vagina and vulva, while in the male the union proceeds further, the primitive halves of the penis joining inferiorly, in such manner as to form a tubular prolongation of the pelvic portion of the urethra along the under side of the penis.

To the arrest of this process may be ascribed the deformity under consideration, especially as a want of closure of this canal is often accompanied by other malformations, as diminutive penis, etc. Mythology, superstition, and credible history all furnish us with instances of these curious freaks—or, more properly, curious interruptions in development, the cause of which, as yet, is totally beyond our sphere of explanation. That it is not due to any maternal impressions may be reasonably believed, when it is remembered that similar conditions occasionally exist in the lower animals, who may reasonably be supposed to be free from such influences. (Gurlt.)

A mechanical explanation of the deformity is given by Oslander and Dugès (*Neue Denkw. für Aerzte und Geburtsh. Bd. i. p. 264*; and *Éphem. Méd. de Montpel., tom v. pp. 17 and 45*), who state that an imperforate urethra, together with a preternatural accumulation of urinary fluid, might cause a rupture of the delicately united sides of the perineal raphé, thus producing a large and permanently open fistula, corresponding in position to the orifice of the vagina, while the meatus urinarius would thus be thrown back beneath the arch of the pubis, or to some point considerably posterior to its normal position.

This man, although solicited, yet having no desires toward the male sex, would never permit approach; but cases are not wanting in which males have lived in wedlock for many years, occupying the station of wife. (*Observ. sur l'Hist. Nat. sur la Phys. et sur la Peinture, tom. i. p. 18.* Julien and Soules; and *Éphem. Nat. Curios. Dec. i. ann. iii. p. 323.* Sampson, etc.)

Hermaphrodisism is said to have been hereditary in a few instances, and authentic examples are cited by Hendemann, Lecat, Baum and others, (*Med. Beobacht. Bd. ii. S. 234*; “*De Fissuris urethræ virilis congen.*” p. 34, etc.) It is also more commonly met with in twins; and from this circumstance, in connection with the fact that “free-martin” cows are prone to sterility, has arisen the idea that a female born as co-twin with a male is liable to be barren. Cribb (*Lond. Med. Repos., Vol. xx. p. 213*) and Simpson have collected sufficient cases to show that, although these twin females are occasionally found without children, yet that it is by no means the rule.

The writer is cognizant of one female born as co-twin with a male who has three children; also, a female, one of triplets, the other two being males, who is sterile.

For an extensive bibliography and exhaustive account of the numerous forms of hermaphroditism, both true and false, the reader is referred to Simpson's posthumous papers, published in Edinburgh, in 1871, entitled "*Anæsthesia, Hospitalism, Hermaphroditism,*" etc.

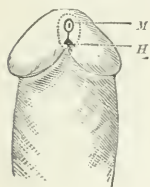
DR. JOHN ASHHURST, Jr., related a case of

CONGENITAL OCCLUSION OF THE URETHRA WITH HYPOSPADIA,

Successfully treated by a modification of Colles's operation for stricture at the meatus of the urethra—an operation recently revived by Mr. B. Wills Richardson, of Dublin, and likewise employed by Dr. Weber, a German surgeon, in cases of congenital narrowing of the part. Dr. Ashhurst's patient was a child nine months of age, and was seen in consultation with Dr. Horace Y. Evans, who had courteously furnished a history of the case. The child was born on April 29, 1875, and on the occasion of Dr. Evans's second visit, six hours afterwards, the nurse called his attention to the fact that the child had not urinated. Examining the parts, Dr. E. recognized an abnormal condition, and supposing that the lips of the urethra were simply adherent, endeavored without success to effect an opening by the use of a fine probe. On account of insufficient light, further manipulation was then postponed until the next day, twenty hours after birth, when the completeness of the occlusion was recognized, and a slight papular elevation noticed on a line with, but a little posterior to, the depression which marked the normal position of the meatus. By pressing upon this elevation, the probe entered, and its withdrawal was followed by the issue of a fine stream of water.

The daily use of the probe was required to maintain an opening sufficient to allow the complete evacuation of the bladder, the urine, when this was omitted, escaping merely by drops. The child was first seen by Dr. Ashhurst in September, 1875, when an operation was recommended, but temporarily postponed, in order to allow the child to be vaccinated, as several cases of small-pox had occurred in the neighborhood. Meanwhile, upon the suggestion of an officious friend of the mother, the child was taken, without Dr. Evans's knowledge, to the office of a prominent surgeon, who simply enlarged the hypospadiac orifice with a bistoury, and then dismissed the case, saying that nothing more could be done until the patient was at least seven years old. As might have been expected, this procedure failed to accomplish

any good purpose, the wound reuniting in about forty-eight hours, and the child then relapsing into his former condition, and passing water only *guttatim*.



M. Depression occupying normal position of meatus.

H. Hypospadiac orifice.

The dotted line represents the incision, and shows the extent of the corpus spongiosum removed by the operation.

On February 10, 1876, Dr. Ashhurst was again asked to meet Dr. Evans, and then proceeded to operate in the following manner. The child being firmly held (but not anæsthetized), one blade of a very delicate pair of scissors was introduced into the hypospadiac orifice, when by a single stroke the parts were divided to the depression which, as already mentioned, occupied the normal position of the meatus. With a very small knife an oval incision was then made through the skin of the glans penis, and a minute portion of the corpus spongiosum then cut away on either side of the newly formed urethral orifice. The operation was completed by attaching together, with numerous silk sutures, the skin of the glans and the urethral mucous membrane, so as to ensure the adhesion of these, and to prevent the closure of the meatus, which was thus made to gape. No dressing was required, save a rag dipped in glycerine and water, and changed when it became saturated with urine; the stitches were removed without difficulty after three or four days; and the child recovered without a single unfavorable symptom. Urination is now effected in a full stream, and the parts present no apparent deformity.

THE AMERICAN GYNECOLOGICAL SOCIETY.

A NUMBER of the prominent gynecologists of the United States met at the Academy of Medicine in New York, on June 3d last, and organized a "National Society for the Promotion of Knowledge in all that relates to the Diseases of Women and Obstetrics," to be called the "American Gynecological Society."

The invitation for organization was accepted by the following thirty-nine physicians, the majority of whom were present: Fordyce Barker, T. A. Emmet, W. T. Lusk, Paul F. Mundé, E. Nöeggerath, E. R. Peaslee, J. Marion Sims, Isaac E. Taylor, T. G. Thomas, New York City; John Byrne, J. C. Skene, Brooklyn; Jas. D. Trask, Astoria, N. Y.; J. P. White, Buffalo,

N. Y.; Ely van de Warker, Syracuse, N. Y.; Geo. H. Bixby, Chas. E. Buckingham, Jas. R. Chadwick, Geo. F. Lyman, Wm. L. Richardson, A. D. Sinclair, D. Humphreys Storer, Boston; W. L. Atlee, T. M. Drysdale, Wm. Goodell, J. V. Ingham, R. A. F. Penrose, Albert H. Smith, Ellerslie Wallace, Philadelphia; Wm. T. Howard, H. P. O. Wilson, Baltimore; S. C. Busey, Jos. Taber Johnson, Washington; Henry F. Campbell, Augusta, Ga.; Robert Battey, Rome, Ga.; George J. Engelmann, St. Louis; Wm. H. Byford, Chicago; Theophilus Parvin, Indianapolis; E. W. Jenks, Detroit; J. C. Reeve, Dayton, Ohio.

The following gentlemen were elected officers for the present year: President, Dr. Fordyce Barker; Vice-Presidents, Drs. W. L. Atlee and W. H. Byford; Secretary, Dr. James R. Chadwick; Treasurer, Dr. Paul F. Mundé; Councillors, Drs. Wm. Goodell, Geo. F. Lyman, T. Parvin, J. Marion Sims.

The meetings are to be held annually on the last Wednesday in September, and will continue during three days. The first meeting will take place in New York City on Wednesday, Sept. 13th next (exceptionally, as a convenience to the visitors to the International Medical Congress in Philadelphia).

We hail the new Society with sincere pleasure, and wish it all possible success and encouragement, of which the many eminent names among its founders certainly give sufficient assurance.

The formation of national associations by the followers of special branches of medicine is, in our opinion, a step in the right direction; and conducive not only to the advancement of the particular specialty, and consequently of medical science in general, but also promotive of friendly intercourse between individual members of the profession, both of which objects are but imperfectly attainable in so unwieldy and incongruous an assemblage as the American Medical Association. In no specialty are such annual meetings more desirable or more likely to be beneficial than in that of Gynecology, a branch in which America so avowedly takes the lead.

The first meeting promises to be one of exceeding interest. Papers by Drs. Barker, Battey, Bixby, Byford, Emmet, Johnson, Noeggerath, Parvin, Taylor, Thomas, and others are expected; and several topics of great importance will be brought up for discussion, such as the Value of Division of the Cervix Uteri, the Treatment of the Pedicle in Ovariectomy, etc.

The full reports of each meeting will be published by the Society in an annual volume of Transactions; we shall, however, give our readers a synopsis of the proceedings in each October number of this JOURNAL.

QUARTERLY REPORT ON OBSTETRICS, GYNECOLOGY, AND PEDIATRICS.

BY

DRS. D. B. HUNT AND C. WILLIAMS.

OBSTETRICS.

1. PROPHYLAXIS OF PUERPERAL FEVER. By PROF. BISCHOFF of Basel. (*Allg. Med. Central-Ztg. Fern.*, 1876.)

THE author believes that this disease, although presenting manifold symptoms, arises only from the infection of wounds. The placental surface is seldom the source of the infection, because of its protected position. Lacerations are frequent, in the lower part of the cervix, upon the posterior commissure, and upon the sides of the urethral orifice towards the clitoris. These wounds are often "self-infected," as by the decomposition of portions of retained placenta, or coagulated blood, etc.; but more frequently the infection is brought from without, as by unclean instruments and impure air.

The reason why wounds in the puerperal woman so easily absorb hurtful matter, and why these local changes of puerperal fever so readily develop, depends chiefly upon the lowered vitality of the parts in the vicinity of these wounds. The bruised tissues, becoming often partly mortified, afford a most favorable base for the spread of the infection.

One means of prophylaxis is the timely use of instruments, as the forceps and perforator, to preserve the tissues from the bruising to which they are exposed during a prolonged expulsive period. The author supposes, however, the existence of wounds and bruised tissue in their vicinity; granted also an individual predisposition to infection; and that, besides, there is exposure to various pernicious influences, B. still thinks danger can be prevented with considerable certainty.

The prophylactic agent which he recommends is carbolic acid. This he has tested for seven and a half years at the hospital, and gives the following account of the way in which it is used:

As soon as labor pains begin, a full, lukewarm bath is given, and a lukewarm vaginal injection, with two per cent. solution of carbolic acid. These injections are repeated every two hours in the following cases; viz.: where the bag of waters ruptures early; in tedious labors; in induced abortion, and where the uterus contains a dead fœtus. Before each digital examination, and especially before any obstetric operation, we wash in a three per cent. solution of carbolic acid, and smear the fingers and hands with a carbolo-glycerine salve or 10 per cent. carbolic oil. The instruments are also thus washed. If it has been necessary to pass the hand into the uterus; if the fœtus be putrid, or the discharges offensive, or in case of a relaxed uterus, we inject at once into the uterus a solution of two or three per cent. carbolic acid.

Directly after birth, the external genitals are inspected; all wounded places covered with 10 per cent. carbolic oil.

Vaginal lacerations and the more frequent lacerations near the urethral orifice are united with carbolized silk; more extensive perineal lacerations and incisions with pure silver wire. The silk sutures are left to themselves, and the silver wires are removed after 14 days, the time of getting up. During the healing a pledget of lint saturated in the carbolic oil is placed in the introitus vaginae. Externally another pledget is used. They are frequently renewed.

We make vaginal injections of a two per cent. solution of carbolic acid twice a day in the simplest cases; three times a day, when the labor has been difficult or ended by instruments; every two hours in cases where a dead foetus has been borne; or where there is suspicion of retained placental or foetal membranes.

In case pieces of placenta have been retained, thus hindering uterine contraction and giving occasion to decomposition, intra-uterine injections are made twice a day with a two or three per cent. solution. The vaginal injections are made as before. This method is continued till all the wounds are healed or covered with protecting granulations. This happens generally in ten days, and then we use daily injections of alum water.

Before each catheterisation we place the catheter in boiling water, afterwards pass through it a solution of carbolic acid, and smear it with carbolic oil. Before its introduction, the urethral orifice must be cleansed from all secretions, lest they be carried into the bladder.

The author has never seen evil results from the use of this acid, even when large quantities have been employed for intra-uterine injections. The author submits the following statistics illustrative of the beneficial effects of this treatment:

From 1862-67 there were 514 puerperal cases. Of this number 33 deaths, *i. e.* 6.4 per cent.; puerperal fever cases, 116 = 22 per cent.; 1868, 83 confinements—percentage of deaths, four per cent.; puerperal fever cases, 37 = 44.5 per cent.; 1869, percentage of deaths = 6.4 per cent.; percentage of fever cases = 37.6 per cent.

From this time the carbolic acid treatment was more vigorously adopted.

1870, percentage of deaths = 0; percentage of fever cases, 22.3 per cent.

Statistics up to 1875 show a diminished percentage of puerperal fever cases, and fewer deaths among these cases. In the year 1875, among 224 confinements there was no death and only a few cases of sickness.

In consideration of these facts, the author maintains that carbolic acid has a most decided prophylactic effect, and he is not acquainted with any other antiseptic agent possessing equal power.

D. B. H.

2. ON THE SIGNIFICATION OF (ANTE-UTERINE) SUB-PERITONEAL EMPHYSEMA DURING LABOR. By HERMANN LÖHLEIN. (*Zeits. f. G. & Frauenk.* I. 2).

MCCLINTOCK called attention to the importance of the above accident, in a paper read before the Dublin Obstetrical Society, in May, 1859.

But little attention has been accorded the phenomenon, either in Great Britain or in Germany, though Kiwisch had previously declared the importance of the symptom as an evidence of rupture of the uterus.

L. mentions three cases—by Jolly, Dohrn, and Winckel.

In the case of Jolly, the rupture was of the cervix and reaching to the internal os. A large pocket was formed in the iliac fossa, and filled with coagulated blood and air, confined laterally by the insertion of the broad ligament to the pelvis, and extending medially to the median line.

The case of Dohrn belongs more strictly in the category as indicated by the heading, there being only an intrusion of air and no blood. Patient æt. 36; third pregnancy—miscarriage induced, because of rachitic pelvis, through for five days continued douche and frequent introduction of the catheter.

The waters came away prematurely; the child was decomposed, and much gas was evolved, giving rise to tympanites of the uterus; and further develop-

ment of gas in the wall of the uterus. Emphysematous crepitation was to be felt over almost the entire anterior surface of the womb.

The anterior surface of the uterus, upon section, was found dark green, with small collections of gas between the muscular fibres, the peritoneum raised over its whole surface, and the space filled with air bubbles, which might be pushed about and gave rise to emphysematous crackling. The muscular substance of the uterus, particularly anterior left, was very soft. The peritoneum was perforated in two places about the middle of the uterus.

In the case of Winkel, after tedious and difficult delivery with forceps, of a very large child, after cephalotripsy, the lower portion of the uterine peritoneum was found raised by bubbles of air, from the size of a pin-head to that of a bean, the entire serous coat somewhat discolored.

The inner surface of the lower third of the womb gave the impression as if its outermost layers had been scratched off, discolored, and with deep depressions reaching to near the peritoneal covering.

Fasbender contributes a case where he diagnosed subperitoneal emphysema, by the emphysematous crackling. The case was one of placenta previa, the vagina had been tamponed. The extraction of the fœtus showed it to be in a decomposed state, referring the emphysema to the penetration of gas.

The emphysema disappeared, and the woman recovered.

Ebell mentions a case with similar conditions, from the entrance of air into the uterine veins.

E. Martin saw a case where ante-uterine emphysema was occasioned by perforation of the posterior wall by a catheter in the hands of a midwife, inducing infiltration of pus in the anterior wall and the connective tissue of the right parametrium.

C. W.

GYNECOLOGY.

3. ON UTERINE MYOMATA ; THEIR ETIOLOGY, SYMPTOMATOLOGY, AND THERAPEUTICS. F. WINKEL, *Sammlung Klinischer Vorträge. Leipzig, No. 98.*

By some authors it is claimed that these tumors are found in from twenty to forty per cent. of all females over thirty years of age.

Since then, better results have been obtained, both by medical and surgical treatment, to which much attention has been devoted.

The data upon which the author bases his conclusions, are one hundred and fifteen cases of his own, the observations of a number of Mecklenburg physicians, and a compilation of all the cases in the accessible literature of every land, up to 1870, by Süsserott.

The author has nothing to add to the complete and classical description given by Virchow of the histology of these tumors, their position, etc.; and is therefore relieved from the task of describing their minute anatomy.

They are sharply defined hyperplasiæ of the uterine parenchyma, in the beginning round, easily enucleated, of concentric arrangement and generally composed of smooth, muscular fibres, with connective tissue and vessels. Except in one case by Bidder, no nerves have been found with positive certainty. Their smooth, muscular fibres are much larger than those of the normal unimpregnated uterus.

Sometimes, several neighboring tumors coalesce, and form one large tumor, between which lie the lymph tracts and their walled capillaries, so that the tumor has, in a measure, a cavernous conformation. In individual tumors, either connective tissue, smooth muscular fibres, or vessels may prevail, and accordingly they are distinguished as fibro-myomata, pure myomata, and lympho-myomata, or cystic telangiectatic or cavernous myomata.

According to the direction of their growth, these new formations are denominated intra-parietal, submucous, or subserous. So long as the tumor is small, *i. e.* from the size of a pea to a bean, it is generally contained within the

uterine wall without causing palpable thickening; indeed, the wall may be much thinner than in a diffused chronic hyperplasia.

As the tumor grows it becomes submucous, or subserous, growing in the direction of the least resistance.

Least often do myomata grow from the body of the uterus into the cervical wall, and scarcely less frequently do they emerge from the uterine wall between the surfaces of the broad ligaments.

In the 115 cases mentioned, there were found 28=24.3 per cent. subserous; 75=65 per cent. intra-parietal, and 12=10.7 per cent. submucous pedunculated fibroids. Hewitt noted, among 93 fibroid tumors, only 14 polypoid, while there were, among 92 cases diagnosed by the 34 Mecklenburg physicians, only 53 intra-parietal and subserous respectively, and 39 pedunculated (polypoid).

By the majority of authors (Virchow l. c. p. 180), the *posterior* wall and the *fundus* are given as the places of predilection of uterine myomata. W. agrees with this statement as far as his 115 cases are concerned.

As regards location, there were: *Subserous*.—Anterior wall, 17; posterior wall, 6; fundus, 3; intra-ligamentous, 2=28. *Intra-parietal*.—Anterior wall, 23; posterior wall, 30; both walls, 2; fundus, 10; lateral, 5; cervical, 5=75. *Submucous*.—Anterior wall, 2; posterior wall, 5; lateral, 1; not noted, 4=12. Marion Sims (*Uterine Surgery*, p. 74) found in 119 fibroids, 62 of the anterior, and 36 of the posterior wall.

Nearly all modern authors, from Virchow down, have contented themselves with the statement, that very little, or nothing, is known of the causes of myomata (Vel, Thomas, Scanzoni, Beigel, Schroeder, Churchill, Gubaru, G. Braun, etc.)

In 528 cases by West, Chiari, Hewitt, Beigel, Schroeder, and Winckel, there were under 20 years of age, 9 cases; 20-30 years, 98; 30-40 years, 180; 40-50 years, 180; 50-60 years, 52; 60-70 years, 6; at 70 years of age there were 2 cases; or 1.7, 18.1, 34.3, 34.3, 9.6, 1.17, and 0.9 per cent. respectively. From this we understand that more than two-thirds present themselves to the physician between the 30th and 50th years, which only indicates that in this time every tumor generally works that degree of discomfort which induces the patient to apply for medical aid. Observation of many cases also indicates that at least two-thirds of the cases show some symptoms of the presence of the tumor before the 40th year.

A division of the cases in classes as below, gives the following result:

| | 20-30 | 30-40 | 40-60 | Average age. |
|--------------------------|-------|-------|-------|--------------|
| 1st. Single..... | 4 | 9 | 11 | 39 years. |
| 2d. Married, barren..... | 11 | 8 | 13 | 35 " |
| 3d. " fruitful..... | 5 | 13 | 20 | 35-37. |

Further, it is proven by careful comparisons made by West, and admitted by Hewitt and Gallard, that it is about the beginning of the fourth decade of a woman's life that uterine myomata most frequently begin to form, and they claim that this time corresponds to the period of greatest functional activity of the uterus—" *De la plus grande activite sexuelle*." This expression, however, requires some explanation, for it may be maintained that this is the time at which conception most often occurs, which is not the case; for in Germany, as in France, the 22d year is the average time of the first confinement.

The average number of children to each German woman is four to five, allowing 18 months between two births. The beginning of the fourth decade must be looked upon as the time of *diminution of fruitfulness*; also as the time of *regular sexual intercourse, without the occurrence of pregnancy*; also as the time at which, from preceding births, some acquired anomalies (catarrh, dislocation, etc.) exist; and, through the non-cessation of the sexual relations, new irritations are produced without the same being recovered from during a new pregnancy.

In 555 patients with myomata there were found: 140 single, and without

children=24.2 per cent. ; 134 married, but sterile=24.3 per cent. ; 281 had once or oftener borne children=51.5 per cent. (more correctly, 25.22, 24.15, 50.63 per cent. respectively. REF.) From this it appears that only one-fourth of those who suffer with myomata are unmarried, and quite as many of those who are married and so suffer are sterile. This enormous number of sterile women having myomata speaks for the supposition that *myomata are the cause and not the result of sterility*, which latter is held by Scanzoni. Bayle dwells upon the preponderating influence of celibacy as favoring the development of the fibroid growth, although Dupuytren combatted this position. Virchow declares, as the general impression of his experience, that he has examined few cadavers of old maids *without* finding myomata, while in the cadavers of many women who had borne children, even in those who had reached old age, the uterus remained free. According to the above figures, the proportion of the unmarried are to the married as one to three affected with myomata ; while in Saxony, of 10,000 adults—5,594 married and 4,406 unmarried, in middle age—the number of both sexes is almost equal, thus leaving 2,203 unmarried and 2,797 married. That would correspond to a proportion of 7.3 to 9, while before we found 3 to 9, that is, hardly half so many unmarried.

From which may be deduced *that the married tend more to the affection in question than the unmarried ; i. e., those who not at all or seldom enjoy the sexual relation.*

The question at once arises if any material influence upon fruitfulness is exercised by this myoid development. Of 46 patients of Winckel's, and 62 collected by Süsserott, the following is the result : Cases, 108 ; births at term, 276 ; abortions, 16. From this it appears that more than one, and many births are by far less frequent among those suffering from myomata than among healthy women, which certainly justifies the conclusion, *that myomata are in fact an important obstacle to conception.*

Among the 108 women above mentioned, there were born only 2.7 children to each, which is more than 33 per cent. behind the average fruitfulness of women in Saxony. True, these conclusions are from a relatively small number of cases, but it is impossible to gather a larger number ; and as these results and those of Süsserott so nearly coincide, the value of each is thereby increased.

It has been claimed that *a myoma never presents itself before puberty.* (Virchow, l. c.) W. thinks, however, that this view admits of doubt.

Beigel reports a case in a girl of ten years, and W. had a case under treatment. Patient, æt. twenty-eight, had never menstruated ; she had been married thirty-one weeks, and had a pedunculated fibro-myoma of the size of a walnut attached to the fundus uteri. To the existence of this tumor neither repeated menstruation nor cohabitation—the latter because of the short married life—could have contributed. W. thinks it probable also, upon other grounds, that new formations may exist in the uterus anterior to puberty. In thirty-eight of his patients who suffered from uterine myomata with menorrhagia, the first menstruation occurred strikingly early, and was at once very profuse, the average age being fourteen years. It is also well known that ovarian tumors and uterine catarrh occur before puberty, and may therefore through irritation of the walls of the uterus lead to myoma before the advent of menstruation.

Much more important is the puerperal relation as cause of partial hyperplasia uteri.

In several of W.'s patients the beginning of their suffering was traced to abortion from injury and damage inflicted by instruments at confinement ; the new formation in the former corresponding to the portion of the uterus most affected by the injury.

Since there can be no doubt that in pregnant women slight injuries to any existing myoma may bruise the tumor and thereby lead to inflammatory conditions of the uterus, so it is in the highest degree probable that an adhesive inflammation may induce the formation of a myoma.

W. calls attention to the fact that large myomata very seldom present an obstacle to delivery, and that in more than 5,000 births he found only one of the size of a man's fist and interfering with the exit of the child.

Stisserott by great diligence collected only 147 such cases from all the countries of the earth, and embracing a period of half a century.

This he considers evidence that the physiological changes in pregnancy only exceptionally lead to the formation of neoplasms.

The effort made in lifting heavy bodies, or long continued high-reaching, often produce severe uterine bleeding and undoubtedly intramural hæmorrhages which lead to the formation of myomata.

Severe shaking of the body or excessive mental agitation, with accompanying bleeding, may have a like result. A violent emetic was the seeming *jous et origo mali* in one case.

Unusual hyperæmia of the uterus, active or passive, during menstruation, may well afford the starting-point of partial hyperplasia. In one case of a choir-singer, severe efforts while practising (*L'Africaine*) seemed to be the primary cause.

The labors of teachers; and particularly the use of the foot-bath, for the purpose of cutting short the menstrual flow, and dancing during the flow, are mentioned as having brought about the disease in a number of cases.

The various exathemata, typhoid, typhus and intermittent fever, epilepsy, have each given rise to myomata.

The reason why myomata are so seldom found in the cervical wall, as shown by Henle, rests upon the much greater thickness of the vessels in the walls of the cervix, and particularly its arteries, which have a much thicker muscularis than those of the body of the uterus.

The position of the myoma influences materially the return and duration of each hæmorrhage. In submucous and intrauterine pedunculated tumors abnormal bleeding was observed only once; in intra-parietal, of seventy-seven cases, thirteen times; in subserous, seven of twenty-eight cases.

In one case, although the patient was married and had a peritoneal polyp, she had never menstruated.

In some cases of myomata very rich in vessels there happens thrombosis and consequent breaking-down of the tumor, pelvic abscess and so-called cystic degeneration.

W. is convinced that the retention of urine does not always come from mechanical pressure upon the urethra, as Hardie claims, but that the bladder readily takes part in the swelling, especially in cervical tumors, and through cedema its contraction is hindered.

Two cases of intramural tumors disappeared without treatment as the result of frequent hæmorrhages; in all cases where the bleeding came from *the tumor* there was diminution of its size thereafter.

The benign nature of these tumors should not prevent the most untiring and energetical therapeutical treatment; and we may congratulate ourselves, in that while formerly these growths were considered as not admitting of extirpation, latterly most flattering results have followed the operation. In 60 cases of intra-parietal fibroids enucleated by G. Braun, 41 were successful; of 29 subserous tumors 18 were cured.¹

Of the 115 cases of the author—in 15 an operation was necessary, which consisted in some in removal, some in incision of the capsule to relieve tension, some in incision of the cervix. In 31 the treatment consisted of the use of ergotin hypodermically and by the mouth. In 12, injections of liquor ferri-sesquichlorat, into the cavity of the uterus were necessary, and were employed in one patient almost for five years, and at nearly every return of the hæmorrhage, each time with good result, and never with any injurious consequence.

Each case must be treated aside from any routine.

In hæmorrhages of moderate grade, the treatment is rest, cool surroundings, cool lavements, injections of vinegar and water into the vagina, and

¹ Virchow—Hirsch, II. 3, p. 760.

ergotin internally, five centigrammes three times a day, or, in severer forms, once daily hypodermically for four or five days.

When there is catarrh or cystoid degeneration of the uterine mucous membrane, or unusual tumefaction of the endometrium, introduce liq. ferri sesquichlorat in gum tragacanth pencils, or by means of cotton upon the sound, which is allowed to remain one hour to one hour and a half, and apply it eventually for several days in succession.

Small polypi or mollusca are to be scraped off by the Récamier or Sims curette.

Incisions over the site of the myoma may be dangerous, because of the profuse bleeding if a venous sinus be opened.

The effect of the ergotin treatment has been that those tumors which were intra-parietal, and especially those rich in vessels, were diminished in size very materially. In one case, the circumference of the abdomen was reduced from 94 to 72 ctm. after 30 injections; in another 17 ctm. after 130 injections and 12.5 grammes of ergotin in pill.

No abscesses formed in any case. The injections were sometimes performed by the patient herself, and always made on the abdomen, each one consisting of 5 centigrammes of Wernich's ergotin.¹

C. W.

PEDIATRICS.

4. ON THE CONDITION OF THE URINE AND KIDNEYS OF NEW-BORN CHILDREN. A. MARTIN AND C. RUGE. (*Zeits. f. G. & Frauenk.* i. 2.)

The urine of the new-born children was collected by means of a bag made of gold-beaters' skin, or rubber, fitted so as to include the penis and scrotum. (Only males were used in the experiments.) Every possible source of fallacy was avoided. For the experiments twenty-four children were used, the time of observation extending over a period of ten days. The babies thrived well, with two exceptions, one being accidentally suffocated by the mother during sleep, and the other dying of gastro-enteritis; the urine of this child, however, was not used after symptoms of disease set in.

It was found, contrary to the generally received opinion, that urination seldom takes place immediately after birth. In this the authors agree with Richard.² The time varied from immediately after birth in three infants to fifty-three hours in one child.

The average time occupied by the labors was 15½ hours. Salicylic acid was given to the mothers before and during labor, and could be detected in the urine of the child, but no trace was found in the amniotic fluid, or in the discharges from the maternal passage, thus showing that urine was not passed by the child *intra vitam*.

The greatest quantity of urine at the first micturition was 28 cubic centimetres, seventeen hours after birth, from a child at full term; the least from one of twins, 1 cubic centimetre; average of twenty-two cases, 9.6 cubic centimetres.

A table showing the quantity of urine passed by an average number of thirteen children, from the first to tenth day inclusive, condensed, gives an average quantity to each one of 3.02 cubic centimetres per day; the smallest average being on the second day, 0.63 cubic centimetres; the largest on the eleventh day, 11 cubic centimetres.

To determine the quantity of urine passed within the first twenty-four hours for each kilogramme of weight of the child, thirteen observations were made, the result being 4.4 grms. urine for each klgrm. of weight.

It was found that while the average quantity of urine voided in the first twenty-four hours by children (the average weight of each being 3127 grms.) was 4.4 grms. per kilogramme, the average for children from the ninth to the tenth day, with a mean weight of 2,968 grms., was 18.8 grms. per kilogramme.

¹ See this journal, Vol. VII, p. 532.

² *Traité pratique des Maladies des Enfants*, 1829.

This very great increase in the amount of urine, the weight being nearly the same, must indicate a greatly increasing tissue change.

The authors think that the estimate given by Gerhardt, who cites the observations of Pollack, of the total quantity of urine voided by the child daily, 250-410 ccm., and by Hecker, 90 ccm. for the first day, very much too high. They obtain a general average for the first ten days of 40 ccm.

The urine tested immediately after emission was found in most cases acid, once neutral, once alkaline.

In ten cases the specific gravity of the first urine was determined; the mean was 1012, in only one case was it as low as 1006, and in one it was 1017. Dohrn places it at 1001 to 1006.

In the first ten days the mean specific gravity was 1004 in eighty-two examinations.

A very marked disparity was found between the amount of chloride of sodium in the morning and afternoon; mean percentage, 0.102 per cent. A. M. against 0.042 per cent. P. M. In the adult this relation of the excretion of the chlorides is the reverse.

Albumen was found in the first few days of the infant's life, oftenest in that voided in the morning, gradually disappearing by the eighth day. In 8 per cent. of the twenty-four cases there was a moderate quantity, in 46 per cent. an unimportant quantity, and in 46 per cent. only a trace was found.

In an average quantity of 39.333 grms. of urine to each child for the first ten days, there was voided per diem 0.1923 grm. urea. On the first day there were per kilogramme (weight of body), 4.4 grms. urine, 0.0205 grm. urea; tenth day, 18.8 grms. urine, 0.0919 grm. urea.

Sixty-seven observations placed the excretion of urea on the eighth day at 0.2284 per cent.

Great difficulty was experienced in the examinations for uric acid, because of the very small quantity of urine obtainable; but reliable results were obtained as to the sixth, seventh and eighth days, the mean quantity being 0.0463 per cent., equal to 0.0214 grm. uric acid per diem.

The result of twenty examinations gave as the mean proportion of water and solid matter: water, 98.826 per cent., solid matter, 0.93 per cent., loss, 0.244 per cent.

In three cases, phosphoric acid was found; 5th day, 0.014 per cent.; 7th day, 0.089 per cent. and 0.032 per cent. respectively.

Biliverdin, even in the urine of those children with icterus, as well as sugar, creatinin, &c., could not be found.

In regard to albumen in the urine of the still-born, it was proved that its presence is constant, that normal fresh urine placed in the previously emptied bladder of the still-born infant, and allowed to remain twenty-four hours, became albuminous.

In various cases the urine of the children of mothers affected with morbus Brightii was found highly albuminous, with cylindrical epithelium, fat corpuscles, &c. The kidneys were found in still-born children of nephritic subjects to be as 1 to 91, and 1 to 89, instead of as in others, 1 to 112, 1 to 136, &c.

Salicylic acid given to the mother during labor was detected in the urine of the child forty minutes later. This rapid communication of substances from the blood of the mother to the child, is not met with alike in all drugs; for instance, iodide of potassium was found in small quantities only after prolonged use of the drug by the mother.

The article closes with an earnest recommendation that the newly-born be as much as possible protected by proper treatment from the danger to which they are exposed from uric acid infarction; *i. e.* hyperæmia, catarrh and inflammation of the kidneys produced by the propulsion of uric acid crystals through the uriniferous tubules. Accompanying the paper are elaborate charts showing quantity, specific gravity, amount of albumen, chlorides, uric acid, &c.

C. W.

REVIEWS AND NOTICES OF BOOKS.

SEVENTH CLINICAL REPORT OF THE ROTUNDA LYING-IN HOSPITAL, FOR THE YEAR ENDING 5th NOVEMBER, 1875. By GEORGE JOHNSTON, M.D., Late Master of the Rotunda Lying-in Hospital, &c., &c. Dublin: John Falconer, 33 Upper Sackville St. 1876. Pp. 42.

THIS report closes the series of excellent annual reviews commenced and continued by Dr. Johnston, the reputation of which has spread all over the world, and the conclusions drawn from which have been received as first-class authority by the whole profession. As far as we are aware, the reports of the Rotunda Hospital have been equalled as a series by those of no other obstetric institution. Of the 1,065 confinements during the year, 813 were perfectly natural—*i. e.* the head presented; the child was born within 24 hours, without artificial aid or untoward circumstances; the placenta was expelled by the natural efforts, and no post-partum hemorrhage occurred. The forceps was applied in 113 cases; or, deducting 40 abortions from 1065, about once in every 9 1-14 cases. Craniotomy was performed in five instances, and in four cases version was necessary. Chloroform was administered in 100 instances.

The mortality was 27 from all causes, or about 1 in 39½. This apparently large percentage is to be attributed, according to Dr. Johnston, to causes entirely independent of the mere locality of confinement; thus, there were 10 cases of seduction, with greater or less mental distress; 2 of fretting, from having been deserted by their husbands; 5 in extremely delicate health, some with pyæmia upon them on admission; 2 cases of scarlatina; 1 bronchitis, pleuritis and pneumonia; 1 diarrhoea, terminating in peritonitis; 1 fungoid tumor of the uterus and fatty tissue; 2 peritonitis; 1 renal disease. Of the 10 cases of seduction, 8 developed symptoms of peritonitis within 36 hours after delivery, and died rapidly.

Of the 113 forceps cases, 83 were in primiparæ (75 mothers, 74 children lived; 8 mothers died, 6 being cases of seduction and fretting, 2 of peritonitis); 30 were pluriparæ (28 mothers, 28 children lived; 2 mothers died, 1 of pleuritis, 1 of fungoid tumor of uterus). Thus, 103 mothers recovered, and 102 children were saved, which latter would in all probability have been lost, if left to the natural efforts. The forceps employed were the double-curved variety of Barnes, which have been used during the last five years; finding that the long and short straight forceps formerly employed were liable to slip. Barnes' instrument is safe, easy of application, does not compress the child's head to any injurious extent, and is perfectly available, whether the head be above the brim, in the brim, or in the cavity—in fact, Dr. J. does not see the necessity for using any other kind of forceps. As a proof of their safety and power, he gives an instance from a former report. A woman had an exostosis of the right sacro-iliac synchondrosis, preventing the head from entering the brim; it took three-quarters of an hour to extract the child, whose head, when born, presented a depression occupying the prominence of the left parietal bone of over 3" long and 1½" broad, and at least ½" deep; yet this boy, weighing 7 lb 12 oz., lived, and was discharged well with his mother on the eighth day.

The chief point of interest in this report, in our opinion, is the application of the forceps before the full dilatation of the os uteri, which was effected in 33 of the 113 forceps cases, the cause for early interference being usually the premature rupture of the membranes. Of the mothers, only one died, innupta and fretting; of the children 3 were born dead and 2 died. In summing up all the cases of early forceps recorded during the period, in which this practice has been resorted to, it appears that 169 women were delivered in this manner, 122 being primiparæ and 47 pluriparæ.

96 male children were delivered, 77 of which lived, 5 died, and 14 were born dead.

73 female children were delivered, 57 of which lived, 8 died, and 8 were still-born.

In 56 instances the os uteri was only $\frac{1}{8}$ ths dilated.

In 80 " " " $\frac{1}{4}$ ths "

In 33 " " " $\frac{1}{2}$ ths "

In 23 instances the head was above the brim.

In 69 " " " in the brim.

In 71 " " " in the cavity.

159 mothers recovered, 10 died, in none of whom could the autopsy show any injury to the soft parts.

Dr. Johnston says that the more he sees of early interference, and the benefits arising from it, the more he is induced to persevere in it; that it has been the means, as far as his experience goes, of saving the lives of many mothers and children, and that he therefore thinks himself justified in recommending it. The argument that it is dangerous or unjustifiable in unskilful hands, is as absurd as to condemn lithotomy or any other surgical operation, merely because a careless or unpractised operator would be likely to fail.

In reply to the objection raised by some, that the forceps is a dangerous instrument, and as a proof to the contrary, Dr. Johnston says that of the 752 cases delivered by the forceps at the Rotunda, within the last 7 years, in no one instance was injury inflicted by the instrument on the soft parts of the mother.

With regard to the question, whether a short 2d stage of labor favors post-partum hemorrhage, it appears that of 653 cases in which the 2d stage did not last beyond one hour, there were 21 cases of post-partum hemorrhage (the total number of hemorrhages in all the labors being 31), equal to two-thirds, rather a large and significant affirmative percentage. Of the 21 cases, 14 were of the first degree, requiring merely the cold wet napkin to the vulva, and proper pressure; 4 of the second degree, where it was thought prudent to inject cold water into the vagina; and 3 of the third degree, where the injection of the solution of perchloride of iron into the uterus became necessary. In the great majority of instances, post-partum hemorrhage may be avoided, if constant, steady, and proper pressure be exerted over the fundus uteri from the moment the child's head is being expelled at the outlet until the expulsion of the placenta, and the proper adjustment of the binder, which should be so arranged, with pad, if necessary, as to secure the uterus from relaxing. A strict observance of this rule not only prevents the complication, but favors the rapid completion of the third stage.

However progressive Dr. Johnston may be as regards the forceps, he thus still adheres to the time-honored obstetric binder, and the much-discussed intra-uterine injection of iron, for both of which acts of conservatism he doubtless has as good reasons as for his enthusiastic advocacy of early and frequent extraction by forceps. While the binder in a normal post-partum state of the uterus and abdominal muscles certainly is a useless, and, if improperly applied, even an injurious contrivance, there is no doubt in our mind that, in cases where there is a tendency to relaxation of the uterine fibres, and where the abdominal parietes are flabby and pendulous, a properly applied abdominal bandage is not only a comfort to the patient, but also an excellent means of promoting uterine contraction and thus preventing hemorrhage.

Of *clumpsia* there were four cases, only one of whom died. The treatment of the convulsions consisted in enemata of turpentine and asafœtida, chloroform, and bromide of potassium combined with chloral; and proved very successful. There were ten cases of *scarlatina*, seven in primiparæ and three in pluriparæ; only two died, one on the third, the other on the fifth day after the appearance of the disease. The rash showed itself between the second and sixth days after delivery. The cases occurred at intervals of one, two, and three months, and in noway depended one upon the other. Puerperal scarlatina thus by no means appears to be so fatal a complication as is generally supposed.

There are other points in Dr. Johnston's report which might be touched upon with benefit, but we think we have called attention to the principal subjects of

interest. We cordially join in the encomiums passed on the report by Dr. More Madden, at the meeting of the Dublin Obstetrical Society, January 6th, 1876; and trust that Dr. Johnston will comply with Dr. Madden's suggestion to condense the series into one volume. We also venture to express a hope that Dr. Johnston's successors will follow his example, and continue to publish the valuable experience annually gained at the Rotunda Lying-in Hospital.—P. F. M.

BERICHT DER NEUEN GYNÄKOLOGISCHEN UNIVERSITÄTS KLINIK—REPORT OF THE NEW GYNECOLOGICAL UNIVERSITY CLINIC AND SCHOOL FOR MIDWIVES AT KÖNIGSBERG, PRUSSIA, for the year ending Nov. 1, 1874, by DR. H. HILDEBRANDT, Prof., with three architectural plans. Leipzig, Breitkopf & Härtel, 1876, pp. 132.

THE scope of this Report is much greater than that of most annual Reports of Lying-in Hospitals, containing as it does a description of the situation and internal arrangements of the clinic, with the rules governing its management, to which the first forty pages are devoted; and also an account of the purely gynecological portion of the clinic, and of the obstetrical and gynecological polyclinic, or out-door department, covering the last forty-three pages; thus, only the intermediate forty-nine pages are confined to the report of the lying-in wards proper.

Of the general arrangements of the clinic, with its three plans, one for each floor, we have only to mention that sufficient ventilation is provided for by means of four large shafts, containing currents of hot air; which shafts communicate with all the halls, and from them, through shutters in the doors and walls, with all the wards, rooms, and water-closets, free egress of vitiated air being permitted by efferent shafts leading to the roof. The nine lying-in rooms contain four beds each; the two rooms destined for pregnant women eight beds each. Each woman is confined in the bed which she is thereafter to occupy, proper rotation being observed in the occupation of the rooms. This practice has been found to work much better than to have one common lying-in room, whence the puerpera is carried through the halls, and subjected to the various accidents of exposure and motion, besides being liable to contract infection from some previous occupant of the common couch. One point of interest is the manner of disposal of the placenta, each of which, after the precedent of Greuser of Dresden, is at once enclosed in a sheet-iron box, and slid into the furnace by the engineer, where it is reduced to cinders in a few moments. The diet of the puerperæ is still on the old principle, meat not being allowed before the fifth day. The women, who spend the last four or six weeks of their pregnancy in the institution, and are used for purposes of instruction, on the other hand, do not appear to suffer in point of food, to judge from the fact mentioned, that the children of these women weighed after birth on an average $171\frac{1}{2}$ grammes (about 6 ounces) more than those whose mothers entered the clinic already in labor. This, as far as our experience goes, is a laudable exception to the rule; we are quite sure that no such result would have been obtained if the same measurements had been made at the clinic of Würzburg at any time during our three years' service as its assistant, in spite of our persistent endeavors to improve the fare of both gravidæ and puerperæ. Motives of economy on the part of the government of such institutions very frequently outweigh the wishes and remonstrances of the medical officers.

All the various chapters of this portion of the Report are discussed in detail; but we must refer chiefs of obstetric clinics and busy obstetricians, to whom they will prove especially interesting and useful, to the work itself; and pass on to the cursory consideration of the body of the Report, the Obstetric Department.

The number of confinements was 356. The average duration of labor for the primiparæ was 11.08 hours, for the pluriparæ 6.5 hours.

Post-partum hemorrhage from uterine atony was treated by Crede's manipulation, hypodermics of ergotin, and cold cervical injections. Hemorrhage

from deep lacerations of the cervix was treated by direct digital compression between the index and third fingers, the middle finger being passed into the rent; three minutes always sufficed to arrest the bleeding.

Ruptures of the perineum occurred in 30 cases, 28 primiparæ, and 2 secundiparæ. As regards treatment, the principle was followed, to unite at once every laceration, no matter how small, as soon as the muscular tissue was found to be injured: wire sutures were used, four being the greatest number needed in any case; the thighs were kept tied together, the genitalia cleansed by pouring some disinfectant solution (carbolic acid or potass. permang.) over them; the bowels were moved on the fourth day by castor oil, and the sutures removed on the fifth or sixth day. In all of the 23 cases in which the immediate suture was used, union by first intention took place. In three other cases in which *serre-fines* were employed, complete union also ensued, the *serre-fines* being loosened every 24 hours and removed after four days. One case of central perineal rupture occurred—a small piece, of the size of a three cent piece, sloughing out a few days after delivery; the opening was closed by nitrate of silver in solution. The correctness of these, certainly very unusually favorable results—26 cases of union out of 26 cases of immediate operation—is vouched for by Hildebrandt himself. Ritgen's method of supporting the perineum with two fingers in the rectum (revived by Goodell), is highly praised; and the comparatively small proportion of perineal lacerations (19.7% in primiparæ, to 21.1%, reported by Olshausen), in a measure attributed to its frequent employment. A laceration of the external sphincter ani was observed in one case, as a consequence of this method, but the perineum was saved. The generally accepted belief that the frenulum is ordinarily torn at the first labor, is not necessarily correct; for out of the 117 primiparæ, 90 left the clinic with absolutely intact perineæ.

Two cases of *puerperal eclampsia* were observed: One in a vigorous plethoric woman (21 attacks, two venesections, the first of 200 grammes, about 7 oz., the second of 500 grammes=16 oz.; internally, benzoic acid 0.1 gramme. =gr. ii. every hour; a bougie in the uterus, and hot vaginal douche to excite contractions); after 30 hours, the cervix was so far dilated as to permit the version and extraction of the dead child; normal puerperal state and recovery. In the second case, the convulsions, of which there were eleven attacks, seemed to be induced by spasmodic constriction of the internal os, for the urine contained no albumen. After the removal of the spasm by morphine and warm baths, the convulsions ceased, and a living child was born; convalescence normal.

The management of *labor in contracted pelvis* is one of the subjects on which Prof. Hildebrandt expresses himself most decidedly, and seems to hold original opinions. The main features of his views and practice are the following:

1. Premature labor is induced in all cases, provided they present themselves early enough, where the pelvis is of the flat and flat rachitic types, with a true conjugate of 6.5 to 8.5 ctm. (2.5 to 3.25 inches), and also in generally contracted pelvis with a conjugate of 8.0 to 9.5 ctm. (3.14 to 3.72 inches). Dohrn's rule is strictly observed, to entrust the treatment of the case to but one physician, and to choose the simplest method, viz., the introduction of a catgut bougie, preceded, if necessary, by a laminaria tent.

2. In pelvis of 6.5 ctm. conjugate and upwards, the accommodation of the head to the pelvic cavity is patiently waited for, and facilitated by maintaining uterine activity and observing the proper lateral decubitus. Should symptoms supervene which indicate the inability of nature to expel the child, or which call for a speedy delivery, the perforation even of the living child is performed, and the forceps and version employed only in exceptional cases, as now to be specified.

3. As a rule, both forceps and version are procedures but little adapted to conduct the head through a contracted pelvis; and, if forcibly employed, dangerous and fatal to mother and child.

It is evident that the proper plan to overcome the obstacle of a narrow pel-

vis, is to seek to produce the most favorable mechanism for the passage of the head, and not to drag it forcibly through, either with the forceps, or the hand after version. The rational method is to study the various ways in which the head adapts itself to the different forms of pelvic contraction, and then endeavor in each individual case to favor the natural mechanism. But neither the forceps nor version are suitable means for this purpose. Although, at first sight, version would seem to answer this requirement, such is not the case; for the mere placing the head with its smallest diameters in the contracted conjugate, by no means ensures the easy delivery of the head, which is usually attended with the greatest difficulty and danger to the child: and here is the great objection to version in these cases. Expulsion of the head by manual pressure over the symphysis is advisable and useful in normal non-contracted pelves, but pelves with any degree of contraction would require the exhibition of so much direct force as inevitably to injure and crush the uterine tissue; and as for the removal of the head by traction on the shoulders or face, it is an absolutely dangerous procedure for the child in a distorted pelvis. This chief objection to version—its insufficiency as regards the preservation of the foetal life—is enhanced besides by a condition, which Hildebrandt thinks is not deservedly appreciated; viz., the spasmodic contraction of the lower portion of the uterus near the internal os, which contraction is a frequent consequence of the irritation produced by the introduction of the hand into the uterus after the discharge of its fluid contents (this being always the condition when version is performed *lege artis* in narrow pelves), and the operation of turning itself. This spasm often acts as a more effectual obstacle to the extraction of the foetal head, and as a more fatal impediment than the contracted conjugate itself.

4. *Version* is indicated in a lax, unirritated uterus, especially if the membranes be still intact or have only just ruptured, in cases where, beside the narrow pelvis, conditions are present which prevent the normal entrance of the head into the brim (pendulous abdomen; excessive pelvic inclination, with anteversion of the uterus; physical weakness of the uterus) or necessitate a speedy completion of the labor (deep seat of the placenta and flooding, eclampsia, etc.).

5. The *forceps* is to be applied to the presenting head under the above indications, when the head has already nearly passed the contracted plane of the brim, and the bitemporal diameter has entered the pelvic cavity, and only slight force is required to overcome the remaining impediment. Its employment is desirable in such cases, also, when the head is still at the brim, because two or three tractions will then generally suffice to conquer the difficulty. In any case where an experienced hand has failed to overcome the obstacle at the brim with three steady tractions, the forceps should be discarded, and craniotomy at once performed.

6. After having been a firm advocate of version for a number of years, because he preferred it to the dangerous forceps and cephalotribe, Hildebrandt has now arrived at the conclusion that forceps and version both have but a very limited range of utility and safety in the management of dystocia in distorted pelves, and has adopted the practice, supported by theory and experience, of leaving the delivery, even in the higher grades of pelvic deformity, to the powers of nature, merely taking care to regulate the contractions of the uterus, and the mechanism of the presenting part; and resorting, when necessary, only to craniotomy, with or without subsequent extraction.

7. Until within a few years, craniotomy could not be considered an innocuous operation. The necessity of employing, as an extractor, "the cephalotribe, the most barbarous and unwieldy of all obstetrical instruments," made it an exceedingly dangerous procedure. Since becoming acquainted with the latest modification of the cranioclast, by Braun, of Vienna, Hildebrandt has entirely discarded the cephalotribe, and treats cases requiring perforation (which operation is performed with Braun's curved trephine, under the conditions above stated), by first leaving the expulsion of the evacuated head to nature, if the expulsive forces are equal to the task; if, however, the pains

are insufficient, or the skull is too unyielding, the cranioclast is at once applied, the solid branch being passed into the cranial cavity, and the fenestrated blade over the most solid part of the head, the face; the compression screw is firmly closed, and the head extracted by slow and steady traction. The skull becomes elongated by traction, and accommodates itself gradually to the shape and size of the pelvic cavity; and is thus able to pass even extreme distortions without injuring the soft parts. Performed in this manner, Hildebrandt declares craniotomy, with or without subsequent extraction of the fœtus, to be a measure positively devoid of danger to the mother; and produces, as example, the three cases of craniotomy occurring in the Clinic during 1873-4, in two of which, the women were discharged on the tenth day (1. conj. diag.; 9. 5 ctm. = 3 72," and 2. conj. dia r. 8 ctm. = 3.14," perforation of living child); and in the third on the fifteenth day (conj. diag. 10 ctm. = 3.83," perforation of living child), none of them having experienced the least puerperal difficulty.

Finally, Hildebrandt expresses the hope that the entirely adequate and all-sufficient cranioclast will utterly supersede the cephalotribe as an instrument for the extraction of the fœtus after preceding perforation; and further, as the practice of repeatedly crushing the skull with the cephalotribe, recommended by Pajot under the name of "*Céphalotripsie répétée sans tractions*," becomes a dangerous operation, by the necessity of frequently introducing or shifting the bulky instrument, Hildebrandt trusts that "the cephalotribe will soon possess but a historical interest."

We are gratified to see this hearty unqualified support of the cranioclast, the advantages of which instrument, as presented by Hildebrandt, entirely correspond with those claimed for it by us in an article on the subject a few years since. ("The Cranioclast as used by the Vienna School." *American Journal of Obstetrics*, May, 1873).

Fluoridization of the uterus after the method of McRae (*Edinburgh Med. Journ.*, Sept., 1873) was employed in three cases of atony of the uterus in the first and second stages of labor, but without the least effect. In the third stage, however, and especially post-partum, and during the first twenty-four puerperal hours, the electro-magnetic current proved itself an exceedingly efficient oxytocic.

A normal puerperality requires, according to H., that the puerpera should at no time possess a temperature above 38° C. = 100.5 F. (rather too high, it would seem to us, REV.), and her general health and the involution of her genital organs should be such as to permit her safe discharge on the tenth day. He considers every febrile condition, as soon as it is recognizable by a rise in the temperature and pulse, or temperature alone, to be pathological; because there must be a cause for the fever, although our diagnostic means may not be able to discover it. Basing on so severe a standard, H. says that it is not surprising, that out of the 367 puerperæ in the institution, there were only 201 who presented an entirely normal puerperal state, while 166 were more or less pathological. Of these 166, however, only 21 were really seriously ill; the remainder had but slight temporary disturbances, referable in many instances to psychical causes, constipation, erosions of the nipples, or mammary congestion, etc. In one case of parotitis on the left side, a metastasis to the right ovary was observed. Four puerperæ died, or one in 93½, three of diphtheritic endometritis and peritonitis (two from undoubted infection by students coming—contrary to orders—from a surgical operative course), one of septicæmia from carcinoma uteri.

Complete isolation in separate rooms, and with separate physician and nurse, prevented the spreading of the infection from these cases of metria.

In the gynecological wards, 160 patients were treated. Of these, eight had vesico-vaginal fistulæ, seven of which were operated, and all cured. Simon's position and instruments and iron wire were used; the catheter only the first three or four days.

Constriction of the cervical canal was met with in thirteen cases, all of which were incised with Sims' knife, and all cured. (Whether of the symptoms only, or of sterility, is not stated.—REV.)

In one case of hyperplasia uteri, the uterus was reduced in size one inch by means of forty-five hypodermics of ergotin. Of seven intramural fibroids, two were completely dispersed by ergot hypodermically, and in the others the hemorrhage was permanently relieved. In one case, 235 injections were made.

One case of tubo-ovarian cyst, ovariectomy, recovery, is reported. The peculiarity of this tumor, (which was first described by Richard, and is referred to by Klob, Scanzoni, and Spencer Wells, although these authors do not mention having seen either a fresh or preserved specimen,) was that the fimbriae directly participated in the formation of the cyst wall, the convoluted mucous membrane of the tube merged into the lining membrane of the cyst, and the original abdominal opening of the tube was so much dilated as to permit the introduction of the index finger from the cavity of the ovarian cyst.

No more appropriate companion to the Rotunda Report could be found than that by Prof. Hildebrandt. Annual reports of this kind, when they contain so large a proportion of original matter and individual comments, possess an incomparably greater interest and value than the mere statistical compilation of a certain array of figures. P. F. M.

THE SURGERY, SURGICAL PATHOLOGY, AND SURGICAL ANATOMY OF THE FEMALE PELVIC ORGANS. In a series of Colored Plates taken from nature, with Commentaries, Notes, and Cases. By HENRY SAVAGE, M.D., Lond. Third Edition. Revised and greatly extended. An additional Plate; also 36 wood engravings, with special illustrations of the Operations in Vesicovaginal Fistula, Ovariectomy, and Perineal Operations. Phila. : Lindsay & Blakiston. 1876. Seventeen Colored Plates. Pp. 83.

Few works have come to us for review which have given us more pleasure than this magnificent volume. As regards the artistic, correct, and beautifully colored plates, they are the same as in the first edition, Plate XVII. having been omitted in the second edition. The text accompanying each plate has, however, been carefully and extensively revised and altered, particularly of those chapters treating of Pathological Histology and Classification of Neoplasms, and of Surgical Reflections and Procedures; and many illustrative cases have been introduced, which materially increase the practical value of the book.

The protracted experience of the author at the Samaritan Hospital for Women, as assistant, and, subsequently, colleague of Spencer Wells and Sir William Fergusson, imparts to his statements and opinions the stamp of authority on the subjects he discusses.

A glance at the contents of each individual plate, with occasional gleanings of important passages from the text, will give those of our readers who have not become familiar with the former editions the best idea of the scope and purpose of this excellent work.

Plate I.—Muscles of the Female Perineum. Three Figs., 4 woodcuts.

Plate II.—Outer Fasciæ and Aponeuroses of the Female Perineum. Two Figs. "The fatty tissue filling up the ischio-rectal fossa and posterior perineal space, and peculiar to this situation, seems never subject to hypertrophic enlargement."

Plate III.—Arteries and Nerves of the Female Perineum.

Plate IV.—Erectile Organs and Veins of the Female Perineum. Two Figs. As none of the genital, perineal, and pelvic veins are provided with valves, the frequent occurrence of all forms of pudendal hæmatocele is readily explicable.

Plate V.—Arteries of the Female Pelvic Organs.

Plate VI.—Veins and Erectile Venous Plexuses of the Female Pelvis. Two Figs.

Pelvic sanguineous effusions are reducible into two practical denominations; viz., pelvic cellular and pelvic peritoneal sanguineous extravasations; the latter of which are decidedly the more frequent; indeed, the fixed relations of

the pelvic peritoneum, although permitting the slow accumulation of pus, would appear to render the sudden formation of a large sub-peritoneal hæmatoma impossible. "Intra-peritoneal hæmatoma occasionally provokes plastic exudation on the part of the peritoneum, with the result of encapsulating the hæmatoma. The upper part of this capsule has probably been mistaken for an elevated portion of peritoneum, covering a so-called sub-peritoneal hæmatoma."

Plate VII.—Five Figs., 12 woodcuts. Relations of the Muscular Floor of the Pelvis to the Bladder, Vagina, Rectum, and Coccyx—to the Presentation at the last stage of Parturition, Vesico-vaginal Septum and Base of Bladder, etc. Anatomical Relations of the Alar Ligament and its Contents, Ovaries, etc.

Plate VIII.—Two Figs., 6 woodcuts. The Relations of the Female Pelvic Organs with the Pelvis and with one another. Peritoneal and Sub-peritoneal Pelvic Spaces. Horizontal and Perpendicular Sections of Pelvis.

"The relations of the peritoneum with the pelvic organs above the pelvic plane, exactly agree with the supposition that they were thrust upwards against its under surface in attaining their respective positions. It may be added that these relations are never afterwards changed, except as the result of a disintegrating process, such as that attending phlegmonous formations."

"The end of the cervix uteri marks nearly the centre of the pelvic cavity—the centre of a general radius of about two inches." After childbirth—after complete involution even—the non-puerperal dimensions of the uterus are permanently increased by about one-fourth—in the vertical direction even by a third. The anterior labium uteri is adherent to the vaginal fibres; the posterior is free of them nearly up to their uterine attachment at the isthmus. No matter how great the distention of the bladder, the peritoneum never quits in the least its attachment to the uterine body, nor those it has with the abdominal or pelvic parietes.

This chapter includes the classification of genital neoplasms; the tissue changes especially prominent in the uterine system; ulcerations; uterine and ovarian cysts; pelvic abscess; three cases of that rare affection, erectile thrombus of uterus; uterine polypi, tumors, and inversion; congenital anomalies of the genital system, illustrated by numerous typical cases; and closes with six pages of highly instructive and practical "Surgical Notes and Reflections" on the above topics, from which we extract the three following axioms:

1. "The majority of uterine affections really local—that is, not constitutional—depend on an unwholesome condition of the inner surface of the uterine, not the cervical cavity, or morbid antagonism between the uterus and cervix, one or both often cured by simple dilatation of the latter."

2. "No surgical proceeding whatever touching any part of the uterine system should be unattended by those precautions observed in operations of a grave character there or elsewhere; in certain states of the general system unshadowed by any recognizable peculiarity, the most trivial operation has been speedily followed by fatal peritonitis."

3. "The pouch of Douglas behind and the bladder in front, notwithstanding the yielding character of its uterine cellular connections, *invariably* follow the uterine cervix, maintaining unaltered relations with it, whatever be the nature of the displacement. *This is actually a rule without exception, never disregarded without evil consequences.* Surgical proceedings for the amputation of the uterine cervix should invariably be governed by the above rule. Cervical hyperplasia may be infra- or supra-vaginal; there is no recorded instance of the two forms existing together in the same cervix." &c.

This chapter, both anatomically and clinically, appears to us to be the best in the book; it will be found equalled in few text-books on Gynecology.

Plate IX.—Four Figs. Fascial coverings of the muscular floor of the pelvis; muscular floor of the pelvis denuded of fascial coverings, perpendicular sections of pelvis.

Plate X.—Six Figs. Nerves of the unimpregnated uterus and vagina. Contents of the alar mesentery, ovaries, round ligaments, &c.

Plate XI.—Four Figs. Mechanism of the structures supporting the uterus and opposing its displacement.

Plate XII.—Two Figs. Pelvic and Uterine Lymphatics.

Plate XIII.—Four Figs. Illustration of the chief varieties of Perineal Plastic Surgery for the radical cure of complete Prolapsus Uteri and Lacerated Perineum. Laceration of the perineum is never produced by uterine prolapsus, nor is the latter affection a necessary consequence of any kind of structural changes in the perineum caused by parturition. The author represents his own operation for prolapse and ruptured perineum, viz.: the resection of all the redundant vagina at its ano-vulvar margin, and the removal of a triangular portion of vaginal mucous membrane, the middle angle extending to some distance upwards along the posterior wall of the vagina, securing with quilled suture in the usual way. This, he says, appears to be the only way of bringing together again at the ano-vulvar perineum the two ischio-perineal ligaments; it shortens also the elongated sacro-pubic line, contracts the ano-vulvar perineum to nearly its natural dimensions, and leaves ample vaginal aperture. He uses the slightly curved needle attached to a handle, employed by Peaslee in the same operation; and by the latter, Thomas, and others, in stitching up the abdomen after ovariectomy.

Plate XIV.—Ten Figs. Vesico-vaginal fistula, the various stages of the operation, literally according to the, in this country, familiar method of Sims.

Plate XV.—The instruments used in the operation for vesico-vaginal fistula, also devised by Sims and now employed by him and the majority of those who follow his teachings, or practice uterine surgery in England and America.

Plate XVI.—Ten Figs. Removal of Tumors connected with the uterus by gastrotomy, being the various stages in the operation of ovariectomy.

Plate XVII.—Eight Diagrams, representing various positions of the non-prolapsed uterus, and the relations of the pelvic organs with a uterine prolapsus artificially induced; and eighteen diagrams of the co-relations assumed by the pelvic organs and peritoneum resulting from various displacements.

This merely superficial review will sufficiently prove the value of the book. In our opinion, no physician practising uterine surgery, even though he may draw from the regular gynecological text-books all the information he desires, can dispense with the assistance afforded him by Dr. Savage's magnificent plates, the reputation of which was already so high during the first two editions as to render additional praise almost superfluous. P. F. M.

THE PATHOLOGY AND TREATMENT OF CHILD-BED. By DR. F. WINCKEL, formerly Professor and Director of the Gynecological Clinic at the University of Rostock. From the Second German Edition, with many additional notes by the Author. Translated by JAMES R. CHADWICK, M.D., Clinical Lecturer on Diseases of Women, Harvard University. Phila.: Henry C. Lea, 1876, pp. 484.

AFTER various perils by land and sea, this long-expected translation has at last appeared; and we congratulate the translator on his pluck and endurance in having conquered the difficulty caused him by the loss of the whole manuscript through the carelessness of English railway officials three years ago, which necessitated the reproduction of the whole work. At that time we heard of the accident from Winckel himself; and naturally concluded that it, together with the appearance of Dr. Fordyce Barker's "Puerperal Diseases" soon after, would put a stop to the enterprise, and render a second English work unnecessary. Now, however, on comparing the two books, it appears to us that one very well supplements the other, and that the translation of the German work is by no means superfluous. While we unhesitatingly believe that, practically, Barker's treatise is to be preferred; we find that in historical and statistical research Winckel in some degree supplies the defects intentionally left by Dr. Barker, as beyond the scope of his "Clinical Lectures." Although Winckel's book is essentially clinical (the various lesions being illustrated by fifty cases); still, with characteristic German thoroughness, the

author has given a large fund of literature in the text, and a bibliographical list at the end of each chapter, which will be found exceedingly serviceable to the obstetrical writer.

The introductory chapter contains a brief but exceedingly comprehensive account of the objective appearances in healthy lying-in women, of the normal state of the genital organs, lochia, breasts, pulse, respiration, temperature, functions of the kidneys, skin, and intestines, weight of the body; etiology of diseases of child-bed; mortality of women in child-bed; general treatment of puerperal diseases—points, many of which are scarcely touched upon in the majority of obstetric works, and which W. treats of in a highly instructive manner. The mortality of puerperal women in private dwellings is given at six to seven per 1,000 (2,412 deaths in 361,855 cases), that in lying in establishments as about 30 per 1,000 cases (24,201 deaths in 717,480 cases). Direct bloodletting from the puerperal uterus is dangerous, and need never be employed, as cold and leeches to the abdomen fully answer the purpose. The danger of forcible vaginal and uterine injections in the non-pregnant condition, and the value of intra-uterine irrigation during puerperal diseases is referred to at some length. The chapter on lacerations of the perineum is an excellent one, and exemplifies, as well as any in the book, the author's plan of giving, besides historical and statistical data judiciously interwoven with the text, only the bare facts and his own experience and views on each subject, without engaging in theories or suppositions. Advanced age seems to predispose primiparæ to this accident. "Immediate union of the torn edges is very essential," even when œdema or colpitis are present; daily enemata produce an equally favorable result, as constipation by opium, and the constant use of the catheter is ordinarily useless. W.'s experience with serre-fines is not particularly favorable; they cut through as rapidly as sutures, and leave small ulcers behind. Of the ruptures brought together by immediate suture, W. saw one half unite fully, and at least a third of the others healed to a great extent.

Fissures of the vestibule, near the meatus urinarius and clitoris, are evidently productive of profuse, even fatal hemorrhage, not generally alluded to in the text-books; lacerations of the vagina proper, of the vaginal cul de-sac, and of the uterus, and vaginal fistulæ (vesical and rectal), occupy the remainder of the chapter. W. agrees with the statement first made by West, in 1858, and corroborated by Simon, that the vast majority of urinary fistulæ of the vagina result from the too long postponement of instrumental interference, and are caused, not by the instruments employed, but by subsequent sloughing from too protracted pressure of the presenting part. Vesico-vaginal fistulæ occur once in 1000 births. The section on fissures and ruptures of the uterus is very cleverly handled; but, as we might expect, the importance of deep fissures of the cervix, although this lesion is mentioned, is neither appreciated, nor is any operation recommended for their cure. Emmet's valuable operation had not yet crossed the ocean in 1869.

In the chapter on *displacements of the vagina and uterus*, attention is called to the unrecognized frequency of these lesions during puerperality, and to the causation of retroversions and retroflexions by a protracted maintenance of the dorsal decubitus, and of all displacements, by the too great and too early use of the abdominal muscles. Rokitansky attributed flexions of the puerperal uterus to subinvolution of the placental site; and Martin found that out of fifty-five lying-in women afflicted with anteversion, twenty-five had indulged in coitus within six weeks post-partum.

The subject of post-partum hemorrhage, external and internal, free and parenchymatous (hæmatoma), is treated of in a chapter of 40 pages, without new suggestions. Thrombus vulvæ or vaginæ does not seem to be caused by varicose veins or instrumental delivery; it generally occurs spontaneously, during distention of the vagina by the fœtal head.

Puerperal salpingitis is quite rare, especially the real purulent form; and although pus is often found in the tubes in peritonitis, it does not follow that the purulent salpingitis was the primary lesion and caused the peritonitis, as

Martin and Buhl affirm. A direct spreading of the metro-lymphangitis to the peritoneum, without necessarily passing through the tubes, is quite plausible. Secondary peritonitis after purulent salpingitis is less common, according to Fischer, Hugenberger, Erichsen, and Winckel, than supposed by Martin and Buhl.

In puerperal peritonitis, cold to the abdomen is much preferred by W. to warm poultices and local depletions by leeches or wet cups. An excellent remedy for meteorism is painting the whole abdomen with Lugol's solution of iodine, twice a day, and covering it with a cold wet compress or ice-bag. Quite a lengthy chapter is devoted to the discussion of "*Diphtheritic inflammation of the vagina and uterus, with thrombosis of the lymphatics and diffused phlegmon, formerly known as metro-lymphangitis*," which the author appears to look upon as genital diphtheria proper, inasmuch as he details the bacteria-experiments of Waldeyer, Klebs, Oertel and others, without, however, committing himself to any belief in their agency as producers or merely carriers of the disease. Although seven years ago the position taken by W. was undoubtedly correct; still it would seem to us that, in adding the notes mentioned in the title, reference should have been made to the growing doubt as to whether the old forms of puerperal fever, known as "diphtheritic," really are true diphtheria, and not merely diphtheroid deposits. The immunity of the children from pharyngeal diphtheria in lying-in wards, where the so-called diphtheritic puerperal fever is raging, would certainly point to the non-identity of the two affections. That true genital diphtheria may occur in a puerperal woman, is, of course, unquestionable; but its real character will speedily be shown by the infection of the infant or attendants, or by the primary attack of the child from whom it was communicated to the mother. (See case by Dr. Pallen, *N. Y. Obs. Soc. Trans.*, this number, p. 483.)

Puerperal Thrombosis of the Lower Extremities, Phlegmasia alba dolens, may be divided into two varieties: 1. With thrombosis of the veins—primary local thrombi from pressure of the pregnant uterus on the iliac vessels, or secondary thrombi proceeding from thrombosis of the vena hypogastrica cruralis, spermatica interna, or of the placental sinuses. Periphrlebitis often precedes the thrombosis, and is followed by phlegmonous inflammation of the extremity, attributable to the purulent decomposition of the thrombus; or merely congestive hyperæmia ensues, which for a time produces an excessive œdematous swelling of the leg. 2. Without thrombosis of the veins, phlegmon of the thigh, with primary affection of the skin, of the subcutaneous and inter-muscular cellular tissue, in which the walls of the vessels (veins as well as lymphatics) may take part; yet sometimes secondary thrombi form within them, although the walls are then not always implicated.

"So-called *puerperal fever*," a brief but excellent historical sketch of which is given, is looked upon by W., in common with the majority of German obstetricians, as a disease caused by the absorption of septic material by a genital wound. Between the puerperal phlegmonous and phlebitic processes, whether mild or severe in character, and the diseases which so frequently supervene in case of wounds (septicæmia and pyæmia) no essential points of distinction can be made out. "Where the very significant term *puerperal fever* is applied now-a-days to the most severe diseases of child-bed, the phlegmonous and diphtheric (? REV.) affections of the genitals are thereby chiefly intended.

The important fact should be borne constantly in mind that the above-named processes do not possess any specific character, nor are they by any means restricted to pregnant, parturient, or lying-in women." Although auto-infection may occur, W. believes exo-infection, by the contact of septic (putrid) material from without, to be the ordinary mode of propagation of puerperal fever, and appears to incline strongly towards the cadaveric poison theory of Semmelweis. In view of his experience (the clearly proven transmission of the infectious matter resulting in puerperal fever by the hands of a student in the Rostock lying-in establishment from puerpera to puerpera, which student had not only examined a puerpera with "diphtheritic" ulcers of the vagina, but had been constantly engaged in arranging alcoholic prepar-

ations, dissecting, and operating on the cadaver; and the sudden cessation of the endemic when that student was forbidden further examination of pregnant or lying-in women), Winckel has come to the conclusion, "that puerperal fever originates as an endemic in lying-in establishments, by means of a direct transfer of the infectious (purulent, putrid or diphtheritic) matter from one puerperal woman to another; that the beginning of such endemics may be sometimes referred to a case of spontaneous origin; but frequently also to infection with some one of these morbid matters. . . . These septic puerperal affections are then also *contagious*, in so far as they engender a specific miasma, although the disease thus induced by the contagion, is, as a rule, not perfectly identical with the primary malady."

In the chapters on *Lactation*, a peculiar neuralgic or rheumatic affection, occurring very commonly in debilitated women, or such who have been nursing too long; is described, viz., cramps and muscular contractions, especially in the muscles of the upper extremities and neck, recurring ten to twenty times a day, lasting from a few minutes to several hours, and often excited by putting the child to the breast. Tonics and weaning the child usually produce a rapid cure. Winckel denies the existence of "milk-fever," and says that simple physiological hyperemia of the breast during the inception of lactation is not necessarily attended by any febrile elevation of temperature, a sub-febrile temperature (101° F.) at the most being excited. A genuine fever is met with only where sore nipples, incipient mastitis or genital fissures, etc., are present.

W. also is an adherent of the theory of Traube, Munk, and Rosenstein, that *eclampsia* is due to anæmia of the brain, produced by cerebral œdema, which owes its origin to an increase in the quantity of serum in the blood, and the consequent augmented tension in the arterial system. Still he asserts that he has met with numerous cases in which this theory was not satisfactory; without, however, endeavoring to offer another explanation.

The chapters on *Mental Affections* and on *Skin Diseases of Lying-in Women* close the book. W. considers puerperal scarlatina to be extremely rare; and expresses the belief that those authors who assert that puerperal women are more liable to scarlatina than other adults, and that at this time it more frequently assumes an epidemic form (Berndt, sen., Dalfitt, Eisenmann, Hodge) have had to do, not with primary scarlatina, but rather with the inflammations of the integument that accompany septic puerperal infections. This supposition is naturally strengthened by the knowledge that scarlatina occurs as a rule but once in the human subject; wherefore we should be inclined to look upon a scarlatiniform eruption in a lying-in woman, who has previously had the disease, and in whose case contagion can be excluded, as not real scarlatina, but rather a traumatic or symptomatic dermatitis, which has nothing in common with scarlatina but the color.

In closing this review, we wish to compare this work, as regards clearness and brevity of style, and variety and multiplicity of matter, with Schroeder's well-known "Manual of Midwifery;" in our opinion sufficiently high praise. The translator has done his work with care and fidelity, and we do not remember noticing any of the not always easily avoidable Germanisms. Of the typography and general appearance of the book it is unnecessary to speak; they, as in all publications of the firm, are excellent. P. F. M.

TRANSACTIONS OF THE OBSTETRICAL SOCIETY OF LONDON. Vol. XVII., for the year 1875. London: Longmans, Green & Co., 1876, pp. 400. With a Catalogue of the Library and Museum, pp. 151.

THIS annual volume contains papers and reports of cases by numerous Fellows, among whom the names of Braxton Hicks, Barnes, Spencer Wells, Routh, Wynn Williams, Ashburton Thompson, Heywood Smith, Lawson Tait, Alfred Meadows, are the most prominent. The famous discussion on Puerperal Fever occupies 168 pages of the volume; and, although the question was still left pretty much *in statu quo*, still forms one of the most important recent contributions to the literature of that affection. Of the papers, those of chief interest

are the following : "Clinical Notes on the Early Cause of Cancer of the Cervix Uteri," by Dr. Charles Liebman, of Trieste ; "On the Occurrence in Normal Labor of Lateral Obliquity of the Head," by A. L. Galabin ; "Note on a Diseased Placenta," by Lawson Tait ; "Note of a Case of Ruptured Vagina during Labor with Recovery," by Heywood Smith ; "Three Cases of Cephalotripsy," by Braxton Hicks ; "On the Treatment of Chlorosis and Anæmia with the Phosphide of Zinc," by Ashburton Thompson, and others which our limited space forbids us to mention.

LECTURES ON ORTHOPÆDIC SURGERY AND DISEASES OF THE JOINTS. Delivered at Bellevue Hospital Medical College, during the Winter Session of 1874-1875. By LEWIS A. SAYRE, M.D., Professor of Orthopædic Surgery. Fractures and Dislocations, and Clinical Surgery, in Bellevue Hospital Medical College, etc., etc. Illustrated by 274 Wood Engravings. New York : D. Appleton & Co., 1876, pp. 476.

No recent book has filled a more evident void in medical literature, or met a greater necessity than this publication of the extensive experience of the author during the last quarter of a century. The name of Lewis A. Sayre is so intimately connected and identified with orthopædics in all its branches, that a book relating his experience cannot but form an epoch in medical science, and prove a blessing to the profession and humanity. Dr. Sayre's views on many points differ from those entertained by other surgeons, but the great successes he has obtained fully warrant him in maintaining the "courage of his opinions."

Although deformities, when not congenital, occur frequently in adults, the majority of cases reported by Dr. Sayre are those of children ; and for this reason we feel called upon to review at least a few of his chapters, as far as our space will permit.

The two affections, in connection with which the author's name is perhaps most widely known, are Talipes and Morbus Coxarius—Club-Foot and Disease of the Hip-Joint. One additional pathological condition, to which the author's attention was first called in 1870, should first be referred to, as, in point of importance and hitherto obscure origin, it probably equals the above-named deformities, viz., reflex muscular contractions, and even paralysis of the lower extremities, followed by deformity, and caused solely by the genital excitation dependent on congenital phymosis and adherent prepuce. Dr. S. reports three cases of this affection which were entirely cured by circumcision.

The discussion of Talipes occupies five lectures. The nature of the subject and the complicated character of the various mechanical appliances for its relief (Sayre's adhesive plaster-bandage, artificial muscles, club-foot shoe, etc.), in the proper employment of which, besides tenotomy, the treatment mainly consists, manifestly precludes a more detailed review of the deformity here. We will merely mention that, according to Dr. Sayre, the treatment of a talipes should be commenced the instant the child is born (before the physician leaves the lying-in house), or immediately upon receipt of the injury ; the simple fan-shaped adhesive plaster bandage, with rubber muscle, the immovable felt, sole-leather, or gutta-percha splints, or the gypsum bandage, are all recommended, the first named being applicable to perhaps a large majority of congenital deformities ; and care should be exercised not to obstruct circulation by the dressing, but to restore the normal position gradually. Manipulation (shampooing) before applying the dressing, will aid much in strengthening the muscles and rectifying the deformity.

Although diseases of the ankle and knee-joints occupy six lectures, a fact which sufficiently attests the thoroughness with which they are handled, we feel compelled to pass them over, and refer briefly to that principal disease of the joints, *hip disease*, to which five lectures are devoted. This affection may fitly be looked upon as the author's pet ; and where else, indeed, can we find a record of 59 cases of exsection of the hip-joint for morbus coxarius, with 47 recoveries, 12 cases dying, and of those only 8 from the exhausting effects of

the joint affection ; the remaining 4 from acute intercurrent diseases (tetanus, double pneumonia, dysentery, and sunstroke)? We are aware that prominent orthopaedic surgeons condemn the excision of the diseased head of the femur, and profess to cure all cases (*which* they cure) by extension and mechanical appliances. From a rational stand-point, however, we cannot help asking, what better treatment can there be for a diseased or dead bone than its operative removal? and surely Dr. Sayre's results abundantly prove the correctness of his views and practice in coxitis. By means of perfect rest and proper extension (fully obtained in the "wire-breeches" apparatus) great comfort is obtained, and comparative slight shortening results. In one case, as we have ourselves seen, the use of the excised joint and limb is perfect, in every respect equal to that of the sound leg. The "wire-breeches" apparatus permits the transportation of the patient from place to place, either for exercise and fresh air, or for travel, or he may be stood up against the wall, and thus relieved from the recumbent posture.

The lectures on Diseases of the Joints Simulating Hip-disease, on Diseases and Deformities of the Spine, Anchylosis, etc., we are compelled to pass over without discussion.

One of the pleasing features of the work are the numerous woodcuts, which are almost indispensable to the easy comprehension of the majority of the deformities, and the more or less complicated apparatus employed in their treatment.

P. F. M.

AN ELEMENTARY TREATISE ON DISEASES OF THE SKIN. By HENRY G. PIFFARD, A.M., M.D. London and New York : Macmillan & Co. Pp. 375.

WITHIN the past few years a dozen or more English writers have published treatises on diseases of the skin. The majority, however, have been imperfect abridgments or mutilations of a few commendable publications. The book before us is the first text-book of any worth by an American dermatologist. It purports to be elementary in character, but yet its 375 pages present a clear exposition of the leading features of the more common skin affections, and embody a considerable amount of original work. The author adopts an etiological classification; frankly admitting, as every one must, that there is a large number of skin affections of an uncertain nature. As will be readily seen, he is a strong advocate of the French doctrine of the diathetic nature of affections, such as eczema, psoriasis, etc., which he groups under the head of "Rheumides," *i. e.*, manifestations of the "*rheumic*," or dartrous diathesis.

Whether the reader be inclined to agree with the author on this point or not, he will certainly be struck with the forcible manner in which the writer's view of this vexed question is presented.

The introduction of five photo-micrographs, in addition to original and well selected woodcuts is a new feature in dermatological literature. The typographical work is of unusual excellence; and the book, though by no means perfect, is well suited to the wants of the American student and practitioner, for whose use it is designed.

G. H. F.

A TREATISE ON THE DISEASES OF THE NERVOUS SYSTEM. By WILLIAM A. HAMMOND, M.D. With 109 illustrations. Sixth Edition. Enlarged and improved. New York : D. Appleton & Co. 1876. Pp. 883.

AN INTRODUCTION TO PATHOLOGY AND MORBID ANATOMY. By T. HENRY GREEN, M.D. London. Second Amer. from Third Revised and Enlarged English Edition. 111 woodcuts. Phila. : Henry C. Lea. 1876. Pp. 316.

TRANSACTIONS OF THE PATHOLOGICAL SOCIETY OF PHILADELPHIA. Vol. V. Edited by JAMES TISON, M.D. Pp. 258.

A volume exceedingly creditable to the Society as regards size, nature of contents, and general appearance.

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ORIGINAL COMMUNICATIONS.

THE OVULATION THEORY OF MENSTRUATION: WILL IT
STAND? ¹

BY

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It is my purpose, in this paper, to examine the evidence upon which is based the ovular or ovulation theory of menstruation.

This subject has received a large share of attention during the past few years, and great discrepancy of opinion exists in regard to many points connected with it. I do not expect to reconcile these discordant views; indeed, in the present state of our knowledge, it would perhaps be impossible to do so. Our great need in this, as in many other problems of a physiological character, is an increased number of well-observed facts; those which we have thus far are too few in number,

¹ Read, by abstract, before the Illinois State Medical Society, at its annual meeting, May 17, 1876.

and, apparently, too contradictory to warrant a definite and entirely satisfactory conclusion.

The progress of scientific knowledge is greatly retarded by the admission of what may be termed, paradoxically, false facts; that is to say, facts which by representing only partially the truth, lead to false results. Conclusions founded upon such premises must almost necessarily be erroneous. Truth in science is rarely found wholly unmixed with error, and, in order that we may rightly appreciate the former, we must properly estimate the latter also. Like the diamond, whose facets reflect a differently colored ray according to the angle from which they are seen, so may a scientific truth present a different aspect to those who behold it from different points of observation. While to one person the gem appears green, to another it is red, and to a third, yellow. He only who sees it from all directions can know the whole truth. Thus it is that imperfectly-observed, partial, or perverted facts result in the formation of a false theory; and a false theory, once adopted, has a most injurious influence. He who is governed by it sees everything through a false medium. As observed by Paris, "He who is guided by preconceived opinions, may be compared to a spectator who views the surrounding objects through colored glasses, each assuming a tinge similar to that of the glass employed." The advocates of the ovulation theory are, it seems to me, somewhat in this position. Many facts have been observed which give apparent support to their opinions, and on these they have been content to rest, overlooking, or, at least under-estimating other facts, equally well known, which strongly militate against those opinions.

The ovulation theory of menstruation implies the following essential propositions:

1. At regular periods, of about twenty-eight days, in the human female, a matured ovule is discharged from the ovary, passes into the Fallopian tube, and is transmitted to the uterus.

2. Coincident with, and dependent upon the maturing and bursting of the Graafian vesicle and the extrusion of the ovule, certain changes occur in the mucous membrane of the body of the uterus, which result in a sanguineous discharge from that organ.

In support of these propositions, evidence consisting of certain facts and analogies has been adduced, as follows: (a) Observations made on the bodies of women who have died during, or soon after the menstrual period, have revealed the presence in one or other ovary of a ruptured Graafian vesicle, and its cavity filled with a blood-coagulum, or, its remains, a corpus luteum, in various stages of development or decadence; (b) Physiologically, the period of menstruation in woman corresponds with the rut or œstrus of other mammalia, when, it is well known, ova are discharged from the ovaria; (c) The artificial removal of the ovaries causes an immediate cessation of the menstrual function. I purpose considering, *seriatim*, these propositions, together with the facts which have been advanced for their support.

1. *At regular monthly periods, in the human female, and coincident with the monthly flow, an ovule is discharged from the ovary, is received into the Fallopian tube, and by it transmitted to the uterus.*

The minor proposition, namely, that the matured ovule passes from the ovary to the uterus through the Fallopian tube is admitted on all hands, and, not being in dispute, need not detain us. The essence of the controversy centres in the alleged periodicity of this process, and of its time relations with the menstrual discharge.

The Graafian vesicles, from the time of their description in 1673, by De Graaf, down to the year 1827, were thought to be the actual ova of mammalia. It was not until the last named period that Baer discovered the true ovule and the relations it bore to its containing vesicle. However, as early as 1672, Kerkringius¹ advanced the idea that ova were discharged at the time of menstruation, but it does not seem to have been founded upon any observations. The first writer who gives positive evidence upon the subject is Sir Everard Home, who noticed the ruptured follicle during menstruation, although its import was not then understood. In 1821, Dr. Power clearly enunciated the doctrine of the periodical ripening of the follicle at the menstrual period; and the discovery six years later

¹ Tyler Smith, Lectures on Obstetrics, third edition, p. 80.

by Baer, already alluded to, that this was only the enveloping structure of the ovule and not the ovule itself, made the rupture an intelligible fact; and so we may regard this as the real birth of the ovulation theory. In 1831, Negrier,¹ working independently, showed by anatomical preparations that the periodical discharge of menstruation was the consequence of an internal hidden function—ovulation. Fresh proofs were brought forward by Gendrin, Paterson, Barry, Raciborski, Bischoff, Pouchet, and others, all tending to show that ovulation and menstruation are simultaneous and necessarily connected one with the other; and the doctrine was so beautiful and reasonable, and seemed so well sustained by the evidence adduced, that we cannot wonder at the fact that it was generally received and adopted by physiologists. Still, there have always been some who were not convinced of its correctness, and who regarded the proofs alluded to as insufficient and inconclusive; who, in the language of Mr. Kesteven,² looked upon the doctrine “as a plausible and ingenious theory, wanting, however, in the true elements of an inductive theory; in short, an example of the *post ergo propter* line of argument.”

In examining the cases which have been cited in support of the ovulation theory, one cannot fail to be struck with the complacency with which conclusions are frequently drawn from irrelevant, or, in some instances, even adverse facts.

An example of this is to be found in a review of Bischoff's work on Human Ovulation, in the *American Journal of the Medical Sciences*, vol. 28, p. 137.

These cases of Bischoff have always been regarded with especial favor and as of great value by the advocates of the ovular theory; and, as they are frequently alluded to, I feel constrained to present a very brief synopsis of them.

The observations³ were thirteen in number. Of these, the time of the menstrual period was known in only ten; the remaining three have therefore no value so far as this inquiry is concerned. Of the ten, three died during menstruation, and in each of these there was found a ruptured follicle. A fourth died two days after menstruation; the right ovary contained a

¹ Recueil de Faits pour servir à l'Histoire des Ovaires. Angers, 1858.

² Lond. Med. Gazette, 1849.

³ Beiträge zur Lehre von der Menstruation und Befruchtung, 1853.

pretty large projecting follicle, which was *still closed*. Both ovaries contained small corpora lutea. In a fifth case, the menstrual period had just passed; the left ovary contained a very distinct corpus luteum, and the right ovary a ruptured Graafian follicle filled with fresh blood. Number six died seven days after menstruation; in the right ovary was a recent corpus luteum. In the seventh, death occurred ten days after menstruation; the right ovary contained a very large Graafian follicle *unopened*. Number eight died ten days after menstruation; the right ovary contained a *recently ruptured Graafian follicle and a fresh corpus luteum*. In number nine, menstruation had occurred eighteen days before death; the right ovary contained a very large corpus luteum. Lastly, number ten died four weeks after menstruation; the right ovary contained a ruptured Graafian follicle.

After detailing these cases, the reviewer says: "The results here obtained show that in the human female, at each menstrual period, a Graafian follicle is ripened, swells, and usually bursts, discharging an egg,¹ and forming a corpus luteum." Now, I submit to any candid inquirer that the cases cited do not show these things. Indeed, so far as they prove anything, it is that there is not even an approximate correspondence between the rupture of the follicle and the menstrual period. In two of the cases menstruation had occurred without any such rupture at all. In Cases 5 and 8 the ovaries are described as containing a fresh corpus luteum *and* a recently ruptured follicle; yet, in the one case, menstruation had "just passed," and in the other, had ceased ten days before death. Inasmuch as a corpus luteum is an older formation than a recently-ruptured follicle, we should naturally refer the latter to the last menstrual period. But to what period or periods do the *fresh corpora lutea* belong? Dalton² says that the corpus luteum of menstruation "reaches its greatest development about three weeks after ovulation, and from this time rapidly disappears, a small cicatrix only remaining." This being the case, can we refer these "fresh" corpora lutea to a menstrual period, in one case thirty and the other thirty-eight days past? If we can do

¹ Notwithstanding the most diligent search, Bischoff was unable, in a single instance, to discover the ovule.

² Prize Essay "On the Corpus Luteum of Menstruation and Pregnancy."

so—if a recent corpus luteum signifies one which may be associated with a menstruation which occurred four to six weeks before—then what shall we say of Case 6, where the presence of such a one is connected with a period *seven days* past?

Barnes,¹ too, with a similar disregard of consistency, after stating that the preparations of Coste, preserved in the College of France, prove that the ripening of a Graafian follicle always coincides with the turgescence of the genital organs, and, according as the circumstances are more or less favorable, bursts at the commencement, towards the end, or at any time during the menstrual discharge—proceeds to state what these preparations are in detail, thus: “In a woman who died on the first day of the appearance of the menses, the ovarian vesicle was manifestly ruptured. In another, who died four or five days after the cessation of the menses, the right ovary presented a vesicle still intact, but so distended that the slightest pressure made it burst. Lastly, in a young virgin, who died fifteen days after menstruation, there was no recent trace of a yellow body, and it could not be doubted that the Graafian vesicle had been arrested in its development.” Surely, these cases, taken together, so far from proving that a Graafian follicle bursts at the menstrual period, show that menstruation occurs, in two-thirds of the cases, without such rupture, and in one-third without even a maturation of a follicle. For in one only of the three was there actually found a ruptured follicle; in one, menstruation had occurred and ceased several days before without any rupture, the follicle being burst by external force *post-mortem*, and in the third there had not been even the ripening of one!

Such a course as is indicated in the foregoing instances is more reprehensible than reasoning without facts; it is reasoning against them. Yet these are only samples of the sort of argumentation which is frequently found in connection with this subject.

(A) MENSTRUATION MAY OCCUR WITHOUT ACCOMPANYING OVULATION.

At first glance it would seem that we ought to accept the discovery of a rent follicle filled with blood, in persons who

¹ Diseases of Women, p. 147.

have died during menstruation, as a proof that the flow is, if not the result of, at least coincident with, ovulation. But such evidence is not at all conclusive, and may be erroneous, for Ritchie¹ has "repeatedly seen the opening of a discharged vesicle to be still patent, and sometimes the vesicle to be filled with a florid blood-clot in the third and fourth month of pregnancy; and, in one case, he found the corpus luteum of a woman in the ninth month to communicate with the surface by a distinct foramen."

We must bear in mind, in the consideration of this subject, that Graafian vesicles are maturing and rupturing, and corpora lutea are forming and disappearing continually; hence it should be expected that, in a woman dying at almost any time, some of these conditions would be found in the ovary. And when we further consider that the menstrual periods occupy from one-sixth to one-fourth of a woman's lifetime for thirty years, we may equally expect to find such ovarian changes at these periods also. So that it seems strange, indeed, that persons who are satisfied of the correctness of the ovular theory because occasionally a ruptured follicle or a corpus luteum is found coincident with a menstrual period, should ever have a lack of evidence. And yet, abundant as such evidence unquestionably is, it does sometimes fail; for menstruation frequently occurs without any such contemporaneous change in the ovary. Many instances of this character have been recorded, but it is only necessary to call attention to a few of them.

Dr. W. W. Gerhard² presented to the College of Physicians of Philadelphia the uterus and appendages of a multipara, twenty-five years old, who died of apoplexy during a menstrual period. "At several points on the surface of the ovary there were minute dot-like orifices, each one corresponding to a Graafian follicle. Two of these being examined under the microscope, were found to present a few granular nucleated cells floating in a homogeneous liquid." Although this woman had been the subject of menorrhagia, the discharge latterly returning profusely every two weeks, there was no evidence of any recent ripening or rupture of a follicle. Again, Dr. Stedman³

¹ Tilt, *Uterine and Ovarian Inflammation*, p. 66.

² *Amer. Jour. Med. Science*, vol. 36, p. 410.

³ *Amer. Jour. Med. Science*, vol. 24, p. 83.

has reported the case of a married woman, forty-five years of age, who died of some pulmonary affection. Menstruation was regular nearly to the time of her death, and yet on examination there was found no trace of the left ovary, but in its place a thin and simple serous cyst nearly two and a half inches in diameter; while on the other side there was a collection of cysts, forming a mass twice the size of an English walnut, upon the surface of which were spread out the thin flattened atrophied remains of the ovary.

Furthermore, it is the experience of ovariologists, that in many cases in which both ovaries have been removed, these organs have been found so thoroughly diseased as to preclude the idea that they could possibly have performed their function of ovulation normally, if at all, and yet the regularity of menstruation has suffered no interruption.

Some of the reported cases of hernia of the ovaries throw valuable light upon this question. For example, Dr. Oldham¹ presented one to the Royal Society, the subject of which was a tall, well-formed woman, nineteen years of age, in whom both ovaries had descended through the inguinal canals, and occupied positions in the upper part of the labia majora. The mammae and external genital organs were well developed, but neither uterus nor vagina could be detected. The left ovary was in a quiescent state, and had never been the seat of pain or swelling. She was under Dr. Oldham's observation six years, during which time he had frequent opportunities of seeing her. For the first three years the right ovary was exclusively enlarged, the intervals varying from three weeks to three months. For the last two years the left ovary was most frequently affected, the right remaining quiescent. Occasionally both were tumid, but one always more so than the other. The swelling sometimes occurred suddenly, although usually it was gradual, the volume of the organ increasing slowly for four days, remaining stationary for three days, and then slowly declining, the whole process lasting ten or twelve days. During this period the organ was tender when pressed, but was otherwise not painful, and did not interrupt the patient's ordinary duties. There were no manifest sympathies excited in the mammary glands

¹ Amer. Jour. Med. Science, vol. 35, p. 284.

or other organs ; and there was no vicarious flux, either of blood or other secretion. The ovary alone seemed engaged in the act. It was supposed, reasonably, that these periods of enlargement were those of ovulation, and I beg to call attention to the fact that they were quite irregular in their occurrence, the intervals varying from three weeks to as many months.

Dr. Alfred Meadows,¹ also, mentions a case of similar character. The patient was a single woman, twenty-three years of age, who began to menstruate at fifteen, and continued doing so at regular intervals, with some pain, down to the age of twenty, when, after stooping, a swelling suddenly appeared in the right inguinal region, caused, as was subsequently learned, by the prolapsed ovary. At the menstrual period following this she suffered violent pain of a character different from any she had experienced before ; it preceded the hemorrhage ; at the same time the tumor was much increased in size. From that time on she suffered in a similar way, sometimes more acutely, so that at every monthly period she was obliged to lie in bed for a week or more. Sometimes the tumor would swell up to the size of "two fists," and be exquisitely tender to the touch. She had no suffering during the inter-menstrual periods. To what does this history point ? Are we not prepared to accept it as an evidence of the truth of the ovular theory ? Prior to the occurrence of each monthly flow, for a period of three years, the ovary enlarges, becomes the seat of pain, and the swelling does not subside until after the cessation of the discharge. What more ought we to require ?

Mark the sequel. At the suggestion of Dr. Meadows the tumor was removed. It was not contained in any cyst or sac, and was readily separated from its fatty and cellular attachments. The upper portion or pedicle, which went through the abdominal ring, was found distended with fluid. This was punctured and about an ounce of the contents let out. The pedicle was then tied and the tumor removed. "The tumor, which measured about two inches in diameter, proved, on section, to be the right ovary. It had, however, undergone remarkable structural change. Instead of presenting the usual dense compact appearance, it contained throughout numerous irregu-

¹ Amer. Jour. Obstetrics, etc., vol. 6, p. 231.

larly-shaped spaces varying in size from a pin's head to a quarter or even half an inch, and all were filled with the same kind of fluid as flowed from the pedicle. These cells appeared to communicate with one another, and the whole organ to be infiltrated, as it were, with the fluid in question. There were no proper Graafian vesicles to be seen." No Graafian vesicles,—no ovules—no ovulation, to account for the great increase in the size of the ovary preceding and during the catamenial period. What is the plain inference? Is it not that the swelling of the ovary was caused by the pelvic congestion attendant upon the menstrual period? And yet, Dr. Meadows has introduced the account of this case in an argument affirming, among other things, the dominating influence of the ovaria, and the fact of ovulation producing the menstrual flow.

This last mentioned case of ovarian hernia, as also one reported by Dr. McCluer,¹ and another which I am informed² has been published by Dr. Joseph English, of Vienna, shows that the cystic degeneration of the prolapsed ovary, even when its essential vesicular structure is wholly destroyed, does not prevent the organ from enlarging and becoming painful during the menstrual epoch. In all of these cases the ovary, which had swollen month after month at regular periods corresponding with the menstrual flow, was found to be so diseased as to leave no vestige of Graafian vesicles, thus proving ovulation to be impossible.

Dr. Tilt³ says: "In three cases in which Dr. Ashwell had opportunities of examining the ovaria of women who died during the flow of the catamenia, there were no signs of the rupture of the Graafian vesicle and the escape of ovules. In one of these cases, the woman had menstruated regularly for several years, and yet the ovaria were perfectly smooth; there was neither rent nor cicatrix marking the site of either a present or former maturation and escape of a Graafian vesicle." Ritchie³ also reports five examples of menstruation which were not accompanied, and could not have been caused, by ovulation. In one of these, the woman died ten days after menstruation. The ovaries were filled with vesicles, but neither of them pre-

¹ Amer. Jour. Obs., vol. vi., p. 613.

² Dr. Paul F. Mundé, private letter.

³ Ovarian Phys. and Path., London, 1865.

sented either a puncture or cicatrix. In another, death occurred thirteen days after menstruation. Here, too, the ovaries contained numerous vesicles, one as large as a garden pea, but in neither of them was puncture or cicatrix. In a third, menstruation had occurred a week before death; but there was neither scar nor opening on the surface of either ovary. Again, in another case where death occurred a fortnight after menstruation, neither ovary presented any sign of recent rupture.

Dr. John Williams¹ has published a series of cases bearing upon the temporal relations of the discharge of ova with the menstrual flow. He believes that the ova are discharged, usually, before the appearance of the catamenial flux, and details observations made by him upon sixteen cases. In several of these, where death occurred during the inter-menstrual period, his conclusions are drawn from the condition of the corpus luteum; but inasmuch as the changes in these bodies do not always take place uniformly, and, as it is always difficult to determine the age of effused blood, the results founded upon these cannot be accepted as certainly accurate. Passing by these, therefore, I wish to invite attention to those of his cases in which death occurred during the menstrual period.

(1.) Was a young woman who died on the fifth day of the flow. "On the surface of the left ovary was a rough, brownish-colored, star-like cicatrix. On section there was seen under the cicatrix a corpus luteum dilated in the middle and narrow at both ends, nearly three-quarters of an inch in length and half an inch in width; its walls were in some parts of a pinkish, and in others of a yellowish color. In the centre was a partially decolorized clot." (2.) Was a patient who died on the ninth day of typhoid fever and the fourth day of menstruation. One ovary contained a corpus luteum, similar to that in case No. 1. In both of these rupture of the follicle had taken place evidently several days before. (3.) Woman had undergone operation for fistula-in-ano, and died five days after the appearance of menstruation; one ovary contained a follicle five-eighths of an inch by one-third of an inch, in which was found a bright-red, fresh, loose clot, and its walls were thin and smooth. *No rupture had taken place.* (4.) Patient with fibroid tumor

¹ Obstet. Jour. Great Britain and Ireland, vol. 3, p. 620.

of the uterine; died on the third or fourth day of menstruation. Left ovary contained a follicle nearly an inch in length, in which was found a soft, dark-colored clot, which appeared to be several days old; *follicle had not ruptured*. (5.) Patient died when the menstrual flow had almost ceased. *There was no rupture in either ovary*, but the right ovary contained a Graafian follicle about the size of a small pea. (6.) Young suicide; died three days after cessation of the flow. *There was no recent rupture in either ovary*; the left contained a follicle similar to the preceding.

Of the foregoing six cases, in only two did a ruptured Graafian vesicle even seem to correspond with a menstrual period; and in two of the cases, the follicles most advanced were so immature that Dr. Williams expresses the opinion that they would probably have ripened by the next return of the flow.

Mr. Paget¹ has reported a case of a woman who was executed for some crime, and the *post-mortem* appearances tell very forcibly against the ovular theory. The woman had begun to menstruate twelve hours before her execution. "The ovaries were of moderate size and presented numerous marks of cicatrices upon their surfaces. In the right ovary, three Graafian follicles projected slightly on the surface and looked healthy, containing clear serous fluid. A fourth was of very large size and prominent. In the left ovary, one Graafian follicle was fully developed and prominent. We looked for ova in the contents of all these, but in vain. The surface of the ovaries was generally rather more than usually vascular, but there was no peculiarly vascular spot, nor any appearance of the recent rupture of a vesicle, or the discharge of an ovum. In the right ovary, near the surface, was a small cyst or cavity, containing what looked like a decolorized clot, and bounded by a thin layer of a bright yellow-ochre substance, an excellent example of a fibro-corpus luteum, of one or more months' date, certainly not more recent."

Dalton, in the essay on the Corpus Luteum, already referred to, reports two cases, in one of which death occurred during the menstrual period and in the other at its termination. In neither

¹ Tilt's Uterine and Ovarian Inflammation, p. 64.

of them had a follicle recently ruptured, although in the second there was one on the point of doing so.

I have had two opportunities of examining the ovaries of women who died at or near the menstrual period. One was the case of a healthy unmarried woman, twenty-eight years of age, who died from an overdose of morphia, taken accidentally. She had menstruated regularly, and a period had ceased four days before death. Both ovaries were normal in structure and size, the right being somewhat larger than the left. It contained several Graafian vesicles scattered throughout the stroma. Two of these were larger than the rest, one being about an eighth of an inch in diameter, and the other as large as a small currant. This latter was near the surface and caused a slight projection. It contained a clear serous fluid. The left ovary contained fewer vesicles, but had the indistinct remains of a corpus luteum not less, certainly, than four or five weeks old. The mucous membrane of the uterine body was pale and covered with a grayish-pink mucus. The other case was that of a young girl, fifteen years old, who died from the effects of a burn. She had commenced menstruating eighteen months before, but the function had been regularly performed only for about ten months. A period had ceased twelve days prior to death. Neither ovary contained corpora lutea, nor bore the marks of recent rupture. The largest vesicle, which was about a quarter of an inch in diameter, was found in the left ovary about a sixteenth of an inch from the surface.

(B) OVULATION MAY OCCUR WITHOUT ACCOMPANYING MENSTRUATION.

I will next proceed to adduce evidence to show that ovulation certainly and frequently takes place without menstruation.

Malpighi and Vallisneri long ago observed that fully-developed Graafian vesicles are occasionally found in the fully grown foetus. Ritchie,¹ also, has demonstrated by at least ten dissections, that in the ovaries of new-born infants, and children as early as the sixth year, may be found highly vascular Graafian vesicles; and that at the age of fourteen, and prior to menstruation, they are found as large as small raisins, filled with their usual transparent granular fluid; that menstruation is not essen-

¹ Loc. cit., p. 62.

tial, either as cause or effect, of these conditions ; that prior to menstruation, the vesicles are found, as at every other period of life, in continual progression towards the circumference of the ovaries, which they penetrate, discharging themselves through the peritoneal coat, thus proving that the catamenial flow is not an indispensable prerequisite to their rupture.

So, likewise, the more recent researches of Grohe, Slavjansky, and Haussmann have shown that the growth of the Graafian vesicle is quite independent of the menstrual period ; and the last named authority,¹ whose observations were made upon eighty-four subjects, asserts that such early development of the follicles as was noticed by Ritchie takes place in about ten per cent. of all cases. Dr. Sinety² confirms these observations, and maintains that "in the ovaries of the newly-born, Graafian follicles are almost always visible to the naked eye ; and they may at this time often be discovered as well developed as in the adult female, and constituting true cystic ovaries, in which are to be seen ovules whose origin is indubitable. In the ovaries of infants, there are often cicatrices and follicles in different stages of atrophy." What, I would ask, causes these cicatrices ? Is it aught but the rupture of the Graafian follicles which, as would seem from the foregoing, may take place at any period of infantile and adult life ?

Slavjansky,³ who has devoted a great deal of time to researches on the physiology and pathology of the ovaries, thus summarizes the results obtained by him :

1. The Graafian follicles develop themselves from the primordial follicles, and are growing towards maturity from the first month of birth to the fortieth year.
2. The larger number of follicles do not mature, do not rupture, do not discharge their contents, but pass over into a condition of atrophy which is analogous to the formation of the corpora lutea.
3. The development and ripening of the Graafian follicles do not take place periodically in a regular manner, and there is no connection between ovulation and menstruation.
4. Menstruation is a physiological phenomenon unconnected with the development and ripening of the Graafian follicle.
5. The rupture of the

¹ Centralblatt, No 2.

² Le Progrès Médical.

³ Allg. Med. Centr. Z. 54, 1874.

more or less ripe follicle is associated with congestion of the genital organs, and is, as yet, an unexplained matter.

On the other hand, ovulation may continue after the menopause. Lawson Tait¹ says: "The cessation of the menses at the climacteric, though it diminishes the activity of the cell-growth at once to a marked extent, never extinguishes it; for the development and extrusion of immature Graafian follicles ceases only with life itself. They are to be found of some size even fifteen or twenty years after the cessation of menstruation."

Of course, ovulation is the necessary condition of impregnation, and it is admitted by all writers that conception may occur in the absence of menstruation. Our literature contains many instances of girls who have conceived prior to the first appearance of the flow; of women who have become pregnant subsequent to the menopause, and during lactation before menstruation has reappeared. Dr. James Young,² Tanner,³ Dubois,⁴ Tilt,⁵ and, indeed, almost every obstetric author, mention cases of this character. Leishman speaks of a woman who married at twenty-seven, and who menstruated the first time two months after her eighth labor. Raciborski states that he has seen on the ovaries one or two cicatrices, although the subjects had never menstruated.

An argument which has been frequently urged in support of the ovular theory, is the fact that conception is more likely to take place shortly after a menstrual period than at any other time.

Dr. W. H. Studley,⁶ alluding to this, considers it as admitting of a very different explanation, and not at all as proving the coincidence of ovulation and the menstrual flow. He says: "My opinion in regard to the rationale of the fact is this: impregnation is more likely to be secured at this time because of the recent deluging with menstrual blood, by which the secretions, especially of the cervical canal, have been washed away, which secretions often prevent impregnation either by their

¹ Hastings Prize Essay of 1873, London, p. 4.

² Am. Jour. Med. Science, vol. 60, p. 568.

³ Hand-book of Pract. Obstetrics, p. 24.

⁴ Journal de Méd., 1850.

⁵ Uterine and Ovarian Inflammation, p. 49.

⁶ Amer. Jour. Obstetrics, 1875, p. 487.

chemical incompatibility with the vitalizing fluid, or by the mechanical obstruction in the form of the firm mucous plug so often found in the canal."

If the ovular theory were true, conception could take place only at or near a menstrual period; but there is abundant evidence to show that it may and does frequently occur at times quite remote from it. My own experience has furnished me with a number of instances where married women, anxious to prevent an increase of family, have observed the "physiological rule" of abstinence for a fortnight after a period, and who have found, to their chagrin, after a time, that their precaution had been unsuccessful.

Dr. Oldham observes: "I know of cases which I have carefully inquired into, where impregnation occurred at the respective times of ten, twelve, and twenty-one days after the menstrual period; and while, on the one hand, I am quite ready to admit a *greater* disposition to impregnation shortly after a menstrual period, yet I know of no facts to disprove the opinion that the human female is susceptible of impregnation at any time between her monthly periods." Hirsch,¹ likewise, has seen a case where impregnation took place twenty-two days after a normal menstrual period; and he observes that, "as the Jewish women are obliged to abstain from intercourse five days before and seven days after menstruating, that race could not be so prolific as it is known to be if the ovular theory of menstruation is true." Tilt,² also, mentions the case of a lady, aged forty-seven years, in whom menstruation had been irregular for two years, and who after a single coitus, seventeen days subsequent to a period, became pregnant.

An attempt has been made to explain these and similar cases, by supposing that the spermatozoa, on the one hand, and the ovule, on the other, may retain their vitality in the generative passages for a sufficiently long time to permit the occurrence of impregnation under the circumstances named. But the facts bearing upon the subject, so far as known, do not justify such an explanation. The ovule occupies from eight to ten days in its passage from the ovary to the uterus, and it

¹ Schmidt's Jahrbuch, 1853, No. 2.

² Change of Life, p. 69.

may be impregnated at any time within that period, provided it meet with fertilizing material. If, as maintained by Williams and others, the ovule is discharged at the commencement of a menstrual period, rather than at or near its termination, we can still understand how a coitus taking place within a few days prior to the flow might be fruitful as well as one had within eight or ten days subsequent to a period. But when a single intercourse takes place from twelve to twenty-two days after a menstrual period, or ten days before the next, and becomes fruitful, we cannot accept the explanation given by the ovulationists without additional and different facts.

To meet the obvious difficulty here presented, it is urged that the spermatozoa may live a long time—indefinitely, indeed—in the generative passages of the woman. While we are not able to say positively that such is not the truth, we do say, that so far as we have actual knowledge on the subject, the tenure of life in the spermatozoa is quite limited. Dr. Sims,¹ who made many examinations of the semen in order to determine how long the spermatozoa may retain their vitality in the matrix, found none alive at a longer period than forty hours, although he admits that they may live longer under favorable circumstances. And he quotes Dr. S. G. Percy as reporting a case in which he found “living spermatozoa, and many dead ones” issuing from the os uteri eight and a half days after the last sexual connection. If we admit the correctness of all these statements we have no right to assume the persistence of vitality in the human spermatozoon for a longer period than that given. Granting this term of vitality—which I feel assured must be quite exceptional—let us see whether it is sufficient to meet the requirements of some of these cases of impregnation following a single coitus. For example, Montgomery² reports a case in which the last menstruation occurred on the 8th of October. Insemination took place on the 10th of November; pregnancy resulted. Now, if the ovule impregnated were shed at the last menstrual period, twenty-three days must have elapsed between that time and insemination. We cannot suppose the ovule to have retained its vitality and capability of impregnation during this long period, for such a supposition is

¹ Uterine Surgery, p. 374.

² Signs, etc., of Pregnancy, p. 258.

quite at variance with all observed facts both as regards it¹ and the history of the decidua (Aveling, Williams, Engelmann). On the other hand, if we suppose that the semen remained in the generative tract until an ovule belonging to the next period was extruded, we must suppose the spermatozoa to resist the mechanical washing away by means of the menstrual flow—a highly improbable notion, and one not made more reasonable by the fanciful idea that the uterus, by a sort of instinct anticipates what is going to take place and governs itself accordingly.² Likewise, it implies the vitality of the spermatozoa for a period of eight days, *plus* the time necessary to meet the descending ovule—probably four or five days more.

It is well known that many women continue to menstruate, with entire regularity, for a considerable time prior to the final cessation without conceiving; and I believe that this fact is explainable by the gradual failure of the ovaries to furnish perfectly developed ovules. Indeed, it is quite probable that all ova which are thrown off are not capable of impregnation at any period of life; for, where other conditions are apparently equal, some females are impregnated every twelve or thirteen months, others every eighteen months or two years, while others have still longer intervals of rest. Dewees mentions an instance of a lady who conceived every seven years, and who bore four children at that interval; and I knew one who had a lapse of three years between each of six successive pregnancies. It would seem, therefore, that it requires a certain period to perfect an ovule, and that the time required is much greater in some instances than in others. And, if menstruation is produced by ovulation, it appears scarcely probable that a succession of imperfectly developed ovules—so imperfect, indeed, as not to be susceptible of impregnation, or even of extrusion, as we have seen is frequently the fact—should yet be sufficient to maintain a completely normal monthly flow.

Finally, it is not at all uncommon to find the menses suppressed

¹ "How long after its maturation the ovum can retain its vitality and susceptibility to the seminal influence is not known, but probably the time is short."—DUNCAN, *Fecundity, Fertility, and Sterility*, p. 428.

² "Under such circumstances, menstruation often does not take place at all, or only very scantily; the uterine system, as it were, anticipating the conception and preventing the failure which might result from a free discharge of blood."—DUNCAN, *Fecundity, etc.*, p. 431.

for some months immediately after marriage, without the occurrence of pregnancy. Are these cases to be explained by supposing that marriage suppresses or retards the development of Graafian follicles?

From all the foregoing considerations, it seems to me conclusive that ovulation and menstruation may, and frequently do, occur independently of each other; that while they may be coincident, there certainly is no such constant connection between the two as to warrant the assertion that "at every menstrual period a matured ovule escapes from the ovary"—an assertion which embodies the very essence of the ovular theory.

2. *Physiologically, the period of the menstruation in woman corresponds with the œstrus or rut of other mammalia.*

It is well known that during certain periods, the intervals between which vary in different species of mammalia below man, ova are matured and extruded from the ovary, and that this process is attended by great excitement of the entire generative apparatus. Upon the supposed similarity of this function—termed rut, œstrus, or œstration—to menstruation in the human female is based one of the strongest arguments in favor of the ovular theory. Indeed, Cazeaux¹ and Pouchet² lay especial stress upon it.

Down to the time of Martin Barry it was believed that sexual congress was the essential determining cause of the rupture of the Graafian follicle, but the experiments of Bischoff, Coste, Pouchet, and others proved that such rupture was spontaneous and entirely independent of male influence of any kind, both in man and the lower animals, although it was hastened in some instances by coitus. Reasoning, then, from the known analogy existing between this and many other of the vital processes in the lower mammalia and the corresponding ones in man, it was assumed that the conditions of rut and menstruation were analogous, and had the same significance.

While it is true that many physiological conditions in man and the other animals of the order to which he belongs are

¹ Second Amer. ed., p. 9.

² *Théorie Positive de l'Ovulation Spontanée*, p. 227.

subject to the same general laws, these conditions differ in specific points just as much as do the different genera and species of that order in their anatomical features.

It may be well in this connection, and before enumerating the important points in which œstruation and menstruation differ, to call attention to the fact, that even in those cases in which the œstrus and ovulation are synchronous, it has never been proven that the former is caused by the latter. Indeed, it is far more probable that they are both the result of a common cause—some crethism of the system resulting in congestion and excitement of the entire sexual apparatus.

The appeal to comparative physiology by the ovulationists has always seemed to me an unfortunate one, for the noteworthy differences between œstruation and menstruation are quite sufficient, I think, to stamp the two processes as wholly dissimilar. These points of difference are as follows :

1. When, during the œstrus, there is a discharge from the genitals (which is not always the case), it is mucous in character, and its source is chiefly the glands of the external organs ; its object is to lubricate the parts, and, in some instances, by its odor to attract the male. In woman the discharge is blood, from vascular rupture ; its seat, the mucous lining of the body of the uterus, and its presence an indication of the disintegration of that structure.

2. The excitement characterizing the œstrus is the only period during which the male is received, and the only time when impregnation is possible. In woman, while pregnancy is possible at any time, it usually occurs during the period of rest, that is, in the intermenstrual period.

3. On the subsidence of the œstrus, there is a period of inappetence, during which the female not only no longer invites, but successfully resists the male approach. At the corresponding time in woman, sexual desire is commonly increased, and in some, present at no other time.

4. The œstrus, or period of sexual desire, is necessary in the lower mammalia, for the reproduction of the species. In woman, desire is not essential either for intercourse or impregnation.

5. Œstruation and ovulation in many animals are determined by changes in the seasons and other surrounding circum-

stances,¹ and in some animals (deer) the semen is only elaborated at such times. In man, changes of season, etc., produce no such effect, and semen is secreted constantly.

6. The œstrus may be excited in some animals (the mare) by the importunities or teasing of the male. Menstruation is neither excited nor hastened by the presence of the male; on the contrary, undue excitement of the generative organs, or of the sexual passion, seems frequently to have a tendency to arrest it, as witnessed in newly-married women.

7. During the œstrus, both the male and female evince a desire for copulation. During menstruation, the female has a delicate shrinking from the act, and the male likewise feels more indifference than at any other time, amounting in many cases to positive repugnance.

8. The ovaries in the lower animals contain ripe ova *only* at the period of heat (Bischoff). In the human female, ripe ova are found at all times without reference to the period of menstruation.

The foregoing points of dissimilarity are so distinctive, and refer to such important features, that I feel warranted in denying that œstruation and menstruation are corresponding processes.

3. *The removal of the ovaries is at once followed by cessation of menstruation.*

Percival Pott, Cazeaux, Wells, Battey, and others, have reported cases in which the artificial removal of the ovaries was followed by the immediate and permanent cessation of the menstrual function; and these facts have been cited to prove the necessity of these organs for the maintenance of the periodical flow.

It must be admitted that, if such an effect were the constant and certain result of double ovariectomy, it would go far towards showing the necessity for ovarian influence. But such result is not constant; indeed, the instances in which both ovaries have

¹ Barnes, *Diseases of Women*, p. 148, states that in the wild state the rabbit has only one or two litters a year, but when its young are taken away at a suitable time, it has perhaps seven. So likewise the period of ovular maturation is changed in the case of the pigeon, domestic hen, etc.

been removed without interruption or discontinuance of the menstrual flow are so numerous and authentic, that recent writers, who, like Leishman, affirm as an admitted fact, "the invariable and immediate cessation of menstruation when the ovaries have been removed," subject themselves fairly to charges either of ignorance or want of candor.

Dr. John Goodman¹ has compiled the following table showing the effect of double ovariectomy upon the menstrual function in all the cases of which he could obtain information down to the year 1872:

Table of Cases in which both Ovaries have been successfully Removed from Women under Forty-five years of Age.

| No. | Operator. | Quoted from. | D'te | Age | |
|-----|------------------|----------------------------|------|-----|---|
| 1 | Pott..... | | 178- | 23 | |
| 2 | J. L. Atlee..... | A. J. Med. Sci., 1844..... | 1843 | 29 | |
| 3 | Bird..... | Lancet, 1848..... | 1847 | 32 | Menstruation uninterrupted; tendency to menorrhagia. |
| 4 | Peaslee..... | Lyman's Table..... | 1850 | 24 | |
| 5 | Burnham..... | Lyman's Table..... | 1853 | 42 | |
| 6 | W. L. Atlee.... | Atlee on Ov. Tumors.. | 1854 | 35 | Menstruation regular. Ceased in 1864, forty-fifth year. |
| 7 | W. L. Atlee.... | Atlee on Ov. Tumors.... | 1855 | 19 | Regular menses with white discharge. |
| 8 | W. L. Atlee.... | Atlee on Ov. Tumors.... | 1861 | 40 | Menstruation regular to 1863, when last reported. |
| 9 | Peaslee..... | A. J. Med. Sci., 1863.... | 1862 | 35 | |
| 10 | Peaslee..... | A. J. Med. Sci., 1864.... | 1863 | 39 | |
| 11 | W. L. Atlee.... | Atlee on Ov. Tumors.... | 1864 | 34 | Last report 1870. Menstruation regular to that time. |
| 12 | Beattay..... | Wells, Dis. of Ovaries.... | 1865 | 37 | |
| 13 | Storer..... | A. J. Med. Sci., 1868.... | 1866 | .. | Menstruating regularly a year after operation. |
| 14 | Storer..... | Peaslee on Ov. Tumors.. | 1867 | 43 | |
| 15 | Wells..... | Wells, Dis. of Ovaries.... | 1868 | 39 | |
| 16 | Wells..... | Wells, Dis. of Ovaries.... | 1869 | 22 | |
| 17 | Hicks..... | Wells, Dis. of Ovaries.... | 1869 | 39 | |
| 18 | Monro..... | Wells, Dis. of Ovaries.... | 1870 | 34 | |
| 19 | Mayer..... | Wells, Dis. of Ovaries.... | 1871 | 29 | Last report one year after operation. Menstruation regular. |
| 20 | Meadows..... | Lancet, 1872..... | 1871 | .. | Last report six months after operation. Menstruation regular. |
| 21 | Priestly..... | Wells, Dis. of Ovaries.. | 1872 | 22 | |
| 22 | A. R. Jackson. | Peaslee, Ov. Tumors.... | 1865 | 44 | Continued to menstruate to the forty-seventh year of her age. |
| 23 | Le Fort..... | Peaslee, Ov. Tumors.... | | .. | Menstruation regular. |
| 24 | Baker Brown.. | Peaslee, Ov. Tumors.... | | .. | Menstruates, but not regularly. |
| 25 | Baker Brown.. | Peaslee, Ov. Tumors.... | | .. | * Menstruates regularly from cicatrix and vagina. |
| 26 | Koerberle.... | Peaslee, Ov. Tumors.... | | .. | Menstruation regular. |
| 27 | Battey..... | Personal information.... | 1872 | 23 | Irregular sanguineous discharges; sometimes profuse. |

Clay, of Manchester, had four cases in which there was subsequent sanguineous discharge.—(Peaslee.)

* The whole uterus, except cervix, removed with ovaries.

Dr. Goodman says: "In order to determine as accurately as

¹ Richmond and Louisville Med. Jour., Dec., 1875.

possible the effects of the removal of both ovaries upon the menstrual function, I have carefully examined and arranged all the cases of which I could obtain reports; irregular sanguineous discharges, I have, of course, not counted as menstrual.

"Of the twenty-seven cases here recorded, it will be observed that in nearly one-half menstruation was not affected by the removal of the ovaries; in one, the hemorrhagic discharge was increased; in one, it was diminished; and, in several, sanguineous flows occurred at irregular intervals."

Dr. Ely McClellan, of Louisville, Ky., in a private letter, dated March 18, 1876, gives the following facts bearing upon this point, and kindly places them at my disposal. The cases referred to were operated upon for pernicious ovulation, by "Battey's Operation," or that known as "Normal Ovariectomy."

"Case I.—But one ovary was removed.

"Case II.—Both ovaries were removed, one in May, 1875, and the other in September, 1875. This lady has regularly and persistently menstruated since the operation.

"Case III.—Both ovaries removed in August, 1875. This case had menstruated vicariously prior to the operation, and is still the subject of such disorder.

"Case IV.—The ovaries of this lady were removed in September, 1875. She menstruates regularly.

"So far as these Louisville cases go, the removal of both ovaries, after the menstrual function has been established, produces no influence upon the regularity of its occurrence. What may result after the lapse of a few more months, it is of course impossible to determine."

Indeed, it is so well known that the removal of the ovaries does not necessarily induce the menopause, that many of those who formerly denied the fact now admit it; but they endeavor to explain the circumstance consistently with the ovular theory. Some of these allege that the ovaries are not the only source of Graafian vesicles. Spencer Wells, for example, states,¹ that, occasionally, the essential elements of the ovaries are sometimes scattered between the layers of the peritoneum, as in the lower animals; and that in some cases "Graafian follicles have been

¹ Diseases of the Ovaries, p. 11.

seen developing in some of the mammalia at a distance from the entire ovary, and that such vesicles have developed into unilocular tumors." Sappey,¹ likewise, states that it is not rare to find a score or more cystic ovules, some of them the size of a pea, on the alar mesentery, in the neighborhood of the ovary, and he accounts for their presence in this unnatural situation by supposing that they "failed to reach their destination owing to some abnormal relations on the part of the Fallopian tube."

Now, while it would be presumptuous to deny that such a condition of things as that mentioned by the last named author is possible, surely it must be exceedingly rare; and, so far as I am aware, there is no instance in which such an anomaly has been found in the human female. I apprehend, therefore, that not the most ardent advocate of the ovular theory would be willing to advance such a hypothetical circumstance to account for the appearance of a periodic monthly hemorrhage in thirteen of twenty-seven cases of removal of the ovaries. For, such an argument would involve the absurd assumption that an ovum which had failed to reach the uterus, after maturation and extrusion, could return to the immature condition and ripen over again, and that, too, without its enveloping fluid and capsule!

The condition mentioned by Mr. Wells must be equally rare, and seems equally weak as a foundation for an argument. In regard to both of these conditions, Dr. Goodman, in the paper already referred to, says: "I think it a very fair conclusion that if such vesicles really existed, they were totally extirpated in some, if not the greater part of the thirteen cases, in which menstruation continued after the removal of both ovaries. Even if some of them remained, it is clearly impossible that they could have been sufficiently numerous to have afforded a ripened vesicle every month for ten or more years. Their only effect would have been to stimulate the nervous system, and maintain in a more perfect degree the ovarian development."

Others, again, have explained the persistence of menstruation after extirpation of the ovaries by force of habit. Schroeder² expresses the argument thus: "We prefer in such exceptional cases, instead of drawing the conclusion which is directly

¹ Quoted by Savage, on the "Female Pelvic Organs."

² Diseases of the Female Sexual Organs, p. 318.

opposed to all our views, viz., that menstruation has absolutely nothing to do with the presence of ovaries, to assume that in these women, too, menstruation was caused by the growth of Graafian follicles in their ovaries, but that the organism had, in the course of years, become so accustomed to the regular discharge of blood that this still continued, although the ovaries were removed."

But, surely, this is no explanation; it is nothing more than a reiteration of the fact in other terms. The "habits" of our bodies are not causeless; they are all explainable on a rational basis. No act is performed in the animal economy without some antecedent cause, and the same may be said of every recurrence of such act. In the case of the menstrual flow, if its periodicity were maintained in the past by the successive evolution of ovules, such ovular action would be necessary still; and if the cause ceased at any time to act, so likewise would the effect cease. But even this alleged force of habit fails to meet the facts in a case reported by the writer in the *Chicago Medical Journal* for October, 1870, and which appears in Dr. Goodman's table as No. 22. In this case, the patient, forty-four years of age, had both ovaries, together with a portion of the Fallopian tubes, removed. A menstrual period had ceased on the 30th August, 1865; the operation was performed the following day. On October 1st, thirty-one days afterwards, a sanguineous discharge appeared and lasted four days, attended by the usual symptoms of menstruation—lassitude, nervousness, backache, etc. There now occurred an interval of eighty-three days, the discharge reappearing December 22d. Its next appearance was on January 20, 1866—four weeks after—and from this last date it continued to return with entire regularity every twenty-eight or twenty-nine days, attended by all the ordinary menstrual accompaniments, and lasting each time from three to five days, down to October, 1867—a period of twenty-two months. It then ceased until February, 1867, when it appeared for the last time, the lady being then forty-seven years of age.

In this instance, the interval of nearly three months, during which the discharge was absent, was certainly sufficiently long to break up any mere habit, and shows that we must look to some other impelling force in order to account for the subse-

quent return to regularity. Here were no ovaries, no monthly-developing ovules, an interruption for nearly three periods of the menstrual "habit," and yet menstruation returned and continued regularly to reappear down to the normal time of final cessation.

The facts of periodicity in the human body are more numerous than generally supposed, and most interesting in their character. Without any intention of amplifying upon the subject, I will merely remark that it is now universally admitted that all the forms of periodicity, whether of a physiological or pathological character, depend upon the nervous system; and there are numerous facts which warrant us in narrowing this dependence still farther, and limiting it to a particular division of the nervous system—the sympathetic. It is well known that a frog's heart will continue its regular systole and diastole a considerable time after its removal from the thorax. The only motive agency left to it then, so far as we know, is that furnished by the sympathetic ganglia which are embedded in its substance. The influence of these centres of nervous action is neither continuous nor occasional, but rhythmical—that is, periodic. The uterus resembles the heart in also possessing numerous sympathetic ganglia embedded in its walls, and in being wholly independent of the cerebro-spinal system in its movements. Furthermore, the recent researches of Goltz and Freusberg¹ seem to show that there exists in the lumbar portion of the spine a nervous centre for the sexual functions. These facts and investigations may afford a clue to the explanation of the persistence of sexual appetite and functional activity of the generative organs after the destruction by disease, or removal of the ovaries—although in these organs undoubtedly originates the primary impelling force which sets this complex sexual machinery in motion. But whatever may be the nature or exact seat of this force, I believe that its action must be persistent and, in a sense, continuous, although some of its results be rhythmical. A pendulum may be set in motion by a single forcible impulse, and for a time it will continue to swing; but unless the application of the force be continuous or repeated, the arc described by the moving

¹ See Dr. Duncan's Address, *Obstet. Jour.*, Great Brit. and Ireland, 1875, p. 361.

body will become shorter and shorter, until finally the motion will cease wholly.

According to the light thrown upon the subject of menstruation by the latest researches, we are perhaps justified in propounding the following as embodying the main facts:

The reproductive organs of the female, including the ovaries, Fallopian tubes, uterus, and vagina, receive their vascular and nervous supplies from the same sources. Prior to the age of puberty, all these organs are in a state of comparative quiescence, and the uterus of a girl of eleven or twelve years is scarcely larger than the organ in infancy. Notwithstanding the fact that ova undergo some degree of development and are discharged from the ovaries from early childhood onward, their growth proceeds slowly, and, so to speak, unperceived by the nervous system. At or about the fifteenth year, the uterine mucous membrane attains a high degree of development, and, at the same time, the erectile tissues of the other genital organs, external and internal, arrive at their structural completion. Like a wound-up clock, with its needed touch to the pendulum, these organs now only wait for some sufficient impulse to arouse them to functional activity. This is afforded by the next recurring period of ovulation. By the advancing growth of one or more vesicles, an irritation of the ovarian nerves is produced; the effect of this upon the sympathetic, and, by reflex action, upon the vaso-motor nerves, is an increased hyperæmia in the uterus and other genital organs. The uterine congestion thus produced especially affects the lining membrane of the organ, for the reason that, structurally, it is more liable to vascular turgescence than the parenchyma. This vascular activity is followed by a corresponding increase of nutrition and hyper-growth—this latter consisting both in multiplication of the cellular elements of the part and development of those already existing. The superficial vessels of the membrane are greatly enlarged around the glandular orifices, as are also the glands themselves. The entire membrane is so thickened and convoluted that the uterine cavity seems scarcely large enough to contain it. This process—called “nidation” by Aveling—takes place in order to supply the possible needs of an impregnated ovum, and should such a one reach the uterine cavity the developed membrane becomes its future nidus.

But if not, a retrograde metamorphosis now takes place. The super-grown parts of the uterus, consisting, as already stated, chiefly of the mucous membrane, lose their excess of blood supply and die of starvation. The first elements which suffer death are the epithelial cells which line the mucous membrane; next, the new cells of the connective-tissue stratum below; and, finally, the vessels which are developing, or may have developed from this surface. All these parts become infiltrated with fat, the new formations are carried off, the vessels open, and there results the active hemorrhage which constitutes the menstrual flow. This process is repeated at regular intervals corresponding to the periodic life of the individual, and varying somewhat in different cases.

In the sense and to the extent just indicated, I regard ovulation as necessary to menstruation; it furnishes to the structurally-completed uterus, through the medium of the ganglionic nervous system, a needed hyperæmia to *originate* the menstrual discharge. In order to do this, it is not necessary that a follicle should burst (Ritchie), although it may do so. Indeed, I have no doubt that a follicle may pass through several periods without discharging its contained ovule. Doubtless, the pelvic congestion of the menstrual period greatly stimulates the maturation of the follicle, just as does the excitement of sexual intercourse, and to a much greater extent, probably, because of its longer continuance; and a follicle which has been subject to these successive periods of excitement eventually matures and bursts, with, perhaps, an occasional exception. The increase in its fluid contents, the thinning of its walls, and its near approach to the surface of the ovary, all conduce to its easy rupture; and such rupture may occur at any time, although it is clearly more likely to do so during the menstrual congestion, the excitement of intercourse, or, when on the point of bursting, from a blow on the abdomen (Schroeder). Such is probably the usual course, although, as already intimated, not an invariable one; for all authorities admit that some follicles never attain full development, but after arriving at a certain stage of growth, cease to enlarge and finally shrink and disappear.

Menstruation, with its phenomena of regularly recurring development and disintegration of the uterine mucous mem-

brane, once established, proceeds side by side with the process of ovulation. The two, while accompanying and aiding each the other, are yet mutually independent; and menstruation, instead of being an effect of ovular maturation and dehiscence, is rather, in a certain sense, their cause. In menstruation, the organ chiefly, and the only one essentially employed, is the mucous membrane of the body of the uterus; the other pelvic organs, that is to say, the uterus proper, the ovaries, Fallopian tubes and vagina, have no part in the process beyond their share in the attendant general pelvic congestion (Beigel).

There are many facts connected with menstruation which are not satisfactorily accounted for by the ovular theory, and I desire, in conclusion, to call attention briefly to a few of them.

(a) The first is that variety of the function known as *remittent*, where the habitual type is changed to another in which the flow occurs usually at shorter intervals—every fortnight, for example. These cases are strongly antagonistic to the received theory. Tilt¹ considers them as dependent “upon some perversion of the nervous force presiding over the generative function, because those in whom the anomaly is observed are generally of a delicate and nervous temperament,” and also because he has always succeeded in restoring menstruation to the monthly type by the exhibition of quinine, a remedy whose efficacy in controlling nervous derangement of a periodical character is well known. Negrier,² quoted by Tilt, after observing that several patients did not suffer from fortnightly menstruation, says: “I do not believe that the ripening of the ovarian vesicles can take place in less than a month; so, in these cases, I think it more natural to suppose that the two ovaries might so progress monthly that, for instance, the right would contain a ripe vesicle on the first of the month, while in the left ovary a vesicle would ripen on the fifteenth.” Does any one regard ovulation as a process whose type of periodicity would be changed by the administration of quinine?

(b) Again, the ovular theory does not account for the regular recurrence of menstruation after the removal of one ovary.

¹ Uterine and Ovarian Inflammation, third Lond. ed., p. 205.

² This author believed that the ovaries alternated their action, one furnishing an ovule one month, and the other the next. This is likewise the opinion of Girdwood.

Ovariectomists are unanimous in the statement that in cases where a single ovary is removed, a healthy one being left, menstruation is not interrupted, or, at least, the function is no more deranged than it would be by any other equally severe surgical operation.

It is not known how the work of maturing ovules is divided between the two ovaries. Mr. Girdwood, from observations made in several cases, states that the number of cicatrices found in the ovaries corresponded with the known number of menstrual periods, and that they were equally distributed between the two organs. Others think that for many months in succession one ovary may furnish all the ovules, and then remaining quiescent, that the other assumes the work for an equal, or possibly an unequal length of time. But it is plain that if either of these hypotheses be accepted—if ovulation be regular in any manner, and its periodicity depend upon the presence of both ovaries, it would be interrupted necessarily by the removal of either of these organs. In the first case it would occur only at intervals of two months; and, in the second, according as the active or quiescent organ, for the time being, were removed, we should have an entire temporary cessation at once, or at the end of its term of activity; and if menstruation were dependent upon ovulation, a corresponding aberration of regularity would be observed in it also. But this never takes place, for, as already stated, single ovariectomy is, as a rule, followed by no change whatever in the menstrual periodicity.

It cannot be said, in answer to this, that, as in the case of the kidneys or testicles, the removal of one gland is followed by increased and compensating work by its fellow. The ovary is not a gland, and the Graafian vesicle is not a secretion. The office of the ovary is simply and only to furnish a suitable place for the development of the primordial follicles existent in its stroma from the beginning.

(c) The remarkable regularity in the ripening and discharge of ovules, one after another, month after month, which is assumed by the ovular theory, is combatted also by the frequent occurrence of a simultaneous discharge of two or more ova.¹ Multiparous pregnancies can, I think, only be rationally

¹ Ritchie, in a case reported by Cazeaux, found six ova in the uterus at one time.

accounted for by the fact that the shedding of ovules is an irregular function, proceeding in both ovaries simultaneously and independently.

(d) The ovular theory wholly fails, too, to account for the menstrual irregularities caused by mental influence. Tilt² says: "I have patients in whom any unusual nervous emotion or over-exertion will bring on the menstrual flow, with the usual menstrual symptoms, although they may have only just recovered from this discharge. How can it be supposed that an ovule can be ripened, and the dense ovarian envelope suddenly perforated, by the fatigue of a dinner party, by hearing disagreeable news, or by an altercation with a servant?" It is well known that influences such as those just mentioned may cause the discharge to appear, and may equally check it when present; and it is likewise known that these, or other similar disturbing causes, may at once change the menstrual regularity; the flow appearing after the usual interval, whether the last one occurred at the right or wrong time. "This sudden shifting of periodic action is the special attribute of the nervous system; it shows the menstrual flow to be impelled by nervous influence, and explains how a strong emotion may repel it or alter the time of its appearance" (Tilt).

The argument which I have endeavored to make may be thus summarized:

1. Ovulation and menstruation may each occur independently of the other.

2. Ovulation is the irregular but constant function of the ovaries, while menstruation is the regular rhythmical function of the uterus (Kesteven).

3. Ova are matured and discharged from the ovaries at all periods of female life, from early childhood to old age, both before puberty and after the menopause; hence, the one cannot be the sign of the other.

4. Menstruation is the consequence of conditions established by the structurally-completed uterus, and depends upon ovulation *only* for its origination.

5. The mucous membrane of the uterine body is the only organ essentially concerned in the menstrual act; the uterus

² *Change of Life*, p. 72.

proper, the ovaries, Fallopian tubes and vagina have their functional activity increased, however, by receiving a share of the general pelvic congestion which accompanies the process.

6. The menstrual congestion of the pelvic organs—of the ovaries in particular—is, of all causes, the one most likely to determine the ovipoint when a Graafian vesicle is sufficiently mature, and hence ovulation and menstruation are frequently concurrent.

7. The theory that would make menstruation dependent upon ovulation fails to account for the possible occurrence of pregnancy at any and all times between the menstrual periods; for multiparous conceptions; for the frequent persistence of menstruation after the removal of *both* ovaries; for the non-interference with menstrual regularity by removal of *one* ovary, and for the menstrual derangements and the shifting of menstrual periodicity from mental emotion.

8. All the known facts in regard to both ovulation and menstruation are consistent with the theory that, after the latter is once established, the two functions proceed side by side, but independently of each other, the former occurring at irregular and the latter at regular intervals; while, on the contrary, many of these facts are wholly inconsistent with the theory, that assumes a necessary ovular maturity and rupture at each menstrual period.

ATMOSPHERIC DISTENTION OF THE VAGINA IN THE KNEE-CHEST POSTURE: IS IT THE REAL FACTOR, OR SIMPLY AN AUXILIARY, IN THE REDUCTION OF RETRO-DISPLACEMENTS OF THE UTERUS? WITH GENERAL REMARKS ON THE LIMITATIONS OF ITS USEFULNESS.

BY

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(With two wood-cuts.)

To one long familiar with the knee-chest posture as a very ready method for the reduction of retroversions and flexions of the uterus, the prominence recently given to "Pneumatic Pressure," in connection therewith, by American and foreign writers, appears somewhat novel and strange. We were early taught the requisites for its skilful performance; but whereas, then the posture was deemed the potent factor in the replacement, aided by suitable manipulations in the vagina or rectum, now it is subordinated to the mere "utilization of *air-pressure* as the instrumentality to effect uterine replacement."

We have so often reduced this class of displacements, with and without careful regard to the admission of air into the vagina, that we do not feel inclined to dignify it as the one essential, indispensable condition or factor in the premises.

Reduction often made in this position with total neglect of this special condition, is proof that it is not indispensable, unless it be held that it has been unconsciously used by operators who have failed to recognize its presence and merits. We believe it true that with the manual and instrumental means ordinarily employed in this posture, the introduction of more or less air does take place—indeed, could hardly be avoided, and for that reason the immediate spontaneous rectification of the uterus ought to have been so constantly observed as to make the notable instances now made public less objects of wonder and puzzling contemplation.

In February last Dr. Paul F. Mundé presented this subject

before the New York Obstetrical Society, by the report of a case in which he had effected the restoration of a retroflexed gravid uterus by securing the free entrance of air into the vagina, in the course of which he remarked that, "in a number of cases of retroflexion or version of the gravid uterus," he had heretofore succeeded "without difficulty in replacing the organ." Aside from the fact that there may have been something peculiar to this case which made it rebellious to ordinary means, his endorsement of the latter by previous experience comports rather strangely with the declaration of Prof. H. F. Campbell, who, in a very interesting article on "Position, Pneumatic Pressure and Mechanical Appliances in Uterine Displacements"¹ announces it as "*the indispensable condition of power and the real instrumentality and sine qua non in the process of replacement.*"

Moreover, that the *gemm-pectoral* position is not indispensable to the employment of pneumatic pressure is seen in the fact that Dr. Mundé succeeded in the *knee-elbow* posture. Dr. Solger, whom the latter quotes, did likewise. So that any position that secures a due gravitation of the viscera will serve the purposes of pneumatic pressure.

We have so often, in retroversions, raised the perineum with two fingers, with a free admission of air into the vagina, without witnessing this magical replacement, that we are sceptics as to its universal and uniform success, and therefore remand it for use to certain favorable cases, the elements of which we may notice hereafter. It is fair to assume that, where the condition prescribed fails in a goodly number of instances, not less favorable apparently than others more successful, it is possible that the explanation offered for the latter may not be altogether correct. The assumed factor may be an important aid, negatively an indispensable condition, or, after all, but a sequence. Causes have often been confused with effects, and in the discussion of physical problems, like those more therapeutical in their nature, it is a common occurrence: the *post hoc propter hoc* finds application here as well as in the observation of remedies.

The title of this paper, as expressed in the interrogatory, reflects the doubt we entertain on the subject. After a careful

¹ See Atlanta Med. and Surg. Journal, June No., 1875.

examination of the several explanations offered of the *modus operandi* of pneumatic pressure, we are still in doubt, the present object being, not so much to discredit the utility of vaginal inflation as to present a true demonstration of the function performed by the air thus admitted. We have used the term "Atmospheric Distention" as synonymous with, but in preference to, "Pneumatic Pressure," to avoid the idea of force that attaches to the latter, which is readily conveyed beyond the *equalizing* power of the air.

Before proceeding to demonstrate our theory, we will present for the benefit of the reader the several explanations offered by different writers. Necessarily there must be elements of the compound forces employed common to all of them, yet the direction given them by the respective parties involves distinctive differences. Of all, however, except ours, the leading idea and expression is that *pneumatic pressure, as a newly applied force, overcomes all obstacles and replaces the uterus*. Solger qualifies this by bringing to the aid of the atmospheric pressure a negative intra-abdominal pressure and the weight of the uterus itself. This does not, however, impair the statement just made, as atmospheric pressure is made the leading mechanical force.

Dr. Paul F. Mundé offers the following: "The explanation of this phenomenon is perfectly simple and obvious; the position of the patient produced a slipping of the movable abdominal viscera away from the pelvis, and a suspension of the intra-abdominal pressure, or *vis-a-tergo*, its place being supplied by a greater or less amount of suction or traction away from the pelvic organs, a certain *vis-a-fronte*, so to speak. The forcible elevation of the perineum opened the introitus vaginae and gave entrance to a volume of air, the pressure of which had already been pushing up the perineum, slightly drawn inwards by the downward gravitation of the abdominal viscera, and the *pressure* of which, when admitted, instantaneously distended the vaginal pouch and *replaced the uterus*—a mechanism identical with that on which the action of Sims' speculum is founded."¹

Solger, speaking of the sudden rectification of the uterus, expresses himself thus: "This unexpected and surprising result

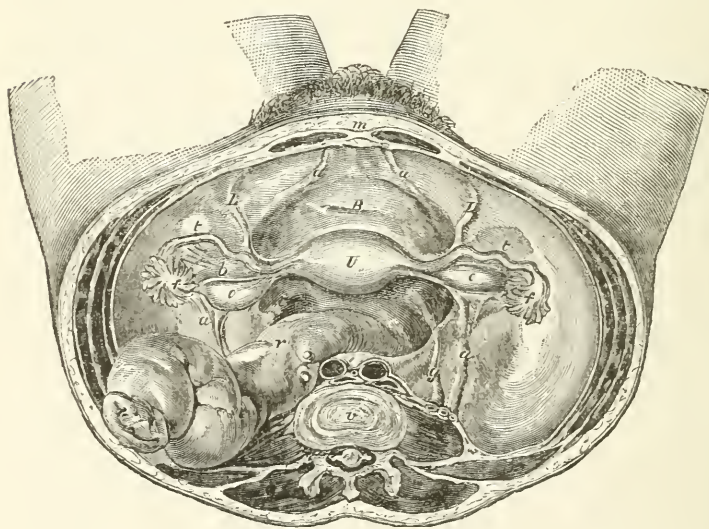
¹ American Journal of Obstetrics, June No., p. 294, Vol IX.

is due to the overcoming of the intra-abdominal pressure (equal to at least 100 pounds) by the atmospheric pressure, (which, taking the antero-posterior diameter of the superior pelvic strait only as high as 8 ctm. or 3" at 15 lbs. to the square inch, amounts to more than 100 lbs.) aided by a negative intra-abdominal pressure, not exceeding, according to Schatz, 10 ctm. hydraulic pressure, and the weight of the uterus itself."

My friend, Prof. Campbell, illustrates his theory of the replacement by a pneumatic pump, the piston of which cannot be drawn back while the air is excluded. In this figure, the gravitating viscera represent the weight (or book which he exhibits); the closed vulva is the end of the pump, with the finger over its orifice; the intra-pelvic space represents the cylinder through which the uterus, now the piston, must be drawn.

Until the "*suction is broken*" by the separation of the labia to introduce air into the vagina, the womb cannot be dragged away from the floor of the pelvis any more than the piston of the pneumatic syringe can be dragged down by the weight

FIG. 1.



attached to its handle, "until the thumb was removed from the opening above the plunger." This atmospheric pressure he characterizes "the indispensable condition of power, and the

real instrumentality and sine qua non in the process of replacement." We will now proceed to elucidate what we think forms the key to the situation, a principle admitting of such enlarged application as to be capable of restraining unwarrantable expectations, correcting errors, and promoting unqualified successes.

The septum that divides the abdominal from the pelvic cavity, composed as it is of all the structures that in gynæcological parlance, form the "roof of the pelvis," is to all intents and purposes a diaphragm. It is properly termed the pelvic diaphragm¹ since it plays a part in its physical relations to the viscera of the two sections, pelvis and abdomen, strictly analogous to its thoracic prototype. The wave of respiration imparts movement up and down to the viscera of the two cavities precisely as if they constituted one. One is apt to conclude, from the fact that they are anatomically distinct, and from the further circumstance that the larger is superimposed upon the smaller, that the latter, perhaps, actually sustains to a material extent the weight of the viscera contained in the former. This is no more true than as between the chest and abdomen, notwithstanding the greater buoyancy of the organs of the first. Normally a state of equilibrium exists between the several sections, which is never varied, except when augmentation by actual physical addition to some organ or organs brings it about; or by powerful muscular action.

Thus it happens that, giving separate existence to the pelvic diaphragm for illustrative purposes, the intra-pelvic pressure is equal to the intra-abdominal pressure. Considering the mobility of the organs of the lower abdominal region and the pelvis, this counterpoise is not necessarily disturbed by the displacement of any of them since the neighboring organs immediately fill the otherwise unoccupied space. The small intestines are peculiarly fitted for this by reason of their great mobility and expansibility.

If, then, with a transverse section of the body above the pelvis before us, exposing the pelvic diaphragm on its upper side, we picture the existence of retro-displacements, however great, with all the altered relations of the pelvic viscera due to

¹ See accompanying wood-cut, Fig. 1.

their presence, the equilibrium of pressure is still maintained: the intra-pelvic still equals the intra-abdominal pressure. It is only when, the organs on either side being displaced, the others are not permitted to occupy their places, that unoccupied space is made, and unequal pressure determined. The reduction of said displacements, outside resistance not being too great, by manual or instrumental means, is altogether practicable as long as this harmony of relations is not broken. Hence, where great enlargement of the uterus or ovaries, adhesion or impaction do not exist, the ready reduction of retroversions and flexions, or prolapse of the ovary, by manual or instrumental means, even against gravity: a system of leverage under careful manifestation, crowns with success the skill of the operator, with the patient even on her back.

In every position of the body, other than the knee-chest (or elbow) or Sims' position, technically so-called, even with a retroversion existing, this equilibrium is not disturbed. Whether prone, supine, or erect, it matters not: the great equalizing power, namely, the *atmospheric pressure*, preserves it with the certainty of physical truth: it is girt about with a law whose operation is as unvarying as the force of gravity.

The moment, however, you raise the hips (the higher the stronger) the uterus meeting with outside resistance sufficient to prevent its immediate response to or compliance with the gravitating tendency of the mobile abdominal organs near by, and in whose movements under opposite conditions it participates, the equilibrium is gone. This entails unequal transmitted pressure. Nature abhors a vacuum. The retreat of the small intestines, if possible, would leave an unoccupied space in the pelvic cavity, to fill which, air from without must be admitted, before gravity could exert its power to move the viscera or replace the displaced uterus.

Observe that it is the posture that has disturbed the equilibrium, the restoration of which demands the admission of air, not as the *factor* in the replacement, but for the one purpose of neutralizing the intra-abdominal pressure, and thereby leave the gravitation of the pelvic viscera to contend without restriction with the ordinary resistance to their return. In invoking gravity for the relief of retroversions, in the knee-chest or elbow posture, the entrance of air into the vagina, except in minor

degrees of displacement, becomes necessary in order to relieve a necessity created by the posture itself. Seeing then its function is limited to this particular duty, it cannot be considered the real factor, *i. e.*, it becomes an antecedent to the forcible employment of gravity alone in fortunate cases, and to gravity supplemented by direct manual or instrumental means in those less so. It is at most an auxiliary.

The pneumatic pump affords a very good illustration of the mechanism here given; not that the reduction is effected by atmospheric pressure as a direct mechanical force, but that it equilibrates the intra-pelvic and intra-abdominal forces, and thus allows gravity to restore the uterus, *provided always the latter is greater than the direct resistance interposed by the surrounding pelvic structures*. In any case where this precise relation of the contending forces is met, the uterus rectifies itself almost instantly upon the admission of air, thus making the latter *appear* the powerful factor. *Complete vaginal distention—that balloon-like inflation—becomes only possible, after or simultaneously with (not before) reduction, and is in no wise the cause of the replacement.*

On the other hand, in all cases (and they are legion) where the external resistance overbalances the force of gravity, supplemental forces are necessary to overcome this; then it is that the fingers and the numerous repositors known to the profession become necessary, *even though air be admitted while in use*. Our observation warrants the assertion that few cases respond so beautifully to "pneumatic pressure," the great majority under the varying sizes of the womb, degrees of displacement, conditions of neighboring pelvic organs, length of time that the retroversions have existed, amplitude of the pelvis, etc., require other aid than posture and pneumatic pressure.

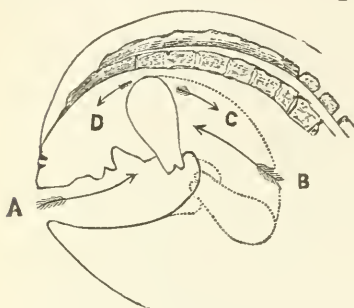
We are confirmed in the above statement by the extensive experience and observation of Prof. Jos. A. Eve, of this city, who for quite fifty years has practised and taught this method of replacing retro-displacements. In a recent conversation, he informed me that the cases were numerous in which, without further aid, both posture and pneumatic pressure fail.

If these views are correct, the glass "air-way" of Prof. Campbell is in no proper sense a "uterine repositor;" it is subject to all the limitations imposed upon vaginal inflation,

and therefore cannot be of equal dignity with the uterine repositors (Simpson's, Bond's, Sims') which it has presumed to declare "useless instruments except in extraordinary cases."

The annexed diagram (Fig. 2) illustrates the theory above propounded.

FIG. 2.



Knee-chest position.

A = Atmospheric pressure.
B = Intra-abdominal pressure = A
C = Gravity.

D = Resistance from friction, etc.

Since $A = B$, no movement is effected by either of them.

If C be equal to or less than D, the uterus cannot move on the admission of air.

If C be greater than D, the uterus moves on admission of air.

The prominence of this subject before the American profession is chiefly due to Prof. Campbell, who asserts that the value of the method has suffered depreciation from want of "adequate presentation." That object, to his credit be it said, has been attained, and a spirit of inquiry has been evoked that will soon settle its claims.

He has advanced its claims to professional adoption so far as to bring all other procedures into clinical bondage to the knee-chest posture combined with pneumatic pressure, as secured by his uterine self-repositor. We quote as follows: "*When it shall be the rule, as most surely it will, that no pessary or other appliance of internal support shall be used, without previous reduction of the dislocated uterus by pneumatic pressure in the knee and breast posture,*" then shall be realized the promise of its usefulness, for, "whatever may be the displacement, unless there is adhesion or impaction, self-replacement is completely and instantly accomplished."

We would prefer to qualify it with Solger's amendment, as follows: "Only in case of this manipulation failing to be successful, is the employment of manual or instrumental pressure justifiable," if we were inclined to assent to it. This admits the possibility of failure without the necessary existence of "adhesions or impaction," the first involving abandonment

and the second a resort to extraordinary measures. This retirement of the numerous means employed in this and other positions of the female, means so often safely, happily and quickly used by eminent men, does a violence to them not justified by their frequent and safe employment. Those means, safe in themselves, that operators have successfully used in other positions of their choice, cannot thus be brought within the reach of implied censure, and that law ought to be plain indeed, and unquestioned, that could thus proscribe them.

But without meaning to discredit the usefulness of the knee-chest position with or without pneumatic pressure, we beg to present certain objections to its uniform and universal adoption, that tend to establish limitations to its usefulness.

1st. Beyond the fact that pessaries are as safely and painlessly introduced in other positions (as on the back) after the reduction of the displacement, it may be objected that the knee-breast posture interferes seriously with the bi-manual examination of the uterus. On the back, one can recognize the retroversion, but cannot examine the uterus in its totality, until reduction is effected. Having performed this, it would be folly to seek the genu-pectoral posture for the insertion of a support. If it be said that the bi-manual examination can be made after the prescribed method of reduction and insertion of the support, we reply that the presence of a pessary prevents a thorough exploration of the body of the uterus. Hence the adoption of this law or rule may deprive the physician of much valuable knowledge.

2d. Still another objection to the introduction of pessaries in this position, is as follows: the vagina under complete distention acquires unnatural width, and thereby excludes the possibility of ascertaining the degree of contractility of the vagina still existing in the case. The true gynecologist seeks, not merely to appreciate this, but to preserve and retain it as a part of the support that he attempts to supply, and adapts his pessary with due regard to it. Emmet declares that success in the adjustment of mechanical appliances of this kind depends upon the accurate appreciation of the differences that constitute the "individual peculiarities" of cases, as well as on the "mechanical skill innate to the operator." It would be impossible to do this, if we reduced retroversions and retroflexions

always on the knee and chest, and then inserted the support. More than this, it places the upper part of the vagina and uterine vicinage entirely out of reach of the finger.

There is, however, one accurate measurement that can alone be made in this position, namely, the length of the vagina, which, of course, determines that of the support. No other position does this as well. A mental estimate, aided by the perceptive power of the fingers, is only available on the back.

Other objections might be urged, but these we deem sufficient to vitiate the proposed law, which, in its sweeping comprehensiveness, demands the surrender of all other cherished plans.

Self-replacement.—Out of this study of combined pneumatic pressure and knee-chest posture there has been evolved the idea and practice of *self-replacement*. It stands in the foreground of the picture like a great enchantress, whose fair words and promised rewards lead to its embracement. Who does not wish that truth, unmingled with error, was the substance of this gift to science and womankind? Truly, “every woman her own doctor” would no longer be denied in this class of disorders, and that which is now so full of trial and patient longing and suffering modesty, would be clothed with a retiring privacy, secure from all outside intervention. Alas! after primary reduction of the dislocation and the adjustment of a support, it is a superfluity; where reduction is necessary it is unreliable, because we have known patients with retroversions unreduced, but reducible, to declare that the uterus retreated under its use, and examination showed that the cervix retreated towards the pubis, the uterus turning upon the fundus as on a pivot: it retreated in the wrong direction under the admission of air, the retroversion remaining still.

Again, in most of the minor disorders for which it is advised, the uterine self-repositor is unnecessary, because gravitation of the viscera takes place on the assumption of the position alone. It is true that the varying sizes of the vulva often secure access of air spontaneously, so to speak; but the idea of self-replacement implies self-treatment, the conditions of which, in this respect, are hidden from patient and physician. The practice of self-replacement may prove a delusion, the fact of reposition not being within the power of the patient to know beyond a peradventure. We have known patients thus to deceive themselves.

Soon after writing the last paragraph, we were required to reduce a retroversion of several years' standing, the patient being familiar with the knee-breast posture, and accustomed to the use of the self-repositor. On raising the perineum, the uterus retreated, its fundus lodging against the promontory of the sacrum, the os looking rather forward, thus converting it into a retroversion of the first degree. It was then beyond the control of the fingers, requiring Sims' repositor to complete it. The patient averred that she always felt the entrance of air and movement of the uterus, when in this position, but, as the examination showed, was deceived as to the reduction. As we have before stated, other cases are not wanting to substantiate this as a common attendant upon self-replacement. In the discussion of the subject before the New York Obstetrical Society, Dr. Peaslee anticipated this with the remark that "the power of air to replace a displaced uterus was liable to be overrated. The air will replace the uterus only as far as the vagina reaches, and a retroversion will then be found reduced from one of the third to one of the second, perhaps from one of the second to one of the first degree; but the sound will still be felt to pass into the uterus, with its concavity turned slightly backward."

Amid the uncertainties that attend this and all other modes of replacement, when uniformly adopted, we venture to assert that the choice of positions and modes of redressing must be left to the good judgment of the operator. It is incumbent upon him, however, to select that which confers the greatest control in any particular case, and becomes the least painful and repulsive to the patient. Whether on the back, or in the semi-prone or knee-chest (or elbow) posture, it may be left to him to determine; no absolute rule can be arbitrarily prescribed applicable to all cases of retroflexions and retroversions.

From the foregoing considerations the following conclusions may be drawn, namely:

1st. That pneumatic pressure, in the knee-chest posture, is an auxiliary rather than a true factor, in the replacement of the retroverted uterus.

2d. Gravity is the potent factor, with or without the admission of air.

3d. Combined posture and pneumatic pressure are often partial only in the replacement of the uterus, and, as compared

with other useful positions, deny useful knowledge of the condition of the organ, when wholly or partially restored.

4th. The knee-chest posture, although available for the introduction of pessaries, is not without serious objections in *fitting* them, arising from the great distention of the vagina.

5th. Self-replacement involves a hazardous commitment of the treatment to the patient herself, delusive in character and results.

SOME REMARKS ON THE TREATMENT OF POST-PARTUM HEMORRHAGE.

By GEORGE T. HARRISON, M.D.,

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IF the views of obstetricians were in accord with regard to the proper treatment of this accident, a further discussion of so trite a theme might well be dispensed with. As such, however, is not the case, it cannot but help to elucidate the subject if those who labor in this field of medical inquiry bring forward from time to time their garnered fruits for submission to the unbiased judgment of their compeers. Its importance can hardly be overstated, and the practitioner of ripe knowledge will readily concede the truth of the statement that no event that can happen in the experience of a physician presents more alarming symptoms than a *post-partum* hemorrhage. Its onset is generally sudden, totally unexpected, and, unless arrested by the intervention of medical art, either rapidly fatal or else seriously jeopardizing the safety of the patient by the acute anæmia induced. Fortunately, we can view the therapeutical resources at our command for combating this formidable accident with feelings of unmingled satisfaction. In the first instance, however, let us inquire if there are no means within our power to secure in a great measure the patient against an attack of hemorrhage with all its attendant dangers. We answer unhesitatingly in the affirmative, and assert that this means consists mainly in the adoption of the method of delivering the afterbirth which is called Crêdé's method. Says Dr.

Barnes,¹ "This may be laid down as an axiom in obstetrics: *by the proper management of labor, including the delivery of the placenta, you greatly secure the patient against hemorrhage and many other dangers.*" If it be true, as the writer just quoted maintains, that the plan of delivering the placenta with which we have associated the name of Crêdé "has long been the familiar practice"² of his country, we must express our surprise that until recently no British writer on obstetrical science—with the exception possibly of individuals of the Dublin school—should have thought it worth while to mention the fact. Thus Leishman, in the first edition of his excellent system of midwifery, makes no mention of this procedure—nay, advocates an entirely different method; while in his second edition, recently issued from the press, he urges the employment of Crêdé's method, which he says has long been practised by some. Whether Crêdé is entitled to priority of discovery or not, he certainly deserves gratitude for having called attention to its paramount importance, and for having caused its general adoption on the continent of Europe, in Great Britain, and to so great an extent in this country. As is well known, the procedure consists, after the birth of the child, in inducing a contraction in the uterus by gentle friction or kneading, in case its walls are relaxed, or waiting a few minutes for a spontaneous contraction, and then embracing the fundus with one or both hands, and forcing out the placenta by the compression exercised. The pressure is made slowly, but should be firm and directed from above downward and backward, care being taken that the force of pressure be in the direction of a line uniting fundus and "os uteri," and that we have the benefit of a good contraction in addition to our extraneous force. In proportion as this method has become generally adopted, in the same proportion have adherent placentas and hour-glass contractions been less and less heard of. A striking proof of the exceeding rarity of morbidly adherent placentas where the Crêdé method is practised is shown by reference to the report of the Rotunda Lying-in Hospital³ for the year 1874, by Dr. George Johnston, master of the hospital, from which it will be seen that

¹ Vide *Obstet. Operations*, Am. Ed. p. 400.

² *L. c.* p. 401.

³ See *Obst. Journal of Gr. Brit.* for March, 1875.

of 1,236 patients delivered in the hospital, only two suffered from retention of the placenta due to morbid adhesions. Even when morbid adhesions do exist they are partial and of limited extent. A totally adherent placenta, which was formerly more spoken of than now, is an impossibility; for such pathological processes, occurring during gravidity, as would terminate in the production of a total or even very extensive adhesion between placenta and uterine surface, would necessarily interfere with the development of the ovum to such an extent as to cause its premature extrusion. A cotyledon may now and then be torn loose from its placental attachment and left behind attached to the uterine surface. Hence the important obstetrical rule always to examine the placenta to see if it has come away in its integrity.

It is well to bear in mind the fact, to which Breisky¹ calls our attention, that the separation of rather firm adhesions can be most certainly accomplished during a contraction, which is excited by energetic manipulation with the hand grasping the uterus externally. What is called hour-glass contraction is generally, if not always, a contraction of the internal os. The natural condition of a puerperal uterus is that of anteflexion, and if spontaneous contraction is unable to overcome the obstacles presented by the angle formed between cervix and corpus uteri, the placenta remains in the cavity of the uterus, and the "os internum" gradually contracts. We very much doubt the accuracy of Dr. Barnes' assertion that "if the placental site be in one angle of the uterus, occupying the area of one of Ruysch's muscles, the central part of that area will be liable to paralysis, and the circular bundles on the margin of that area being excited to action, will close in upon the placenta, forming a sac." The nearest approach to anything of the sort occurring in our experience we encountered not long since; the placental site occupied the left angle of the uterus, and as a slight adhesion existed there it was necessary to introduce the hand into the uterine cavity and peel off the placenta at that point. The outer layers of circular fibres surrounding that angle could be felt contracting, and did form for the moment a sort of pocket, constricting a portion of the placenta, but the space included by them was not large enough to contain more than a fraction

¹ See Volkmann's Sammlung, No. 14.

of the placenta, nor did the intensity of their action convey any idea of power at all comparable to that of the muscular fibres surrounding the internal os. The time for the practice of the Cr  d   method is immediately after the birth of the child; we are thus enabled to secure to the patient a state of quiet and repose more speedily than would otherwise be the case; moreover, when the muscular fibres have undergone such contraction that the uterus is only sufficient in its dimensions to contain the placenta within its cavity, the additional degree of contraction necessary to bring the uterus to its puerperal condition is remarkably well borne. A relaxation and distention of the uterus is more apt to occur, too, when we wait for the spontaneous delivery of the placenta than when we exercise a forced compression immediately after the birth of the child. But where prophylaxis is out of the question, or where its use has still not prevented the onset of hemorrhage, how shall we proceed? The therapeutical principles which should guide our conduct in the premises are not recondite. We may say with Breisky that there are three indications to be fulfilled; first, to arrest the hemorrhage; secondly, to prevent its recurrence, and thirdly, to combat the dangerous consequence of severe hemorrhage, acute anaemia, if it exists. The chief cause of *post-partum* hemorrhage, it is well known, is atony or inertia of the uterus. Fibroid tumors of the uterus and inversion of that organ may also give rise to serious hemorrhage, but these complications are rare. Hemorrhages *post-partum* may also take place from injuries done to vulva, vagina or cervix during parturition, though it is rare to have excessive loss of blood from these sources. In the preponderating majority of the cases we are called upon to treat, the problem we have to solve is, how shall we arrest the flow of blood from the patulous sinuses of the placental site? Let us consider in the first place the method adopted by nature to stay loss of blood from this point after the placenta has been extruded. "If we consider," says Dr. Breisky, "the condition of the uterus just emptied of its contents, after a normal birth, two processes may be discriminated without difficulty in the spontaneous blood-stanching mechanism; one of them being accomplished by the functional activity of the uterine muscular fibres, the other by the formation of thrombi in the torn ends of the vessels of the placental site.

The participation of the uterine muscular tissue in the arrest of hemorrhage stands here decidedly in the foreground, and takes place as well in the way of rhythmical contraction as by the more continuous action of the tone, which we will distinguish from *contraction* by the name of *tonic retraction*." The object of our therapeutical endeavors must then be to imitate nature. We must rouse the dormant energies of the uterine muscular tissue, and excite it to contraction, and induce that state of tonic retraction so necessary for the permanent safety of the patient—and failing in this, we must take measures to cause the formation of thrombi in the open vessels. The therapeutical resources at our command which may be regarded as reliable may be reduced to four, and so far we have never encountered a case where they failed us; they are: first, friction, kneading and compression of the uterus; secondly, hypodermic injection of ergot above the symphysis pubis; thirdly, injection of hot water, at a temperature of at least 100° F., into the uterine cavity; and fourthly, the injection of Churchill's or the U. S. tincture into the uterus; we have enumerated them in the order in which they should be employed. Of the first measure nothing need be further said here; all know its value, and constantly have recourse to it. Of the hypodermic use of ergot we cannot with too much emphasis insist upon its advantage over the administration of the remedy by the mouth or rectum. It unfolds its peculiar power over the muscular tissue, when thus hypodermically used, with a degree of promptness and certainty, which those who know of the action of the drug only through the other modes of administration can have no idea of. We use Squibb's fluid extract of ergot, diluting it generally with an equal quantity of water, and inject thirty minims of the solution; we have never had any trouble from abscesses in consequence of this mode of use. Those who have witnessed at Dr. Emmet's clinique, at the Woman's Hospital, in cases where he has removed a fibroid growth or polypus from the uterus, the quickness and certainty with which hot water injections into that organ, as employed by this surgeon, have brought on uterine contractions and controlled all bleeding, need not be told that it is invaluable in post-partum hemorrhage. The power of the hot water, when injected into the uterine cavity, of exciting the muscular fibres to energetic action, is

truly remarkable. The following case will serve to illustrate this.

July 31, 1875.—Called to see Mrs. F., East 36th Street. Found labor well advanced. Patient said she had only been in labor a few hours; she had had three or four children previously. The child was born about half an hour after my arrival. Placenta delivered by Crêdé's method. The uterus well contracted; binder applied; the patient left to repose; went into an adjoining room to look after the infant. On returning to inquire into the condition of the patient was shocked to see her looking exceedingly pale. Found the uterus distended with blood, reaching to the umbilicus; immediately grasped the fundus through the abdominal coverings with the right hand, and passing the left into the uterine cavity turned out all the clots, and by this conjoined manipulation soon brought a firm contraction. But as the contraction was rhythmical, and during the state of relaxation blood continued to pour out, I at once resorted to the use of hot water injections. Placing a bed-pau beneath the patient, the nozzle of a Davidson's syringe was passed into the uterus, and the water pumped in from a basin. Care was of course taken not to throw any air into the uterine sinuses. The uterus responded at once to this means, and permanent contraction was secured. There was no necessity to resort to the use of ergot, which was sent for; the patient had been in wretched health for some time prior to her confinement, and the state of the general health doubtless caused the uterine inertia. Of the hemostatic power of iodine in post-partum hemorrhage we can speak from personal experience, but would limit its application to those cases where the other means have been tried and found wanting. For cogent arguments in favor of the decided superiority of iodine over the solution of the perchloride of iron, as an injection into the uterine cavity for intractable forms of hemorrhage, the excellent paper of Dr. Trask should be consulted by those who did not have the pleasure of hearing it read before the Obstetrical Society. Finally, to combat the acute anæmia, besides the ordinary means resorted to, we would earnestly advocate compression of the abdominal aorta, in order to confine the blood as far as possible to the upper half of the body.

DISEASE OF THE BLADDER CONNECTED WITH UTERINE
DISPLACEMENTS.

BY

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ALTHOUGH in the majority of cases of uterine disease no disorder of the bladder manifests itself, yet there are some in which such disorder is developed, and, unless relieved by the most appropriate means, ultimately leads to intense suffering and serious results.

My attention was first called to this matter by Dr. T. A. Emmet, in the wards of the Woman's Hospital. There had been, at various times in our service, cases of cystitis in which we could find no distinct cause for the disease of the bladder, either in the patient's past history, or in her actual condition, and, at the first, it was by relieving coexisting uterine disorder that the relation of cause and effect became evident.

Since that time several cases have come under observation which fully illustrate this point. By these we see that great relief may often be afforded, and, in some cases, even a cure effected, once judicious means have been adopted to remedy the pre-existing evil.

By them, also, it will be seen how in some cases of cystitis the true cause and origin of the disease has been entirely overlooked, or, if the co-existing uterine trouble has been recognized, how it has been considered as of secondary importance, and not as being, as is really the case, the starting-point of the disease which has led the sufferer to seek relief.

This subject, although not entirely new, has yet not received the attention to which its importance should naturally entitle it, most of our text-books, however, having broached it in a passing manner.

So far as the subject has been considered hitherto, we find reference made to "tenesmus vesicæ" and irritability of the

bladder as a symptom pertaining to one or another uterine disease; yet, as far as I have been able to find, the study has not been followed up, nor does any author lay sufficient stress upon the value of these vesical symptoms as accompanying and indicative signs of uterine disorder.

To the uninitiated, even should his attention be specially directed to it, the actual cause may appear most trifling and utterly incapable of producing so much secondary trouble. The fact is not generally appreciated that the affection of the bladder, which is at first merely a warning symptom of some uterine disorder, may in time develop into a disease, the original factor becoming obscured by the intensity of the secondary trouble.

Several of the cases which have come under observation were well calculated to mislead, either from the fact, as I have stated above, that the more active disease obscured and masked the primary one; or again, that there was in reality only slight disorder of the uterus to be detected, and that not by any objective symptom apart from the one under consideration, but solely by the vaginal touch.

The cases to which I would especially call attention have been due to displacement of one kind or another; and in these instances it has so happened that the disorder of the bladder was the only symptom sufficiently marked to arrest the attention of the physician. All his skill, in these cases, has been directed to its relief, the exciting causes remaining overlooked. His efforts have in consequence been in great part without result, save for a slight temporary palliation of the distressing symptoms; and these are sure to recur once the treatment is abandoned.

Concerning the displacements: As I have said above, one or another may produce the trouble under consideration. Yet some are much more likely to do so than others.

Of mere elevation of the uterus in the pelvis, little need be said. It is true that such an occurrence, due to fibroids, for instance, does often give rise to vesical symptoms. These are most similar to like symptoms occurring in pregnancy, and, as a rule, are more functional than organic. Apart from this, should the above-mentioned fibroids be the cause of this elevation, attention will most surely, at some previous time, have

been directed to their existence by similar symptoms produced by an exactly opposite position of the uterus, viz. : prolapse, if not by a displacement of worse form.

Procidentia uteri, or prolapse, is certainly often the cause of bladder trouble. This has been constantly noticed and their connection often described. Yet, by many, the fact of their being associated is entirely ignored. Thus, in some cases, what was merely an irritation at the neck of the bladder, is given time to develop into an actual cystitis usually of a chronic form.

The *modus operandi* of this form of displacement seems to be, not so much that it does not allow of a complete evacuation of the contents of the bladder, as that it irritates by constantly pulling at the base, or more properly, at the neck of the bladder and dragging on the urethra, for it would seem that it is there the most felt, and that there we must look for the irritable spot at the outset of the malady. In some instances there is a marked thickening at this point, which gradually extends backwards along the base of the bladder and in some of those cases in which we have been enabled to see the actual thickness of the vesico-vaginal septum, it has been fully half an inch through.

Displacement in this degree oftentimes produces more real discomfort, as regards the bladder, than when it is more marked. The “*tiraillement*” seems to be greater in this position, as is readily understood when we consider that the uterus still remains in the axis of the upper strait, whereby the cervix is really *further* removed from the symphysis pubis than is normal.

In the second degree of procidentia, the uterus has already begun to follow the “*curve of Carus*” or the axis of the pelvic cavity, and the cervix is, in fact, *nearer* the symphysis than in the normal state, so that traction on the base and neck of the bladder is thereby diminished. It is true that if the bladder trouble has existed with the first stage, and has been of any standing, the distance from the symphysis to the cervix uteri is likely to be shortened, through the inevitable thickening of the base of the bladder ; but this condition, at the same time, really tends to hurry on the second form, and in that position, the line being as a natural result shorter, actual relief ensues.

Furthermore, we are not likely to have prolapsus go beyond

the first degree, unless we have a want of support through rupture of the perinæum, and in that case we are sure to have rectocele and cystocele occurring first. Starting at the neck of the bladder, one fold after another of the anterior vaginal wall rolls down, a proportionate thickening of this septum taking place at the same time. The same thing is noticeable on the posterior vaginal wall; the direction of the bowel, until within a short distance of the anus, being in the axis of the vagina, the fæces act powerfully in this direction, gradually producing such a bulging of the recto-vaginal septum as to necessitate the patient's forcing it back in the direction of the anus, in order to secure a movement. These two causes do not fail, in time, to bring down the uterus even to the external world. With this condition the bladder is often most tolerant, but at other times an immense amount of suffering is produced by it, and the distress is sufficiently great to urge the patient to seek early advice. When this is the case, even she cannot have failed to notice the entire want of uterine support and the consequent disturbance in the relation of parts.

In anteflexion and in anteversion the bladder suffers sometimes to a considerable extent also.

As regards the former, our text-books simply mention irritability of this organ as one of its possible symptoms; but we now and then meet with cases in which the cystitis complained of is clearly referable to this cause.

The way in which the uterus, in this position, affects the bladder, is of course the most frequently by the body pressing down on the fundus of the bladder and either preventing this organ from filling completely or inducing too frequent micturition.

With this condition also, the circulation is markedly interfered with, and we are thereby apt to have a hyperæmic condition of the bladder constantly kept up.

There is a case recorded by one of the older authors in which the symptoms produced by an anteverted uterus were similar to those usually induced by the presence of a stone, and it is to be supposed, through neglect in using proper means of investigation, the physician was actually led to practise lithotomy for its removal. The patient having died, the true state of the case was fully made out.

I fancy this case was really one of ante flexion, and that the distinction was not made simply from the fact that at that time ante flexion had not as yet engaged the attention of the profession.

In congenital ante flexion we frequently find the cervix of an unusual length, in which case, being too long to find room in the normal uterine axis, it is bent forward in the axis of the vagina. In this position it may remain perfectly harmless, so far as the bladder is concerned. But it is not unusual to find that the presence of this foreign body, as it were, becomes a source of great irritation to the base of the bladder, very much as do some ante version pessaries, leading to vesical catarrh, and finally, perhaps, to a confirmed low grade of inflammation. In treating of retroversion I shall again take occasion to refer to a similar cause.

In treating of ante version different authors mention the irritable bladder, and even hint at a further pathological change, but apparently attach but little importance to this symptom. Now, it may be true that, *as a rule*, this condition does not develop to any alarming extent; yet once it has set in at all, and especially if the exciting cause be not removed, there is nothing to hinder cystitis becoming fully established, and this will be largely due to the continual dragging backwards of the neck of the bladder by the cervix uteri.

In studying out the manner in which retroversion affects the bladder, there are several points to consider, and it becomes a question of great difficulty always to determine positively in what manner it acts.

Hitherto, in speaking of these different uterine disorders, I had reference only to the non-pregnant uterus, and it is still in this light that I intend viewing them.

Simple retroversion may act on the bladder in the same manner as does ante flexion; that is, by the cervix uteri being pressed up against its base—this pressure, and dragging at the same time, being greatest in proportion to the degree of displacement. In one case of retroversion which I report, the cervix was even atrophied, yet it had become so hard from the long-continued use of nitrate of silver that its presence was held as sufficient cause for the cystitis.

Should the retroversion be due to a ruptured perinaeum, it is

likely to start out by the first degree of prolapsus, and this want of support also entails further troubles, of which we have already spoken.

Apart from a possible subinvolution, or uterine tumor producing enlargement of the uterus, this organ, once retroverted, is apt to become more and more hypertrophied from the obstruction to the circulation due to its abnormal position, so that the vesical symptoms become more and more marked as the case progresses.

Very similar troubles may be produced by other causes than the one which now engages our attention. Such, for instance, is found in cases of extensive laceration of the cervix, in which the anterior lip is so much hypertrophied, and, for want of space, is forced forward in the axis of the vagina, pressing consequently on the base of the bladder. Here, however, we are also likely to have a want of perinæum, with also rectocele and cystocele, entailing procidentia in second or third degree, but with it, as I have said above, comparative relief from vesical symptoms.

I might also mention other causes of cystitis which are little appreciated, such as occur from accidents or mismanagement in labor, but the subject would carry us too far, and I have purposely avoided its consideration. Also those cases which have their origin in urethritis due to uterine discharges; but again, this subject, besides having been fully discussed in a recent article by Prof. Skene, of Brooklyn, is foreign to the purpose of this paper.

CASE I. *Prolapsus Causing Cystitis*.—Mrs. V., æt. 28, menstruated for the first time at fourteen years of age, regular from the first period, lasting four days. Married at seventeen. Had two children at full time, and six miscarriages. Youngest child is four years old. Last pregnancy terminated in a miscarriage twelve months ago.

For three or four years she has had more or less trouble with the bladder, but for the past four months has had some irritation about the vagina, slight fever, and a constant desire to pass water. In fact, she has suffered so much that it is impossible for her to retain any urine.

Dec. 14th, 1874.—Diagnosis: Chronic cystitis. It was

found that the trouble with the bladder was due to the prolapsed uterus dragging on the urethra.

She was sent to the Woman's Hospital, and after a few trials an instrument was fitted, which raised the organ up from the floor of the pelvis. From that time she became more comfortable, could walk better, and the bladder trouble gradually diminished. She was discharged January 4th, 1875, much improved, with every prospect of a speedy cure.

She was again seen at the hospital one afternoon in January, 1876, and found completely cured of the bladder difficulty; however, another instrument was fitted, as she again complained of a little dragging about the urethra.

CASE II. *Prolapse and Cystitis*.—Maria C., æt. 41. Native of Nova Scotia. Resident of West Farms. Admitted to hospital October 9th, 1868. First menstruated at fourteen; regular in respect to time and quantity, period lasting about five days. Menstruation always painless. At sixteen years of age gave birth to a child; was confined to bed only three days. Several years ago her bladder began to trouble her; was obliged to micturate every half hour or so, especially when moving about. About three years ago she applied to various doctors, and pessaries were used for "falling of the womb," but without any improvement. These same symptoms have continued up to the present time. Patient's general health is good.

Pathological condition: Cystocele, with partial prolapse of the uterus.

Dr. T. A. Emmet decided to perform his operation for proidentia on the anterior vaginal wall, and the operation was performed on the 20th of October, without ether.

On Nov. 3d, when the sutures were removed, it was found that the upper portion of the operation had succeeded perfectly, but at the lower extremity of the line one suture had cut out. This, however, does not materially affect the result of the operation. The uterus is at present in good position, being swung up by vaginal folds, which were brought from either side and fastened in front of the cervix.

Nov. 20th. Discharged cured.

This patient returned to the hospital October, 1873, and said she had felt perfectly well ever since her operation. The uterus

remains in good position, the folds, brought from either vaginal wall, having held the cervix well up; but from this point down to the ostium vaginae the tissues which were brought together have entirely given way. The cystocele is in fact reproduced, yet, the uterus being held up in its normal position, the bladder symptoms have not reappeared.

The patient's visit to the hospital in the fall of 1873, was on account of a recent hæmatocele, which obliged her to remain under treatment for a considerable time, and even then, although reabsorption had been going on rapidly, there remained still quite a large mass in the left side of the pelvic cavity.

CASE III. *Anteflexion and Cystitis.* — Sarah A. McK., æt. 20, single, teacher, admitted to hospital February 12th, 1869.

Menstruation first appeared at the age of fourteen, regular in time and quantity, painless, lasting about three days. For a long time back this patient has suffered with pain in the bladder. Micturition has always been very frequent, as often as twenty or thirty times during the day. She has been examined by several physicians, and her case pronounced one of chronic cystitis.

She was sent to the hospital by a physician, who thought it a likely case for Dr. Emmet to establish an artificial vesico-vaginal fistula, in order to give the bladder rest and thus promote her cure; but on examination, Dr. Emmet found the uterus completely anteflexed, and did not therefore consider such an operation justifiable until every other means of relief had been attempted.

Attention was directed to the improvement of the uterine condition, and at the same time various remedies were administered internally to overcome the suffering caused by the diseased bladder. Pareira brava was administered for some time, and the bladder washed out with warm water every other day. The *tritium repens* was afterwards substituted for the pareira brava, and it seemed to alleviate the pain very much. Injections of solution of morphia were also used, and afterwards a solution of biborate of soda in glycerine and water.

As these various remedies failed to afford the required relief, Dr. Robert Newman was requested to make an examination of

the bladder with the endoscope. This was done on June 4th, the patient being under ether.

The lining membrane of the bladder was intensely red, and exhibited little eminences, strawberry-like, analogous to the appearance of granular lids. Examination of urine showed reaction, slightly acid, color light straw, some albumen. The microscope revealed a great deal of mucus and some pus.

It is unnecessary to detail the further treatment of the case. As regards the cystitis, Dr. Newman took full charge of the patient, and his history of the case is given at length in a paper on the endoscope, read by him at a meeting of the Medical Society of the State of New York, in 1870. Suffice it to say that by continued treatment directed both to the uterus and bladder, this patient was finally cured, without its having been necessary to resort to extreme measures. She was discharged from the hospital in August, nearly cured, but remained under observation during the summer and the following fall.

January 30th, 1870, Dr. Newman writes: "This patient has remained under observation. She is well, and has kept her place as teacher in a public school, attending to her duties regularly, without missing one hour."

CASE IV. *Retroversion and Chronic Cystitis*.—Mrs. F., from Baltimore, æt. 30. Admitted to Woman's Hospital April 28th, 1874. Menses appeared at age of fourteen, regular, flow abundant, lasting eight to ten days. Dysmenorrhœa severe, more marked sometimes than at others. Married at sixteen years of age. She has had four children; the first was born twelve years ago, the last six years later; no miscarriages.

She has been sick for "many years;" frequent micturition with burning sensation, which has continued up to the present time. Any excitement causes increased frequency of micturition. Some days she does not suffer as much, and micturates less frequently.

Her general health is fair, but not what it used to be. Appetite variable.

The urine, on examination, is found to contain a small amount of albumen and some pus, but no casts.

At the first, treatment consisted in having the bladder washed out twice daily, and also in the use of hot vaginal baths; but

she suffered so intensely each time this treatment was attempted, that it was soon discontinued, and Dr. Emmet resolved to at once establish a vesico-vaginal fistula for the relief of the diseased bladder.

On May the 1st the uterus was put in position, and on May 5th Dr. Emmet made the artificial fistula. The hemorrhage was considerable, so much so that it became necessary to take a stitch on the upper angle of the wound, in order to check it.

From this time up to June 30th, on which date patient was discharged much improved, great difficulty was experienced, both in keeping the fistula open and in maintaining the uterus in position. The former indication, however, was effectually met by carrying out the appropriate suggestion, as recommended by Dr. Emmet in similar cases in his paper on "Chronic Cystitis in the Female," published in the *American Practitioner* for February, 1872.

Before leaving the hospital also, a suitable pessary was adjusted, which the patient wore comfortably, and which fully served its purpose of keeping the uterus in its normal position.

Readmitted September 21st, 1874. Patient's general health is much improved, and the bladder is in a much better condition than when she left us in June. The uterus, however, was again found somewhat displaced. This was rectified and another instrument fitted.

Throughout the Fall considerable difficulty was experienced in overcoming the tendency to retroversion. The retraction in the line of the fistula causing a continual dragging of the cervix forward, it was necessary to use a pessary which would as forcibly hold the cervix well back in the hollow of the sacrum.

January 12th, 1875. The bladder being in a perfectly healthy condition, the patient was etherized, and the artificial fistula closed. The stitches were removed on the 21st, and union found to be complete.

February 3d. The instrument was readjusted, and although at the time she complained of some discomfort about the bladder and rectum, these sensations gradually subsided, so that she was able to return home on February 23d, completely cured.

In the course of the year we had occasion to hear of this

patient, and she was said to be perfectly well and steadily gaining in flesh and spirits.

CASE V. *Retroversion and Cystitis*.—Mrs. John L. C—, from Indianapolis, æt. 32. Menstruated first at fourteen. Period lasting five days, regular from the beginning. Married at twenty-five. Sterile.

A year after marriage she began to suffer from cystitis. In October, 1871, after a correspondence with Dr. Emmet, Dr. Parvin, of Indianapolis, punctured the bladder for the relief of the cystitis, but patient says that she was not in any way benefited by the operation. Sixteen months after the time of the operation, Dr. Parvin attempted to close the fistula, but failed. Fourteen months later a second attempt was made, but with no better success. Again in September, 1874, two more unsuccessful attempts at closure.

February 1st, 1875. Diagnosis: Uterus retroverted to the left and fixed; also vesico-vaginal fistula.

The organ was finally replaced, having been held by the utero-sacral ligaments.

The patient was then sent to the Woman's Hospital, March 21st, 1875, and the fistula closed (March 23d).

April 1st. Sutures removed. Patient has kept dry.

April 13th. Operation entirely successful.

April 18th. Uterus again replaced, and instrument introduced.

June —. Patient finally discharged, wearing an instrument which effectually holds the uterus in position, and she is entirely relieved.

It is evident that in this case, although the patient did not at the first experience relief, yet that, from the bladder being entirely at rest, cure of the cystitis did actually ensue, since we see by the history that Dr. Parvin was led to attempt the closure of the fistula sixteen months after the original operation. Had he succeeded, there would most certainly have been a return of the bladder difficulty, as the uterus was backwards and fixed, which was the cause of the whole trouble at the outset. Once this organ was replaced and held in position, the closure of the fistula was naturally called for to make a complete cure of the case.

A CONTRIBUTION TO THE THERAPEUTICS OF OVARIAN CYSTS.

BY

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IN view of a procedure to be described at the close of these remarks, this question has at the present time assumed more than ordinary interest, and I have thus been led to deem it appropriate to subject the different therapeutic methods to a brief discussion.

As a matter of course, the *diagnosis* must, first of all, have been determined with certainty, and in this respect too much caution cannot be exercised. I recall a case where a well-known gynæcologist made the diagnosis, with *absolute* certainty, of a simple ovarian cyst with fluid contents. Intense marasmus forbade any heroic interference, and led to a speedy death. The autopsy revealed *ascites*.

The diagnosis having been rendered certain, the next question that presents itself is that of the treatment. I will refrain from considering the internal remedies formerly recommended, among which the alkalies and the preparations of iodine and of mercury appear prominently, as we can scarcely, even to a degree, expect from a remedy that acts by being taken up by the general system, the local effect which here is all-important. Neither will I consider *spontaneous recoveries*, for it would be a sorry matter, indeed, for women, to leave them to the hope of such. We have left, thus, no other remedy but an *operation*. It must be borne in mind, however, that under this name are comprised procedures that differ very materially indeed with respect to the danger to life to which they expose the patient.

Among the most heroic of the operations we must class "the extirpation of the degenerated ovary"—*ovariotomy*. "*Overdo nothing*" was, in addition to self-confession, the quintessence of Greek wisdom. This golden rule applies also to *ovariotomy*. I do not believe that we can dispense entirely with *ovariotomy*;

I believe, on the contrary, that we shall always have desperate cases in which all other methods will fail us, and thus necessitate this. It should, however, be the rule not to have recourse to so heroic an operation until so relatively safe a method as that which I shall describe farther on shall have been tried. It may perhaps be said that other less radical procedures will only serve to waste precious time. This would be an unfounded objection. No time should be wasted in arriving at a diagnosis. It will then be time enough to institute a milder procedure, and, if this prove insufficient, to have recourse to one more radical. Nor can any guarantee be given, after ovariectomy, against further ovarian trouble; where there is the tendency, the *other* ovary may become the seat of degeneration, and a second ovariectomy in the same person will hardly be attempted.¹

True it is that a Greek philosopher has called physicians happy, "in that the sun shines on their triumphs, while the sod covers their mistakes;" but let us not carry grist to the mill of this scoffer.

For ovariectomy remains, and will ever remain, a highly dangerous proceeding. The sudden removal of a foreign body weighing many kilogrammes from the abdominal cavity, the opening of this, the injury to the peritoneum here and in the loosening of adhesions, cannot be borne without great danger to life. It is true that the percentage of favorable results is at present much larger than formerly. This is due, on the one hand, to the circumstance that now the operation is performed when the tumor is still comparatively small; on the other, to the high degree of perfection at which ovariectomists have arrived. Yet, let the cyst be ever so small, the danger, though lessened, will not be abolished. No cyst is smaller than a normal ovary, and the removal even of this would be a dangerous proceeding. I will, therefore, cease to dwell on extirpation of ovarian cysts, the more in that the details of the operation are abundantly familiar. On the occasion of a visit to Germany I saw tumors removed from the abdominal cavity with extraordinary dexterity—tumors that weighed twenty or more kilogrammes, and that necessitated for their extirpation incisions

¹ Extirpation of both ovaries has, however, been practised. I will point, for example, to the case of *Peaslee*.

from the umbilicus, or even from the xyphoid process to the pubis. The rapidity and skill of the operator were admirable, to be equalled by few, and probably excelled by none. That patients died, nevertheless, was certainly not the fault of the surgeon. I have been told of a case that occurred in Vienna, where death took place a few hours after ovariectomy. I should deem it superfluous to adduce here additional statistics, as the danger of opening the abdominal cavity has, from the first, appeared to me so obvious that its restriction to cases where it is unavoidable should follow as a matter of course.

Another method to obtain a cure in ovarian cyst is *tapping*, followed or not by injections of more or less concentrated *solutions of iodine*, or the insertion of a *catheter* (mechanical irritation). Simple puncture (which *Martin* recommends as an explorative puncture for diagnostic purposes, and, as is shown by the case mentioned in the beginning, with some reason) is, as a rule, resorted to not with the expectation of a cure, but only as a palliative remedy, often only for momentary relief. Still, there exist cases where repeated tapings eventuated in a cure (*Jobert de Lamballe, Lalesque, Preuss*, and others); the circumspect physician will, however, not content himself with this procedure, as reliability, even to a degree, is out of the question, and moreover, because, as we shall see further on, there is a safe and far more efficient method for the removal of ovarian tumors.¹

Following tapping of the cyst with injections of iodine or mechanical irritation (introducing an elastic catheter) is not devoid of danger. The object is to produce inflammation of the cyst-walls, and in this way cause shrinking and disintegration of the new formation. Instances, however, are not rare, where an inflammation of this kind extended to the peritoneum and gave rise to fatal peritonitis. This will be the more likely to occur, if, on withdrawing the cannula, some of the liquid should find its way into the free abdominal cavity. The custom of some English physicians of employing, in the place of iodine, injections of perchloride of iron or chloride of zinc, will not tend to diminish the danger.

As to the influence of *mechanical irritations*, this likewise is

¹ The same holds good of external applications (inunction) of *iodo-glycerine*, the *Brower-Stark ointment*, the *black oxide of copper*, etc.

by no means a matter of indifference. RICHARD and THOMAS witnessed the death of patients in whom the trocar was allowed to remain after the operation. If now we reflect that even in favorable cases (those without a fatal result) we have not even a comparative certainty of radical success, this method can hardly lay claim to unconditional recognition.

I come now to the procedure which I have repeatedly employed and described, *viz.* : *Electrolysis of the cystic degeneration of the ovary*. I purpose to effect by means of electrolysis a destruction of the ovarian tumor, through a chemical process, and at the same time so prejudicially to influence the neoplasm as to cause its complete disappearance, or its reduction to a minimum—objects that I have frequently attained. As, however, the procedure may not be familiar to the majority of practitioners, I deem it necessary first to explain its *rationale*, and for this purpose will quote from my treatise “on electrolysis of laryngeal tumors”¹ those passages which bear on the subject.

It is well known that in the electric (galvanic) current that is obtained from two dissimilar (generally metallic) bodies which communicate through one or two conducting liquids, as well as in frictional electricity, we distinguish a positive and negative current, and that it is the positive electricity that serves exclusively for the designation of the direction of the current. In the zinc-carbon element (the form which I prefer for the treatment of ovarian cysts), therefore, the direction of the current is from the positive electromotor (zinc) through the conducting liquid (a solution of bichromate of potash with the addition of sulphuric acid) to the negative (carbon). At the poles of the battery, however, free electricity gathers, which is positive at the carbon, negative at the zinc pole. The positive pole, through which the current enters into the conducting liquid, is known as the *anode*; the negative pole where it leaves, the *cathode*.

In its course the electric current has to overcome a certain resistance. Among the *good* conductors (those bodies that offer but little resistance to the current) may be counted the metals; among those less good, liquids generally. A warm

¹ On a New and Safe Process for the Removal of Laryngeal Tumors. Vienna, 1872. Carl Czermak.

saline liquid may be classed as a medium conductor, and to the fact of its saturation by a warm saline fluid is due the electric conductivity of the human body—a circumstance that is of prime importance in our discussion.

The conducting of a galvanic current through a fluid or a tissue saturated with such, cannot take place without giving rise to a change in their composition, *which consists in a chemical decomposition*. We term this decomposition “electrolysis.”

The *form of the instruments* by means of which the electric current is introduced in the body (and which is known as electrode or rheophor) is, in electrolysis, usually that of a *needle*. (In the electrolysis of ovarian cysts I generally connect the one pole with a needle, while the other, which is to be applied to the skin, is in form of the ordinary electrode.)

If two needles, connected with the poles of a galvanic battery, are dipped into water, decomposition at once commences, oxygen gathering at the positive, hydrogen at the negative pole. A mass of small gas bubbles covers the surface of the needles, their number being in direct proportion to the strength of the current.

In my lectures I generally make use of a piece of muscle to illustrate galvanolysis. Shortly after the needles have been introduced, a frothy liquid makes its appearance at the spot which communicates with the negative pole—a sign that decomposition has begun. Usually this can also be determined by the ear, a crepitant sound becoming audible. At the same time the tissue becomes discolored, yellowish gray. If the channel made by the needle be now opened, it will be seen that this discoloration extends along its entire length, wherever the needle was in contact with the muscle. The layers of muscle treated in this manner bear a resemblance, under the microscope, to cooked meat, and it is not unlikely that the abstraction of water is the prime factor in electrolysis. The needle remains easily movable, and can at any time be readily removed from its channel. It is different with the positive needle. Here a grayish black coloration, continued along the entire length of the needle-channel, makes its appearance; the needle, also, is more adherent to the tissue.

These are the phenomena of electrolysis on *dead* tissue.

The effect on *living* tissue resembles in many respects that

which I have described, at least with respect to electro-chemical occurrences ; and it is to this circumstance that is due the reliability of the procedure, which appears comparatively largely independent of all vital functions. The vital processes, however, play an important part, nevertheless, more especially in respect to the destruction of the tumor. While, for example, in the cadaver the electrolytic process is limited to the parts that are immediately exposed to the action of the current, the influence of this *in vivo* goes much farther, inasmuch as there obtains here not merely a local destruction of the parts touched by the needle, but in addition to this the nutrition of the pseudoplasm is efficiently disturbed, superinducing its mortification.

After what has been said, it will be scarcely necessary to state that *electrolysis should not be confounded with galvanocausty*. Both processes differ essentially from each other : while in galvanocausty a piece of platina is made white hot and serves for the removal of morbid tissues, in electrolysis we have to do with a chemical process, in which increase of temperature is no factor.

If then I should sum up tersely, and from a practical standpoint, the process of the *electrolytic destruction of cystovaria* and of neoplasms generally (for it follows as a matter of course that the greatest variety of neoplasms can be destroyed with equal facility as ovarian cysts—whereof I propose to speak in a separate article), I should feel compelled to say : *In electrolysis a destructive process is initiated through the chemical effects of the current introduced into the ovarian cyst by means of a needle, which leads sooner or later to a shrivelling or entire destruction of the neoplasm.* (The same holds good of neoplasmata of every nature, benign as well as malignant.) The operative procedure, then, repeated more or less frequently, according to circumstances, is reduced to a *simple acupuncture* ; certainly a minimum as compared to an operation like ovariectomy, that requires the opening with the knife of the abdominal cavity. The treatment might even be carried out, in the majority of cases, at the physician's office, although as a rule I prefer the safer course in choosing for this purpose the patient's residence. Where the cyst contains a large quantity of fluid, this should be voided before proceeding to electrolysis,

in order on the one hand to ease the patient as rapidly as possible, on the other, not to waste the current in decomposing a liquid that can readily be removed otherwise, but rather to confine its force directly to the cyst walls. I am in the habit of combining with electrolysis percutaneous electric applications, although I cannot advise any reliance on these alone (some few favorable experiences notwithstanding), because in most cases valuable time would thus be lost, and in ovarian cyst it is not good to lose much time: for although these tumors sometimes remain *in statu quo* for a long period without appearing to enlarge, they often begin suddenly to enlarge with great rapidity.

I was the *first in Vienna*, nine years ago, to employ electrolysis in a neoplasm (a fibroid situated near the left trochanter major), and caused the case to be presented to the "Royal Imperial Society of Physicians of Vienna." In the year 1868 I thus treated the first case of ovarian cyst (she was a patient in my ward of the Imperial Vienna Hospital) with the best result¹—a multilocular ovarian cyst of the size of a man's head contracted down to the size of a hen's egg, and the patient subsequently married. In another case a still larger cyst of the ovary, in a woman about thirty years of age, disappeared so completely that at present no tumor can be felt, leaving only a funicular remnant and some dulness on percussion. Three punctures were made after the last electrolytic treatment (the last of them about a year ago), and only the traces mentioned remain of the neoplasm. I wish to state here, that in electrolysis of ovarian cysts the effects are not always patent at once, but make themselves apparent rather in the form of after-effects, *i. e.*, the degenerative process set on foot by the electrolysis progresses more slowly to the point when a visible shrinking of the neoplasm takes place. It is therefore appropriate to allow an interval between the different treatments, although correct judgment is necessary to determine the length of this, in order by too much procrastination not to give the destructive process time for self-limitation—the neoplasm for recovery from the electrolytic influence.

On this account, and also because the other conditions for a

¹ The first mention of "*Electropuncture*" in ovarian tumors I find in a pamphlet (in the year 1848, Hirschwald; Berlin) by Dr. I. I. Bähring.

suitable carrying out of the treatment, such as the nature of the elements to be employed, their number, the duration and frequency of the electrolytic séances (eventually also the percutaneous application of the galvanic or faradic current), and other important items too numerous to mention here, require considerable special experience, I do not deem it advisable that electrolysis of cystic ovarian disease should be practised without such experience, and do not think myself guilty of a statement *pro domo* if I counsel its relegation to the specialist. I need not dwell upon the great importance of this method to so many women, who otherwise have only the choice between certain death and an operation that is dangerous to life; the matter speaks sufficiently for itself.

A NEW METHOD OF TREATING HEMORRHAGE AFTER ABORTIONS AND AT FULL TERM, WHEN DUE TO UTERINE INERTIA.

BY

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AN article in the June number of THE AMERICAN JOURNAL OF OBSTETRICS, by Dr. H. T. Hanks, on excessive or unusual uterine hemorrhage, induces me to publish a method to which I have resorted during the last four years for the arrest of this troublesome and sometimes alarming accident, when due to uterine inertia.

The procedure is simple, safe, and effective, and up to now has never failed in a single instance in which I have used it. It consists in passing into the cavity of the uterus a rubber bag, which is afterwards distended by means of air or water, preferably water, until it fills the entire uterine cavity. The bag will press in the direction of the least resistance, and adapt itself to all the little inequalities of the placental site. We thus, without using any great amount of force, bring sufficient elastic pressure to bear upon the mouths of the bleeding vessels

to effectually seal them, and render further hemorrhage an impossibility.

The hemorrhage being arrested, we can leisurely direct our attention to inducing uterine contractions, by kneading the uterus, and the administration of ergot, either by the mouth, or hypodermically. When uterine action sets in (and it occasionally will immediately after the flow has ceased, especially if ice-water be used to distend the bag), we can allow the air or water to escape from the bag *pari-passu* with the contractions, or better, allow it to remain. The bag being elastic, is easily moulded to the parts through which it has to pass, and will be expelled just as the bag of waters would, and at the same time continue to act as a valve to the bleeding vessels until the womb is so thoroughly contracted that further hemorrhage will cease.

These rubber bags have a tube attachment similar to Barnes' dilators, and for cases that occur before the fifth month of gestation has passed, a large Barnes' dilator can be so distended as to fill the entire uterine cavity, even should the walls be so relaxed that the organ can be distended to as great an extent as it had been by the fœtus and secundines. I have never seen a uterus so thoroughly paralyzed that it would admit of such great distention, and I doubt very much whether one could be distended by a rubber bag to such an extent, from the fact that the bag has a tendency to give way at the point of least resistance, and would be likely to bulge from the os unless the os was contracted very firmly—an occurrence, I believe, that never takes place in uterine inertia. This tendency of the bag to bulge can be exemplified by taking one in the closed hand and distending it; it will be seen to pop or bulge out between the thumb and forefinger.

The following three cases will illustrate the efficacy of this method of controlling post-partum hemorrhage.

CASE I.—July, 1872, I was sent for to see Mrs. H—, a large, robust woman, residing three miles from town, who had just miscarried of a three months' embryo. The placenta had not been expelled up to the time I was sent for. She had lost a great deal of blood when I saw her, was very weak, and almost completely exsanguinated. I had her brought to the

edge of the bed, introduced the speculum, and commenced to pick away the adherent placenta with a pair of uterine dressing forceps, and was able to get away only a small portion at a time. The hemorrhage becoming somewhat more profuse, I passed lump after lump of ice into the uterus, hoping thereby to induce contractions, but failed to do so. At this time she began to complain of being tired, and requested me to allow her to rest. It was then that the idea first suggested itself to me of passing a Barnes' dilator into the uterine cavity, and blowing it up in order to temporarily arrest the flow, and allow her to assume a more comfortable position. This I did, and to my delight the hemorrhage was arrested completely, although a portion of the attached placenta still remained in the uterus. She was allowed to rest an hour, at the expiration of which time I let the air escape from the bag, and removed it. The hemorrhage returned, but not to so great an extent as previous to the introduction of the dilator. I then finished picking away the placenta, and before leaving, injected half an ounce tinct. iodine into the uterine cavity to insure perfect contraction. Her convalescence was normal, and involution perfect.

I have no doubt that if, in the foregoing case, I had given ergot, uterine action would have set in, and expelled the bag. I would have done so if all the placenta had been removed before it was introduced.

CASE II.—Hannah E——, negro, married, five months advanced in pregnancy, miscarried Sept. 4th, 1873. On arrival I found the fœtus delivered, and the placenta partially attached. Hemorrhage very profuse. Removal of the placenta was not followed by uterine contractions, and the flooding continuing, we introduced into the uterine cavity a large size Barnes' dilator, and distended it with water, which controlled the flow. I then gave her a hypodermic injection of fluid extract of ergot, which induced prompt uterine contractions and expelled the bag, after which time there was no further flow. The woman convalesced without a single bad symptom. Involution was normal.

CASE III.—Mrs. W——, a young, healthy woman, was delivered of her first child April, 1873, after a tedious but other-

wise normal labor. As soon as the child was delivered, she began to flow fearfully. I was immediately sent for; upon arrival, found her sinking very fast and blood gushing from the vagina; introduced my hand and found the placenta lying mostly in the vagina and attached by a small portion to the inside of the uterus. Removal of the placenta did not check the flow or induce contractions. We immediately passed a rubber bag into the uterus and began filling it with water, and had not thrown in more than half a gallon before the hemorrhage ceased. We then gave her ergot and kneaded the uterus, which induced contractions. Within an hour the bag was expelled and no further hemorrhage ensued. Her convalescence was normal and involution perfect. She has since borne another child.

On no occasion have I ever found it necessary to distend the uterus to as great an extent as it was before delivery took place, and I have never had to wait more than two hours for the uterus to expel the bag and become firmly contracted. Should ever occasion require me to carry the distention to so great an extent, or to wait even a day for the uterus to regain tone and power sufficient to expel the bag, I should have no fears in doing so, being fully assured that it could be re-aroused to contractions, and that involution would go on normally.

This method has several advantages over those that are usually employed for the arrest of post-partum hemorrhage. Even leaving the elastic pressure out of the question, it is better than passing ice into the uterine cavity, should we be disposed to rely upon cold as a means of astringing the blood-vessels and inducing contractions of the uterus. We can inject the bag with ice-cold water, and save the woman the annoyance of a cold bath to the buttocks, which will surely take place as the ice in the uterus melts and the water runs from the vagina. Its advantage over pressing the placental site with the hand is considerable. First, it is perfectly reliable; the distended bag will cover the entire site, which the hand will not always do; and, besides, there is not the least danger of bruising the uterus, as there would be if one were placed against the placental site on the inside of the uterus, and the other is pressed against it from the outside of the abdomen.

Over the persulphate of iron injections, its greatest advantage is its perfect harmlessness. We run no risk of inducing metritis or puerperal septicæmia; and, besides, we avoid the disagreeable stickiness of the hands that one is sure to have if the hands are brought in contact with the iron injection. But the chief and greatest advantage it has over all other methods is that it can be more speedily resorted to and does its work more quickly, one or two minutes generally being long enough to fully distend the bag and check the flow. Second, that it is perfectly harmless, and the most awkward need not hesitate to resort to it. Even should the bag burst, which is the only accident that can possibly happen, we simply wash out the uterus, which can do no harm—that is, if the bag had been filled with water, which I think is best and safest, though I have frequently used air and had no accident to happen.

In offering this method of arresting post-partum hemorrhage due to uterine inertia, to the profession, I am willing to allow it to stand on its own merits, feeling fully assured, from the uniform success I have had with it, that, if properly carried out, it will seldom or never fail to accomplish the desired end, and will often relieve the obstetrician of much anxiety.

WHEN AND HOW OFTEN SHOULD WE USE THE FORCEPS?

BY

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WITHIN the past few years there has occurred a very considerable change of opinion as regards the use of forceps, tending, notably within the last decade, to overstep the landmarks laid down by the older authors as characterizing the conditions demanding a resort to instrumental delivery.

The teachings of even quite modern authors, and especially the English, had tended to discourage the use of this instrument, except in a very limited number of the conditions met with in labor; leading us to believe that the instrument,

although powerful and decidedly valuable, is at the same time very dangerous in its use, and should never be taken in hand except by those who have acquired proficiency in obstetric operations.

Dr. Robert Lee crystallized the prevailing opinion of the British practitioners of a few years ago, into the following words. Said he: "There are few practitioners of judgment and experience, who have recourse to the forceps, or who employ it before the orifice of the uterus is fully dilated, and the head of the child has descended so low into the pelvis as to make an ear to be felt."

As a legitimate consequence of this practice, when the powers of the uterus failed to cause the head to descend so low as to enable us to feel an ear, a condition which quite frequently occurs, *perforation* must needs be resorted to, and a life sacrificed. If we appeal to the records of English lying-in hospitals, the correctness of this deduction will be amply verified.

In a comparative view of the frequency of forceps and craniotomy cases, as shown by the records of eleven lying-in hospitals of Great Britain, the forceps were applied only once in every 617 cases, while craniotomy was resorted to once in every 141 cases; thus, craniotomy was resorted to about $4\frac{2}{3}$ times oftener than the less destructive mode of delivery by the forceps. To the average mind, the enormous craniotomy practice, and consequent destruction of life, shown by the English records of but a few years ago, is chargeable in a large degree to the opinions there held, forbidding the timely use of the forceps, except in a very limited number of cases.

In Germany, they reach the opposite extreme, using the forceps once in every *seven* cases, while craniotomy was performed only once in 2,093 cases. In France the forceps are employed once in every 293 cases, craniotomy once in every 1,854 cases. [Churchill, Amer. Ed.] In this country we do not possess sufficient obstetrical records from which to construct a positive comparison and average between these two methods of artificial delivery, yet it is believed that we occupy a happy medium ground between that of the French and Germans.

This great difference in the application of the forceps, between the English on the one hand, and the Continental European and American practitioners on the other, is a significant

fact which we should not overlook, inasmuch as it seems to argue very conclusively that, either from defect in the mechanism of the instrument, or from a flagrant want of proper rules as to their employment, our British brethren have, so often in the past, had recourse to a most fearful operation on occasions when, in this country, and in Continental Europe, a timely use of the forceps would obviate its necessity, and save human life.

In a practice extending over the space of twenty-seven years, and with a very considerable experience in obstetrics, I have not had occasion to resort to craniotomy in a single instance where the case was my own *de novo*. Twice during this period I have been called upon to assist others where this operation was required; and in both instances, I feel justified in charging the necessity of mutilating the child, and its consequent death, to the teachings of Burns, Denman, Davis, Ramsbotham and Churchill, and those who taught after their systems. In both cases the attending physicians had delayed a resort to the forceps, in obedience to the rules they had learned, until their successful application became impracticable; and I am of the opinion that in both cases the delivery could have been brought to a successful termination, by an early resort to the forceps, and the lives of one or both children saved.

I am aware that the medical profession, everywhere, has taken an advanced step in this department of obstetric practice; and I submit herewith some of the most recent views published.

Dr. More, of England, maintains (*Lancet*, Oct. 25, 1873), that obedience to the rule of the older teachers, "that the use of the forceps should not be had recourse to so long as the fœtus makes *any* advance," is fraught with evil, both to the mother and child. He further says, "The timely use of the forceps, shortening the second stage of labor, is the *great practical* improvement of recent midwifery." His statistics show that the assisted cases get up and are about sooner, and feel better than those left entirely to nature.

He concludes his article, by enumerating the classes of cases wherein he deems it justifiable, and even obligatory, to resort to the use of the forceps. Inasmuch as Dr. More enlarges the scope of cases demanding or justifying interference with the forceps, I give them in full, and in his own order:

First. All cases where the first stage is completed, and the head remains stationary. In these cases he would not wait more than two hours. *Second.* In cases where, though the head is advancing, the labor is rendered tedious, from the fact of the pains being too weak, or having almost ceased. *Third.* When the pains are stronger than is warranted by the advance made. *Fourth.* In cases of hemorrhage, especially if severe. *Fifth.* In some cases of convulsions. (We would inquire, why not in all cases, as soon as it is practicable to adjust them?) *Sixth.* In all cases favorable for operating, when the patient is very desponding or impatient. *Seventh.* In cases of occipito-posterior presentation and not advancing quickly. *Eighth.* In cases of the second twin, if a head presentation, and not advancing quickly. *Ninth.* To save time, if the case is favorable, to relieve the woman and himself from work.

In the *Brit. Med. Journal* for Aug. 1873, Dr. Hugh Miller, of Glasgow, says he applies the forceps in tedious labor from debility. This, he claims, is preferable to ergot. It prevents exhaustion of the uterus, and hence diminishes the chances of flooding.

Dr. McDonald is of the opinion that it is exceedingly doubtful if, in cases of contracted pelvis, it is not safer to let the case go to term—then see what nature can do; next, the forceps, if there is room—then craniotomy or the Caesarean section, as a last resort. Turning in these cases does not present any advantage to the mother over the long forceps in contracted flat pelvis, and is more dangerous to the child.

It is wholly inadmissible where the pelvic contraction is general, and more dangerous than the long forceps, or any of the higher operations.

Dr. Murray agreed mainly. He pointed out the unsatisfactory nature of the generality of recorded cases. There were so many sources of error in pelvic measurements that they were not to be relied upon to any great extent. Cases of any considerable contraction are extremely rare, post-partum evidences having repeatedly revealed errors of diagnosis in this regard. Especially, a prognosis from the history of a first labor was very misleading generally. Even in contractions down to near three inches, he was much encouraged to look for favorable results by reports from recent German authors. He claims no desire

to assume very strong grounds, but regards the forceps as safer in general, both to the mother and child.

These are some of the views recently put forth tending to the advancement of obstetrical knowledge. I am not prepared to adopt all the theories advanced by the authors quoted; yet I feel constrained to admit, that the too infrequent use of this valuable instrument is fairly chargeable to our American practitioners—especially outside of the large cities.

My own experience has been too limited, and my statistics too loosely kept, to urge them as a criterion for others. All my statistics of cases prior to 1861 were, some years since, destroyed by fire; but from 1861 to the present I have a tolerably complete record. In looking over, I find my recorded cases of forceps delivery have gradually increased their ratio, year to year, as compared to the whole number of cases recorded.

From January 1st, 1861, to December 30th, 1865—five years, I applied the forceps once in every 147 cases. For the next five years, once in every 51 cases; and in the last five years, ending December 30th, 1875, I find I have had occasion to use them once in every 23 cases; and I am very favorably impressed with the results of the more frequent use in later years. I am notably well pleased with the very meagre number of still-born children. I am required to note upon my records, as compared with some of my professional neighbors, who seldom or never employ instruments—some of whom, even, admit that they do not own a pair of forceps. In cases with a large head in proportion to the pelvic capacity, or with a head having the fontanelles preternaturally ossified, and the second stage having been reached, if the pains were feeble and not easily provoked, or if strong and no advancement being made, I would not wait for half a day or so, as I would have done twenty years ago, but, after giving nature a fair test of from four to six hours, I should complete the labor with the forceps. And so with those cases where, for hours, our patient is annoyed with weak and ineffectual labor, in all the degrees, from pains which merely cause the abdominal tumor to descend without any appreciable advancement of the foetal head, to complete inertia of the uterine efforts, I regard it as good practice,—all the conditions for operating being present—to apply the forceps after waiting from two to four hours, according to the more or less complete absence of uterine effort.

In this last class of cases, I would first employ ergot, provided the os was sufficiently dilated, and the head well within the superior strait.

We frequently meet with an irritable and sensitive patient who will scarcely brook control,—but, in spite of herself, becomes fretful, restless, and despondent. The second stage is reached—uterine contractions are sharp and frequent, but lack the necessary vigor to cause but trifling advance of the child's head;—she will perhaps tell you that her pains are doing her no good, and implore you to complete the labor at once, or give her something that will render her unconscious of her misery. The present state of our art will not permit us to sit idly by, and allow our patient to wrestle with her ineffectual pains; and the result is, we are driven to the necessity of administering anæsthetics,—which, in my hands, especially chloroform, generally protracts the labor—or deliver by the forceps. In such a case, all the conditions being suitable for operating, I would unhesitatingly choose the latter.

These are the classes of cases in which I have more recently resorted to the forceps, thereby increasing the ratio of instrumental to the whole number of cases, and I am heartily satisfied with the results obtained by this method of treatment. My patients thus operated upon have invariably got up speedily, and made good recoveries.

CLINICAL CASES.

A CASE OF FULL-TERM EXTRA-UTERINE GESTATION OF THE TUBO-OVARIAN FORM.

WITH SPECIAL EXAMINATION OF THE SAC, UTERUS, AND APPENDAGES.

BY

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(With two woodcuts.)

THE interpretation of phenomena, whether natural or diseased, can be attained only through gradual processes, based upon independent predicates. These must be harvested by many laborers. It therefore becomes the duty of each, into whose path a grain may fall, to preserve it for the reckoning of the future.

For such considerations as these, as well as for its intrinsic interest, I have thought it well to put on record the following remarkable case of extra-uterine gestation; having made the section at the examination after death, removing the pregnant cyst, uterus, and appendages, subjecting them afterwards to a careful study and dissection. The present paper contains the results of my investigations.

“One of the most perplexing questions in the study of a case of extra-uterine pregnancy,” says a recent writer,¹ “is that of the class to which it belongs; and this is true after death as well as during life; consequently, the literature of the subject abounds in statements that are utterly unreliable.” In my description of this case I have endeavored to give as exact an account of the anatomical condition and relations of the parts interrogated, as a special macroscopic examination would permit; and if at times I should appear to become prolix, I trust that this will be pardoned on the score of my good intention. I preface my account of the autopsy with a brief relation of the

¹ Dr. James D. Trask, Astoria, N. Y. (*Amer. Jour. Med. Sciences*, April, 1876).

patient's previous history, gathered chiefly from the several physicians under whose more direct observation she chanced to come from time to time. Though not in attendance myself, I had seen her at various times during her stay in the hospital, and the case had become an object of some interest to a number of the profession in the city.

Previous History.—K. P., colored, a pluripara, about thirty-three years of age, of medium stature and rather full habit, entered the Freedman's Hospital in this city, November 28th, 1875. She was then supposed to be in premature labor, having experienced during the preceding night, and at the time of her admission, intense pains in the back and abdomen, their character being that of decided uterine efforts; though from her own calculation the full time of her pregnancy would not have expired until about the approaching Christmas. Opiates were administered at appropriate intervals to alleviate her sufferings, and the pains ceased that evening. There was no hemorrhage observed at that time. Subsequent to this attack she was sufficiently improved to leave her bed and to walk about the wards; and did not require any particular treatment, except on two or three occasions, when an anodyne was administered for temporary attacks of abdominal pain of which she complained.

At the time of her admission there was no indication of abnormal gestation, the abdominal tumor corresponding very well in size and general appearance to that of an eight months' pregnancy, in agreement with her supposition of the duration of her gestation. Her general condition had been good both before and since the beginning of this pregnancy; she had borne three or four children anterior to this time. Her oldest child, a boy fifteen years old, is still living; the youngest died about a year ago, being then about two years old. So that, in her case, there had been no previous condition of a decided and prolonged sterility. During the night of December 21st, she again appeared to have the symptoms of approaching labor, the pains became more decided and intense, and of the nature of expulsive efforts.

In the meantime, Dr. T. R. Wright was in attendance, the conduct of the delivery having been previously assigned him. After this Dr. A. H. Baker also visited the case several times; but the labor did not progress, as there had heretofore been

every reason to expect. Prof. Joseph A. Eve and Prof. Henry F. Campbell saw the case in consultation during the continuance of the pains.

The first indication that some abnormal condition existed was founded upon the peculiar action, or non-action, of the os and cervix. With the beginning of the pains, or at the time of the first vaginal examination by the gentlemen in attendance, the os was observed to be dilated sufficiently to admit the forefinger only partially; and this, notwithstanding the decided violence and pertinacity of the pains. During the course of the 22d of December there was some hemorrhage. In the afternoon of the 23d a clot was expelled from the uterus, containing a membranous mass, which could be spread out in a distinct lamina, and when put upon the stretch could be torn like a delicate membrane¹—as I am informed by Dr. R. O. Gercke, Superintendent of the hospital, to whom, in answer to my inquiries, I am indebted for some of the facts connected with the history of the case. This, I presume, was the decidua—developed within the uterus by the indirect influence of the ectopic pregnancy, as the uterus, in the words of Dr. Parry, “prepares to do its work precisely as if the fertilized germ had entered its cavity”—and its separation the cause of the hemorrhage observed. After the expulsion of this clot a rather fetid discharge, described as containing small pieces of “fleshy matter,” was noticed, lasting two or three days, and in quantity like an excessive leucorrhœa. The patient said she had felt the movements of the child up to the morning of the 23d; and I am informed by some of the physicians who saw her at the time that the foetal heart was heard on the 22d, and observed last about noon of the 23d.

Labor not progressing, and her pain being great, morphine and chloral hydrate were administered, and the pains finally ceased gradually. After the 24th or 25th of December, her general condition became by degrees about the same as before this attempt at parturition. During the pains, which are described as having been in their force and general characteristics identical with those of *bona fide* uterine labor, the vagina was but slightly dilated, and the os and cervix showed but

¹ Engelmann: The Mucous Membrane of the Uterus; Am. Jour. Obstetrics, May, 1875; pp. 81-2.

little change in their dilatability from first to last. She is said to have complained during the labor of violent constant pain in the left iliac region. On inquiry, I cannot learn of any indication of menstruation having been observed during her stay in the hospital. About the middle of May, ascites supervened with œdema of the inferior extremities; this becoming worse, respiration was seriously impeded, the œdema probably extending also to the interstitial pulmonary tissue. For this paracentesis abdominis was performed July 20th, by Prof. L. D. Ford, assisted by Dr. G. A. Wilcox, and about two gallons of serum removed from the peritoneal cavity; the subsequent dripping relieved her of a considerable quantity more. After the tapping her respiration was much relieved, and the swelling of the lower limbs diminished. She had now been strictly confined to her bed for three or four weeks; seemed gradually sinking; with the dyspnœa she had great drowsiness—it was difficult to arouse her. The symptoms became decidedly worse during the last two or three days, and she died at 1½ P.M. on the 26th of July, 1876.

Post-mortem.—Three hours after death an autopsy was held, there being present Professors Ford, Eve, Campbell, Geddings, Drs. Coleman, Eve, Wilcox, Goodrich, Gereke, Washington, and Mr. Bowers. I had been requested to perform the section and remove the specimen. A longitudinal incision having been made from near the pubes and extending up towards the ensiform cartilage, the sac began at once to protrude beyond the abdominal parietes, rendering it unnecessary to complete the crucial cut as I had expected, and indicating its large size, as well as the small degree to which it was confined to the adjacent viscera and peritoneum. Assisted by Prof. DeSaussure Ford, I separated the sac from the attachments which existed, here and there, between it and the intestines lying behind it. These adhesions were easily torn away from the cyst, by the fingers, with little force. Having removed the few posterior attachments, and having severed also the external borders of the broad ligaments from their continuity with the lateral pelvic peritoneum—taking care not to exclude any of the uterine appendages—the entire mass was held up by two of the gentlemen present, and its full liberation accomplished by a transverse incision through the walls

of the vagina, near its junction with the cervix. No appearances of special import were observed in the abdominal cavity. A small amount of dark blood escaping from the uterine and vaginal plexus after their vessels were cut, mixed with some ascitic fluid, was emptied from the subperitoneal pelvic cavity, during the progress of the discision, as it interfered with a view of the tissues then being severed. The cyst having been removed from the abdomen, and its anterior wall laid open, the foetus was seen within surrounded with liquor amnii. During my subsequent study of the sac and appendages, I made the following special observations and measurements:

The Sac was nearly round in its outline, though not entirely spherical, as its antero-posterior diameter was much shorter than the vertical and transverse, it being moulded upon its contents and by the pressure of its surroundings. This oblate form, however, was determined by the position of the foetus. Its dimensions—with the foetus in its original position, and after the liquor amnii had been allowed to escape, which probably caused some shrinkage—were these: transverse circumference, $23\frac{1}{4}$ inches; vertical circumference, $21\frac{3}{4}$ inches; oblique circumference, 23 inches; transverse diameter, $9\frac{1}{2}$ inches; antero-posterior diameter, 5 inches; vertical diameter, 8 inches. The external surface was quite smooth, except here and there at points where its suspensory attachments had been torn from it. Near the centre of the posterior aspect, about three inches from its inferior pole, was a patch of white fibrous tissue, its area being two inches vertically by one and a half transversely, very thick, and appearing as compact in structure as the fascicular ligaments. It was no doubt its chief posterior support, probably attaching it to the mesentery. On the anterior superficies six inches from the uterus, a thin, fringe-like membrane, adherent to the sac for two or three inches, grew upward; it was about seven inches in length, highly vascular, and probably nourished the distal zone of the cyst through its capillaries, drawing its supply from an attachment to some of the tissues above.

The Uterus did not differ materially in size and conformation from the ordinary unimpregnated multiparous organ. I, however, give the following external measurements: entire length from superior margin of fundus (where attachment of

sac began) to anterior lip of cervix, $3\frac{1}{2}$ inches ; posterior measurement the same ; circumference of fundus, $4\frac{3}{4}$ inches. The os externum and cervical canal were still sufficiently patulous to admit the round handle of an instrument $1\frac{1}{4}$ inch in circumference. Transverse diameter of fundus, $1\frac{1}{2}$ inch ; antero-posterior diameter, $1\frac{1}{4}$ inch. Circumference of intra-vaginal portion of cervix, $2\frac{3}{8}$ inches ; circumference of supra-vaginal portion, 4 inches. On opening the uterus by a longitudinal incision through its anterior wall and crossing this at right angles by a transverse incision from the uterine extremity of one Fallopian tube to that of the other, while the general corporeal cavity and that portion of it leading through the angles to the two Fallopian tubes were found to be contracted as compared with other pariparous¹ uteri, the os internum, like the os externum, was still somewhat dilated. The mucous membrane was smooth and of a pale red color. Anterior wall in median zone of body, $\frac{7}{16}$ of an inch thick ; superior fundal wall, $\frac{5}{16}$ of an inch.

The Right Fallopian Tube was $5\frac{1}{4}$ inches in length from its beginning at the right superior angle to the termination of its fimbriated extremity. It was of usual size and normal curve, downwards, backwards, and inwards ; its fimbriae but poorly developed. It was well attached to the right ovary near its termination, not by the fimbriated surface, but by that portion of the tube opposite to the fimbriae, they being turned outward and away from the ovarium.

Right Ovary, $1\frac{1}{2}$ inch long, $\frac{5}{8}$ inch wide, $\frac{7}{8}$ inch thick. Right ovarian ligament, $\frac{5}{8}$ inch long. On section of ovary two small corpora lutea were found, one larger than the other. Just opposite the adhesion of the extremity of the Fallopian tube, the larger corpus luteum was $\frac{1}{8}$ inch from the surface. Its section was $\frac{5}{16}$ of an inch long and $\frac{3}{16}$ of an inch wide.

¹ I have ventured to introduce a new expression, I believe, as I have just here felt the need of it ; not being aware of any single term, now in use, to convey the idea. We already have the adjectives *multiparous*, *pluriparous*, *uniparous*, *primiparous*, *imparous*, etc., as applied to the present condition, or to the past gestative life of a woman, or of the uterus. By analogy with these, we may have as well, in comparing two or more uteri, to express similarity of past gestative history—in age, number of births, etc.—*pariparous*, (*par*, *paris*, equal, similar ; and *pario*, to bear) ; or if it is preferred, from the Greek, *homotocic* (ὁμος, the same ; τοκος, *partus*, gestation).

The smaller body was $\frac{3}{16}$ inch from the surface of the ovary, and just over it, on the investing membrane, was a deeply tinged congested spot. The smaller body was about half the size of the larger; both were filled with small white granules easily removed from their convoluted walls.

The Left Round Ligament was about twice as large as the right, the latter being of the usual size. The right round ligament was inserted lower down than the left. While the right tube was inserted into the free right superior angle of the uterus, the junction of the left tube with the uterus was about half an inch higher up, and just under that portion of the sac where it began to rest upon the fundus. The left round ligament had a higher insertion than the junction of the right tube with the uterus. While the left tube began at the uterus a little above the insertion of the round ligament, the right tube began at the same altitude as both its round and ovarian ligaments, the former before and the latter behind it. I mention these slight deviations in symmetry as they have some bearings on the case.

As will be seen from the cuts, the sac rested directly upon the fundus of the uterus. Its principal attachment to that organ was by means of a ligamentous capsular structure extending from the circumference of the fundus to the inferior surface and up the sides of the sac for four or five inches—the inferior zone of the cyst being contained within this cup-shaped ligament—the two together reminding me of the familiar toy of cup and ball, or of a flower in its calyx. This structure was a continuous, thick, ligamentous membrane, extending without any interruption from a little to the left of the median line in front across to the right, thence around the posterior superficies of sac and fundus to a point corresponding to the angle made by the folding down of the left Fallopian tube, presently to be described. The calyx then ceased to pass as a continuous lamella from fundus to under surface and side of the sac, as it had up to this point, but its lower portion still continued around from this point towards the front again to the left superior angle of the uterus. After it became incomplete above, it was supplemented by separate bands extending up from the body of the uterus to the sac, and up the sides of the latter, some under and some over the left Fallopian tube,

the first portion of the tube curving around the left inferior region of the cyst in the interspace where the calyx was somewhat incomplete. That is, the calyx formed a continuous and complete capsular ligament around the base of the sac through an arc of over 280 degrees. This broad attachment of the sac to the fundus, which I have termed the calyx, was not identical with the proper structure of the sac wall; it could be easily separated in its entire extent by tearing it away with the fingers from the sac. This was not the case in its relations with the fundus; it apparently grew out from its periphery, and had evidently been developed from the uterus, when the sac and contents increasing in size and their demands for nutrition and support, rolled over from the left iliac region and took its final position upon the fundus. These proliferations were then thrown out, both affording to the pregnant sac a steady support below, and sending blood to the fœtus through its vascular structure, as it was seen to be well provided with capillaries. This ligament could without much difficulty be separated into two laminae. By its long residence upon the surface of the fundus, that portion of the inferior zone of the sac which rested directly upon it had become slightly adherent to the fundus; but I easily separated the two entirely from each other, showing that there was no communication between the cavity of the uterus and that of the sac, unless we include its possible capillary communication through the ostium uterinum of the left Fallopian tube, which from the evidences in the case, was probably closed. The thickest portion of the calyx was in the median line on its posterior aspect, where it had below, near the uterus, a slightly ribbed appearance like a palm-leaf.

The Left Fallopian Tube.—Immediately upon the removal of the cyst and contents, with the uterus and appendages from the abdominal cavity, we saw, without further dissection, that an intimate relation had existed between the sac and the left tube. Running up from the left superior angle of the uterus, in a curve whose radius would about equal that of the circle formed by the projection of the sac upon a perpendicular plane, parallel with its posterior aspect, viewing it from before—the tube was situated somewhat to the front and a little beneath the roundness of the cyst, and attached to it by delicate fibres. The superior extremity of this curve was three inches from the junc-

tion of the tube with the fundus. At the first casual examination, the left tube seemed to terminate at the end of this curve, as no more of it could then be seen ; and, still more plausibly, because small, reddish fibres, radiating from its extremity and binding it to the sac, appeared as if they might be the fimbriæ, clinging to the sac, and that the cyst might have begun its development at this part of the tube, or at some point intermediate between it and the left angle of the uterus. But I afterwards found an entirely different solution, which involved no element of doubt.

During the subsequent study of the specimen, I made this interesting discovery. Having removed the fascicular attachments running over the left tube, holding it up, in the curve described, to the left inferior region of the sac ; and dissecting away from the tube that portion of the left broad ligament also which was necessarily carried up with the tube, and lay against the under surface of the sac on the left side ; I found that the continuation of the broad ligament outward to the left and somewhat posteriorly, still continued to embrace the under left surface of the sac, and that in the upper margin of this portion of the ligament, *a second portion* of the left Fallopian tube existed, heretofore concealed, equal in length with the first portion—that is, three inches—which at its termination entered the cavity of the sac by a canal three lines in diameter.

After leaving the left superior angle of the uterus, the left tube ran around and up the side of the sac, and was attached at the upper extremity of the arc thus formed, as before described. It then became decidedly enlarged beyond its former size—as we should expect, as it continued outward to form the pavilion—and was then folded downwards abruptly upon itself, entirely changing its former direction, the second portion running downwards and slightly backwards to the under surface of the sac, on the left side. The two limbs made an angle with each other of about thirty-five degrees ; this angle does not refer to the bend of the tube upon itself, for that was a complete and abrupt folding down and apposition of surfaces ; but to the angle formed by the direction of the two after their departure from this point. The second portion of the tube—the *ampoule* of Henle—after extending for three inches downwards and backwards, became continuous with the structure of the sac

wall, just at the point where the tube can no longer be seen in Fig. 1. It was about three-eighths of an inch in diameter just

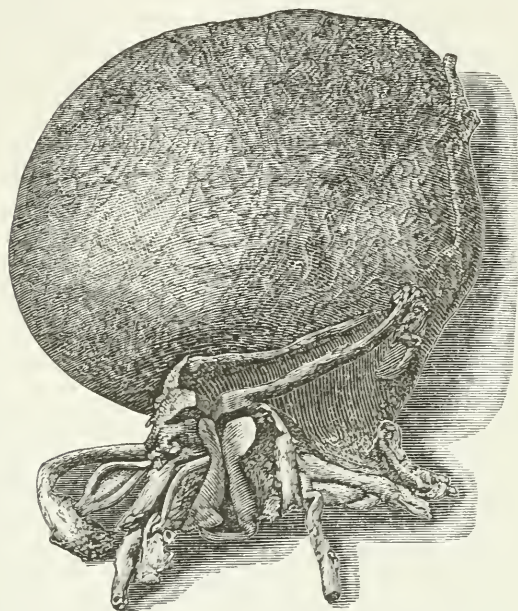


FIG. 1.—Anterior view of the external surface of the sac and its relations. The left Fallopian tube curving upwards and outwards, and after bending downwards at an acute angle, abruptly entering the sac towards its under surface. The loose tissues on the left, under that side of the cyst, are the remains of the broad ligament, after dissecting it from the sac, tube, round ligament, and larger vessels. The uterus incised as in Figure 2.

before it entered the sac; its wall about half a line in thickness. The canal of the first portion, or isthmus of the tube, was still minute, if not closed entirely; its walls seemed atrophied and weak, perhaps from stretching and long pressure of the sac; hence, to explore the second portion, I made an incision into its wall about an inch and a half from the sac, and introduced through this opening the round ivory handle of an aneurism needle. It passed through the tubal canal into the interior of the sac, and still further on for about two inches. Here the instrument passed out of the canal of the tube, which became more delicate within the sac, and was incorporated with its posterior wall. The point where the instrument passed out

was under the placenta. The apparent termination of the tube was, therefore, between the attachment of the placenta and the sac wall, with the structure of which the tube seemed to become blended. The manner in which the tube entered the sac was abrupt, not gradual; there was no appearance of an ovoid enlargement of the tube, but its junction with the sac was like that of a large, round melon with its slender vine.

The Left Ovary could not be found. It might be urged by some, that it either became bound down to the peritoneum in the left iliac region, after being pressed upon by the weight of the superincumbent pregnant sac, when in the first stages of its development it occupied the left side of the abdomen, and was accidentally left in the cavity during the removal of the cyst and its connections; that it was atrophied by the same pressure, and lost sight of, by reason of its small size and on account of the changes of form and relation in the parts; or became involved in the posterior portion of the calyx; or inclosed in the interior of the sac—provided there had been early rupture of the cyst; or was itself greatly concerned, together with the left tube, in the formation of the sac. These suggestions I at first thought might be topics of some debate. After a thorough investigation, I found that the last was the only tenable explanation—that the gestation belonged to the tubo-ovarian class, which I shall be able to demonstrate beyond cavil. In my search for some structure that might be recognized as the metamorphosed ovary, or even its ligament, I observed, in a part of the left posterior wing of the calyx, formed by the posterior wing of the left broad ligament, a band of tissue somewhat thicker than that adjacent to it, which also appeared to contain some red fibres. The idea presented itself that this might have been formed from the left ovarian ligament, carried up in the left broad ligament, as it arose to embrace the sac and assist in forming its protection and support; as these ectopic products appropriate so freely and make subservient to their own nutrition, stability, and vitality, whatever natural tissues happen to be within their proximity and influence.

I probed the right Fallopian tube its entire length, from the right superior angle of the uterus to the fimbriated extremity, though as the instrument at hand was too large for the purpose I had to employ some force. I found it impossible to

probe the left tube throughout in the same manner, as its first portion was much more contracted than that of the right tube, and its walls also too weak to sustain the amount of force required in a forcible passage of the probe.

Interior of Sac, Fœtus, Membranes, etc.—The sac having been opened by a free vertical incision through the whole extent of its anterior wall, a remarkably well-preserved female fœtus was seen within, bathed in an ample supply of liquor amnii of light brown color—probably a pint and a half in quantity. There was no decidedly unpleasant odor arising from the interior of the sac. The fœtus was evidently one of nine months' gestation. The head was provided with a full growth of hair, and the finger-nails were perfect. The skin was remarkably well preserved; merely the cuticle with its pigmentary layer had been rubbed off at some of the prominent parts presenting anteriorly, from long maceration in the amniotic fluid, and from handling the sac and contents during their separation and removal from the maternal abdomen. There was not the least tendency to putrefaction, and this in accordance with the preservative qualities attributed to the liquor amnii; the fœtus being shut up within its unbroken envelope, surrounded by the antiseptic fluid provided for it by nature, had resisted putrefactive changes during the seven months of its retention. "An important property of the amniotic fluid," says Prof. Austin Flint, Jr., "is that of resisting putrefaction and of preserving dead tissues. It is stated by Robin to be the best fluid for the preservation of the embryonic tissues, when it is desired to keep them for examination" ("Physiology," p. 904).

The Position of the Fœtus in the sac was nearly the reverse of the normal position in utero in ordinary cases of natural labor with vertex presentation. That is, it approximated the second condition of Barnes, preceding difficult breech presentations, whether of the anterior or posterior variety, viz.: it was in the abdomino-anterior position, with the legs extending upwards and approaching the face. The head was situated principally in the left side of the fundal zone of the sac; the right shoulder in the right side of the fundal zone, and the other parts disposed as seen in Fig. 2, where the sac is represented, opened by a crucial incision, with the fœtus *in situ*.

The Membranes, though generally investing the fœtus, had

apparently fallen away from the anterior prominent points after softening and pressure against the abdominal walls of the mother. They still surrounded some portions of the limbs, but some folds were also observed to pass around the trunk between it and the limbs—between the folded-up legs and the abdomen, and again under the right arm and over the right thoracic space; as if this strange arrangement of its investment had been caused by the energetic movements of the child, perhaps about the time of labor.

In those portions where the membranes were best preserved, the two layers of the amnion and the chorion were still beautifully shown. Where they covered the placenta, after the delicate non-vascular layer of the amnion was raised, the chorion was seen, with its large vessels, the size of a crow-quill, radiating from the funis about an inch apart from each other; and beneath this again the densely crowded and innumerable vessels of the foetal tufts, constituting the great mass of the placenta.

The Placenta was attached to the interior of the sac wall, and situated in its left inferior region; the lowest portion of its circumference being just opposite the point where the left Fallopian tube was attached to the outside of the sac, or became continuous with its wall, and entered its cavity. The placenta was twenty-one and a half inches in circumference; seven inches in one diameter, and about eight inches in another; its greatest thickness one and five-eighths inches.

The Umbilical Cord was nine and a half inches long and about five-eighths of an inch in diameter. It did not join the placenta at its centre; was quite eccentrically inserted, being only two inches from the inferior margin and five inches from the superior. The point where the left Fallopian tube entered the sac was three and a half inches from a point opposite to the junction of the cord with the placenta, measured on the outer surface. The indications of this eccentric union of the cord with the placenta, the direction in which the asymmetry existed, and the proximity of the site of the placenta to the left tube and the usual situation of the left ovary, are obvious.

The placenta was firmly attached to the sac; it required considerable force to tear it away from the wall. The site from which I had removed some portion of it was left rough

and shaggy from the broken ends of the tufts of the chorion which still remained adherent to it.

The Sac varied in thickness from about the third of a line

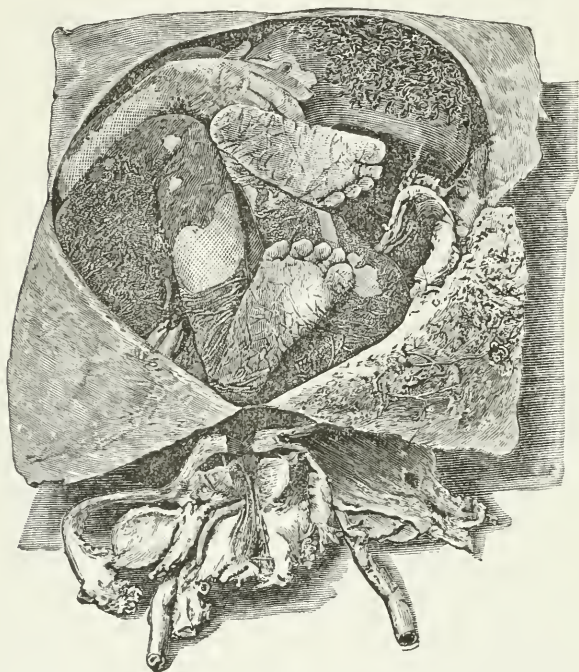


FIG. 2.—The sac opened by a crucial incision through its anterior wall, and the flaps reverted. The fœtus in its original position, the membranes having been removed. The umbilical cord on the left, indicating the situation of the placenta. The uterus opened by a vertical incision in the anterior median line, and by one running transversely between the angles. The right ovary laid open longitudinally. The “shaggy” appearance of the inner surface of the left inferior flap, caused by the extremities of the fetal tufts still adherent, after the outer margin of the placenta had been torn from that portion of the sac wall.*

to one line; average thickness about half a line. It was formed, apparently, of inelastic, fibrous tissue; small capillary vessels were seen running through its structure. Its inner and outer surfaces had nearly the same appearance. In some

* The cuts are from photographs from Usher's Gallery, Augusta, Ga.; to the proprietor and his assistants I am indebted for courtesies received.

portions its wall could be separated into two laminae. It had the same general appearance to the naked eye as the calyx-like ligament that supported it below, viz., fibrous tissue, striped with the numerous capillary vessels supplying its structure. These small vessels were much more abundant near and around the uterus, radiating from the direction of the fundus, finally becoming fewer and more widely scattered as we approach the superior pole of the sac.

Vessels.—The main trunk of the uterine artery on the left side was still enlarged, and measured three lines in diameter; the corresponding artery on the right side was considerably smaller, measuring about two lines in diameter. The left ovarian artery was also larger than the right. Probing the former with an instrument the size of an ordinary silver probe, from its outer extremity in the left broad ligament, the instrument passed through the main branch to the left superior angle, and then appeared in the open mouth of one of the vessels severed in the vertical section of the uterus, six or eight of which were seen with their gaping canals in the middle muscular layer of the corporeal wall. The branches of the ovarian pampiniform plexus were larger on the left side than on the right; as also were the vaginal and cervical venous plexus on the left. A probe passed through the enlarged main trunk of the left uterine artery stopped at the left superior angle of the fundus, just where the left ovarian artery passed in to supply the inner muscular wall and anastomose with the uterine. No vessels of any important size could be found leading directly to the cyst, as we sometimes see thrown out in the exigency of their requirements, or formed from the exaggeration of small normal branches, to supply abnormal or ectopic growths. Hence the foetal sac must have been sustained in its early stages of development—when it resided in the left iliac region, and before it had any connection and intimate relations with the uterus—by the pampiniform plexus of the left ovarian artery, supplying the ovary and Fallopian tube. After it had taken up its habitat upon the fundus, receiving its strong attachment with the uterus—through the cup-shaped pseudo-membrane, assisted on the left by the broad ligament—and called for an increased supply of blood; this must have been answered through the vascular layer of the calyx, which was well pro-

vided with numerous arterioles and small venous branches, somewhat larger, contained in a middle layer, between the two fibrous laminae constituting the cortical surfaces. So that the thick vascular walls of the uterus—no doubt much enlarged during gestation—must have acted as a great sponge, or diverticulum; rapidly receiving the blood through its proper vessels, and thence transmitting it through the vascular layer of the calyx to the pseudo-matrix, which was serving in its stead.

Weight. The sac and contents, after draining off the liquor amnii, with the uterus and appendages, weighed seven pounds and a half. The foetus alone weighed five pounds and a quarter.

From the foregoing description I think it will now be agreed that of the several classes of extra-uterine foetation recognized by systematic writers, the present example was plainly of the tubo-ovarian form. Though the extremity of the left tube was in union with the sac wall, and the canal of its pavilion communicated with its cavity, the cyst was not developed in the continuity of the oviduct—it was not an instance of the purely tubal form. (1.) Because the free portion of the left tube not at all involved in the sac—even after its fimbriated extremity had been contributed, so to speak, to assist in forming the cyst—was still found to be a little longer than the tube on the right side. (2.) The almost spherical shape of the cyst. When formed in the continuity of the tube, the sac is usually ovoid or spindle-shaped; “when formed in the fimbriated extremity, the sac developed partly out of the dilated mouth of the tube, and partly by attachments to neighboring structures, especially the ovary, has usually a spheroidal shape;” and as a corollary to this reason, (3.) The abrupt manner in which the tube joined the sac—not by a gradual dilatation of its calibre, the dimensions changing at once from a tube a little over an inch in circumference to the spheroidal cyst, whose greatest circumference was over twenty-three inches. (4.) The close resemblance, in its general appearance, which the sac bore to an ovarian cyst. (5.) The absence of the left ovary; which indication is greatly strengthened when viewed in connection with the fact, that (6.) The point in the pelvic cavity where the tube joined the sac was the proper situation of the ovary, modified

only by the slight deviation caused by the upward and lateral growth of the cyst. (7.) The fact that the left broad ligament embraced and was attached by its posterior wing to the left inferior surface of the sac. (8.) That the gestation went to full term, without any indication at any time—before or after death—that there had been rupture of the cyst. “It may be said generally,” remarks Dr. Barnes, “that the sac bursts the earlier, the nearer its seat is to the uterus. The tubo-ovarian sac may not burst until near the ordinary term of uterine gestation, whilst the tubal or interstitial sac usually bursts at dates varying from six weeks to three months.” * * * * “The tube,” continues the same author, “although consisting of a mucous and a muscular coat, is ill adapted to keep pace in growth with the rapid development of the ovum. The adaptation is not simply, as in the case of uterine gestation, obtained by growth of the tube *pari passu* with its contents; the tube is stretched as well, and there comes a time when the stretching exceeds the distensibility of the tube, the sac bursts, and the contents escape into the peritoneal cavity.” There having been no previous indication of the hemorrhage and collapse, so marked after ruptures of the cyst, combined with the other evidences, excludes the idea of this being a case of early rupture of the original envelope, resulting in the abdominal form.

The entire disconnection of the uterine extremity of the Fallopian tube—so far as any incorporation was concerned—with that portion of the sac against which it lay, together with the independence and easy separation of the inferior zone of the sac and the thick wall of the fundus upon which it rested, precludes also the possibility of an original interstitial gestation.

The peculiar curve and bending of the attached left tube is easily understood, if we imagine the gestation at first going on in the left iliac region; the sac as it increased in size rolling a little forward and then centrally, thus resting upon the tube; contracting adhesions with it; and as its dimensions still further increased, carrying the tube upwards attached to its external surface, in a curve determined by the manner in which it at first happened to rest upon the oviduct. The abrupt bend upon itself, just about the juncture of the isthmus and the ampoule may have existed before conception, as “it is not rare to

find the tube doubled up, either before or behind, and bound down by pathological adhesions.”¹

The comparatively large size of the left round ligament, would seem to indicate the development of its muscular fibres, while acting as a support to maintain the normal position of the organ resisting right latero-version, as it was being borne upon by the enlarging foetal cyst in the left region of the pelvis, pushing against the left side of the fundus, and thus having a tendency to force it to the right. In the case of Mr. Hutchinson, referred to below, the cyst had succeeded in forcing the uterus over to the right. According to Rainey, the round ligament is rather a muscle than a ligament, consisting principally of striated fibres.

Barnes, Campbell and Hecker have called attention to the fact that, as in the present example, the left tube has been the one concerned in a considerable majority of cases. The former has offered as an explanation that the left tube is more liable to displacement and compression by the sigmoid flexure lying in close relation to it, and its being often disturbed by feculent accumulations.

There are other topics of interest suggested in the study of this case, which I omit for fear of prolonging the report beyond its proper limits.

As to the question of the propriety of operative interference, I briefly refer to a few eminent authorities.

Mr. Jonathan Hutchinson, senior surgeon to the London Hospital, published in the *Medical Times and Gazette*, August, 1860, a tabulated paper of one hundred and two cases of extra-uterine foetation. He reports a case (*London Lancet*, July 19, 1873) which in many particulars bears a striking resemblance to the subject of this paper. In his case also the left Fallopian tube was the one involved, the broad ligament passed downwards in front of the cyst, and he was unable satisfactorily to identify the remains of the ovary. In his concluding remarks, in view of his experience with his last case, and quoting also from his former treatise the opinions he had formed from a study of the large number of instances he had collected, he says: “Those opinions I may briefly sum up in the following

¹ Barnes on Diseases of Women ; Chapter, the Fallopian tubes.

practical rule—that extra-uterine foetation cysts ought not to be meddled with in any way, either by puncture or incision, until suppuration has occurred, and an abscess fistula has been formed.”

Dr. Campbell, of Edinburgh, “who collected eighty-five cases of extra-uterine gestation, showed that sixty-two recovered, whilst twenty-three died as a direct consequence of the abnormal pregnancy; of the sixty-two in which recovery took place, in twenty-one the foetus remained quiescent through life for periods varying from four to fifty-six years, and in the rest its removal had been effected by ulceration. He advised that abdominal section should not be performed until after the system had been restored to its unimpregnated condition, and nature had evinced a disposition to remove the extraneous mass.” (Barnes: Chapter on Extra-uterine Gestation.)

Dr. Parry’s work, just published, on Extra-uterine Pregnancy, based upon the consideration of five hundred cases, is perhaps the most extended treatise and highest authority extant upon the subject of ectopic foetation.¹ Not having the work at hand, I quote a summary of his views from the review, by Dr. J. D. Trask, in the *American Journal of Medical Sciences*, April, 1876:

“The prospect of saving the life of the child ought not to be taken into consideration, and the primary operation cannot be too emphatically condemned. In proof of this, if we compare the mortality of cases left to nature, with those following primary gastrotomies, that of the former class is 52.65 per cent., while that of the latter is seventy per cent., or 17.35 per cent. greater than if they had been left to nature. The mortality following the secondary operation, that is, months or even years after the termination of pregnancy, stands 38.88 per cent., or, as compared with those left to nature (52.65 per cent.), 13.77 per cent. in favor of the operation.” He agrees with Mr. Hutchinson that the cyst should not be disturbed in any way until suppuration has occurred and a fistula has been formed, as “the operation is thus degraded from the important

¹ Extra-uterine Pregnancy: Its Causes, Species, Pathological Anatomy, Clinical History, Diagnosis, Prognosis, and Treatment. By John S. Parry, M.D., Obstetrician to the Philadelphia Hospital, etc., 1876.

and dangerous procedure gastrotomy, to the simple and less dangerous performance of opening a large abscess."

The expectant treatment pursued in the present case seems, therefore, to have been borne out by the above authorities; especially, as the post-mortem examination revealed the fact, that there had not been any intimate adhesion between the wall of the cyst and the parietal peritoneum. At the same time, had it been possible, during life, to diagnosticate the minute pathological conditions and relations—as to circulation, mode and extent of attachment, and the exact variety of the abnormal pregnancy—which have been brought to light, with all the advantages of a careful study and dissection, after death; there are few, doubtless, who would in this case have condemned the secondary operation.

CASE OF ECLAMPSIA, VERSION, RUPTURED PERINEUM (WITH OPERATION), RUPTURED UTERUS, DEFORMED PELVIS, WITH REMARKS.

BY

D. WARREN BRICKELL, M.D.,

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On the 18th of April, 1875, I was invited to the Charity Hospital in this city to operate for restoration of ruptured perineum, in the case of Lucy ——. I received the invitation eight hours after the occurrence of the rupture, and I operated nine hours after its occurrence. The history of the case is briefly as follows:

Lucy—negress—æt. nineteen years—married—native of Louisiana—in New Orleans one year—entered Hospital Feb. 24, 1875. Pregnant first time. At the time she was considered well in every respect, except that the lower extremities were swollen. This increased gradually until about April 8th, when it aroused suspicions of albuminuria, but no albumen was found on careful test. No headache or other disagreeable symptom was complained of, but her face and upper extremities now became involved in the swelling.

During the day, April 16th, she had distinct and progressive symptoms of labor; these increased gradually until twelve m. of 17th, when she suddenly went into violent puerperal spasm. Consciousness soon returned, and at 1.30 p.m. she had a second spasm. In a few minutes she had a third, and then she remained unconscious. Remedies were resorted to, but, although there were extreme efforts, no progress was made, and at five a.m. of the 18th she was delivered, by version, of "a large child." I give the language of one of the attendants, though the child was not weighed. At seven a.m. (two hours after delivery) she had a *post-partum* spasm, and three others within three hours; then three more within one and a half hours—remaining unconscious during this time.

At two p.m. I visited her to operate; she was semi-conscious; temperature somewhat elevated; pulse 135, but round and smooth; generally bloated appearance, with tongue swollen; respiration 28. Considering what she had passed through, I regarded her general condition as fair. The laceration of the perineum was very severe, both sphincter and muscles being completely severed, and the surfaces of the wound being very ragged. As she complained openly when put on the table, and as I was determined to do the operation deliberately and thoroughly, she was well chloroformed. After the operation she was removed from the table to her bed, and before I left the ward she had quite sufficiently aroused from the chloroform to receive my injunctions about lying still, etc.

At nine a.m. next morning, the 19th, her pulse was 131, temperature 102, and respiration 28. She was perfectly conscious, but her tongue protruded so that enunciation was difficult. She had passed a pretty good night, kidneys were acting well (with no albumen in urine), and general swelling subsiding.

At nine a.m. of the 20th there was no apparent change in general condition, save that the temperature was 103, and the tongue had receded into the mouth. Things looked really favorable.

At nine a.m. of the 21st I found an unfavorable change. The temperature was up to $104\frac{1}{4}$, and the abdomen was tympanitic and painful to the touch. Peritonitis had set in during the night.

At nine A.M. of the 22d she was worse. The temperature had been 105 the evening before, though it was now but 103. She had been very restless, and diarrhœa had set in.

From this time she tended downward, though slowly, and at seven P.M. on the 25th she died. An autopsy was held at eleven A.M. of the 26th, or sixteen hours after death.

Results of Autopsy.—1st. There was no union at all of parts operated on. While she was yet living, and in consequence of there being no suppuration and the parts being fairly in contact, I had expected to find partial union; but there was none.

2d. On opening the abdomen there were all the evidences of a diffused peritonitis. There were about six or eight ounces of pretty clear serum floating above as much more fluid that was apparently sero-purulent, with numerous black specks incorporated. The intestines were everywhere glued together by coagulable lymph, and the omentum was closely glued to them.

3d. The *uterus* was found to be ruptured extensively on the right side, the rent extending from the median line of the fundus down fully one-half the length of the body, and the opening was in a gaping and sloughing condition.

4th. Right ovary of dark blue color, with its right extremity in a disorganized or sloughing state. The condition of the left ovary, in marked contrast, was of pale white hue and perfectly firm. The right ovary was the seat of the corpus luteum.

5th. No adhesions of the womb in any direction.

6th. No coagula in the abdomen, and no signs of there having been any beyond the black specks described as existing in the sero-purulent fluid found around the womb.

7th. Every portion of the body of the uterus not involved in the rupture was firm and solid. The neck of the organ was also solid, save the inferior extremity or lips. This latter tissue was soft and flaccid, but there was no evidence of injury or of inflammatory action.

8th. *Cellulitis.*—Everywhere between the bony pelvis and its contents there were the palpable results of active cellulitis. In sweeping the pelvic contents out with the knife, this condition of things was beautifully illustrated. The cellular structure was gorged with serum, and the knife would lay open innumerable bags of water. Between the bladder and the pubic bones this condition was most conspicuous.

At the same time it was remarked that there were no similar effusions in the duplicatures of the peritonæum, or between that membrane and the organs to which it was applied.

9th. *Bony Pelvis*.—The bony pelvis was preserved in the wet condition. Stripped of its contents, it measured as follows :

Superior Strait.—Antero-posterior diameter, $3\frac{1}{2}$ inches ; transverse and oblique diameters, four inches. Opening tolerably symmetrical ; promontory of the sacrum a little inclined to the right.

Inferior Strait.—Antero-posterior diameter (from pubic arch to junction of sacrum and coccyx), four inches ; transverse diameter, $3\frac{5}{8}$ inches.

The normal diameters of the pelvis, according to Cazeaux, are : *Superior Strait*, antero-posterior, $4\frac{1}{4}$ to $4\frac{1}{2}$ inches ; transverse, $5\frac{1}{4}$ inches ; oblique, $4\frac{3}{4}$ inches. *Inferior Strait*, antero-posterior (from pubic arch to point of coccyx), $4\frac{1}{4}$ inches, increasing in labor to $4\frac{3}{4}$ inches ; transverse, $4\frac{1}{4}$ inches ; oblique, $4\frac{1}{2}$ inches.

I have always regarded the normal diameters of Cazeaux as rather excessive, but the extraordinary difference between them and the specimen under consideration will be at once remarked.

Remarks.—Great interest attaches to this case.

1st. Here was a clear case of uræmic convulsions, in which extensive laceration of the perinæum occurred. Notwithstanding the parts were brought together in a most complete manner, and were held together while she lived, no union at all ensued. True, three different obstacles, independent of any action on the part of the patient, would seem to present themselves against adhesion, viz. : general vitiation, length of time elapsing (nine hours) before the operation, and extraordinary internal inflammation. I do not pretend to assert more than a theoretical belief in the premises. Even if a patient were operated on within an hour or two after the rupture, it would seem that grave doubts of adhesion might be entertained where the general vitiation was such as to produce profound uræmic convulsions, with the antecedent phenomena. And yet, when we open the body of this patient, we find all the evidences of what is generally called *adhesive* inflammation of the

peritoneum; and if of this membrane, why not of the lacerated surfaces? The answer would seem to be plain, viz.: that this effusion by the peritoneum of a "coagulable lymph," which applies itself in such manner as to glue the viscera together, is not the action demanded of the lacerated surfaces we have sewed together. On the contrary, if it be true, as Simon says, "that tissues are independent of blood-vessels, except for the administration of food and the removal of refuse," and "that they develop and grow by the life of their own germs, and according to the several patterns which they respectively represent," then we can readily comprehend that under existing uræmic vitiation, while the peritoneum may shed forth any amount of coagulable lymph, there may be as between the lacerated surfaces of the perineum an absolute want of cell reproduction and election, and consequently no union. In other words, it is not inflammation that is demanded to produce union—not pathology, but unimpaired physiology.

In point of prognosis, then, this is an interesting proposition for the medical man, both in a professional and a protective point of view. When we understand more thoroughly the intricate pathology of uræmia, it will be far better for us. Now we can only say to ourselves and to the friends of such a patient operated on, that the vitiation arouses theoretical doubts of adhesion occurring, as the tissues sewed together cannot be called healthy.

But, if such tissues, promptly operated on, are surrounded with grave doubts of adhesion, our prognosis must certainly be more unfavorable after the lapse of many hours before operation, as in this case. When I operated it was only with the view of taking all the chances. The tissues were turgid, ragged, and swollen. It is by no means an uncommon thing to see sloughing of these lacerated parts take place, even where no effort is made to hold them together artificially, and the impression conveyed to my mind was that this was a case in which such action was to be anticipated. Independent of any uræmic vitiation, we know that long-continued labor, with distention of these soft tissues, tends to degrade. During the past winter I have seen in the Charity Hospital a moderate rupture of the lip of the uterus result in sloughing and nearly fatal septicæmia.

This occurred after efforts of the patient to deliver herself during six or seven hours in second stage.

2d. The *post-mortem* discovery of rupture of the uterus, in this case, must interest all readers whose minds are lent to the study of Obstetrics. I think there is no doubt at all that not one medical man who saw this case from beginning to end ever for a moment suspected the existence of such a lesion. The supervention of peritonitis, of course, led me to fear that some lesion of lips, or internal surface, may have been inflicted in performing version, but no more. At the same time, no fact is better established than the one that even fatal peritonitis may ensue on what has been, to the best of our judgment, the simplest and easiest of labors.

From the books we are very apt to receive the impression that the diagnosis of rupture of the uterus is a very easy matter. If, during active labor, there be sudden and fixed cessation of uterine effort, if the patient at the same time is conscious of a sharp local agony, and a "giving way of something," if there be recession of the child, if the presence of the child in the cavity of the abdomen be established, and if there be the speedy supervention of the phenomena of shock or hemorrhage, then the diagnosis is easy. Indeed, if there be sudden cessation of active labor, with marked phenomena of shock or hemorrhage, the diagnosis may yet be clear. But this case clearly teaches that formidable rupture may take place and without manifestation of any of those salient symptoms which lead to diagnosis, and to this extent it is surrounded with unusual interest.

In consequence of the stupor resultant on the uræmic spasms and the administration of chloroform, the patient herself uttered no complaint which could have led to suspicion of the lesion, and the gentleman who performed the version perceived no other suspicious symptoms. If the rupture occurred before version, notwithstanding the rent was large, there was not even partial escape of the child into the abdomen. If it occurred during version, one would suppose it would be perceived by the operator. I wholly incline to the belief that it occurred before version, and that cessation of uterine action was not noticed by those in attendance. The gentleman who delivered the placenta noticed no unusual hemorrhage, and up to the hour of my operating on the perineum, there had been no

general signs of shock or serious hemorrhage. At the time of operating there were no such signs, and the amount of blood flowing from the vagina was only such as we witness every day after normal labor. More, the uterus at this time was firmly contracted. I handled it freely to produce contraction, because I had to place a large sponge against the os to prevent blood from flowing over the lacerated surfaces, and I had, at the same time, to guard against any chance of concealed hemorrhage.

3d. It is very clear that no "large child" was at all likely to be delivered by even great exertion of the woman through a pelvis of such dimensions, and protracted effort could only endanger both mother and child at every step. In the history of this case, we have only another glaring illustration of the effect of the dilatory practice which prevails in our profession, and which finds its warrant in nearly all our "text-books" and lecture rooms. The legitimate province of the obstetrician is to recognize trouble early and to act promptly. If we wait until women wear themselves out in vain efforts at delivery, then must we anticipate failure of our best directed efforts, with all the odium which attaches to failure. If operative procedure is warranted late in the progress of a difficult case, then much more thoroughly is it warranted early; and this must, sooner or later, become the recognized maxim in obstetrics.

CASE OF VAGINAL CYST.

BY

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Mrs. L. æt. 40, married, consulted me a few months ago, with reference to the following condition:

For nearly one year she had been troubled with an itching and burning sensation about the external genital organs, vesical tenesmus, and often excessive pain during sexual intercourse; these symptoms were constantly becoming more aggravating. Upon examination I found a tumor protruding from the

introitus vaginae, distinctly visible without separation of the labia; it was attached to the lower third of the posterior vaginal wall; and, when brought entirely into view, by lifting the anterior wall with Sims' speculum, appeared like a distended intestine, smooth and glossy. There was slight prolapsus, but no rectocele. The vagina surrounding the tumor was considerably thickened.

At this time, Mrs. L. would not consent to any surgical interference; but after four weeks, the pain and suffering having increased, she again sought my advice. Upon this examination I found that the growth had not increased in size, but its smooth surface was covered with a vascular membrane; and whereas formerly the growth appeared to arise freely from the surface of the vagina, it seemed at this second examination to have its origin in the submucous tissue, and in its growth to have pushed the mucous membrane before it.

After puncturing the cyst, and collecting its contents, a small piece of the wall was removed, and the inner surface of the cyst freely painted over with tincture of iodine. The cavity was $1\frac{3}{4}$ inches long, and $\frac{3}{4}$ of an inch wide. The recto-vaginal septum was left very thin.

The parts healed rapidly, and the result was entire relief from the symptoms previously mentioned.

The contents of the cyst consisted of a thick mucilaginous fluid, which, upon microscopic examination, contained epithelial cells and globules of fat. The cyst wall was vascular, and made up of densely-packed bundles of connective tissue, and its internal surface was lined with pavement-epithelium.

CASE OF GENERAL PRURITUS DURING PREGNANCY.

BYT. CURTIS SMITH, M.D.,
Middleport, O.

Mrs. D. æt 28. A blonde, of nervous temperament, generally in good health, called my attention, February 26, 1876, to an intolerable itching, which she stated had been of extreme annoyance to her for the six weeks just past. I learned from her that she considered herself, at that time, in the ninth month of gestation, that this was her fourth pregnancy; had once "miscarried" during the sixth month, and before the pruritus made its appearance. But, with the two pregnancies preceding this one that had gone to full term, she had been afflicted the same as at the time she called on me in this pregnancy. She stated that her former physician (she then lived in Kentucky) had advised her to use no medicines other than spice-wood tea, under the idea that other and stronger remedies would likely prove injurious in her delicate condition. She further stated that her former attacks of the pruritus were of the same nature precisely as this one, and began in each instance about eight weeks before the time for confinement. She further stated that her mother was once afflicted in a similar manner, and that was when Mrs. D. herself was a fœtus in utero, being so informed by her mother; but none of her (Mrs. D.'s) sisters had been thus afflicted while pregnant. She is not a rheumatic, having never been afflicted with that painful disease, and the only instance of it in her family connection was in her maternal grandmother, who ultimately died from the disease—rheumatism. There is no history of scrofula, syphilis, or any kind of skin disease in this case or her relatives, that she can remember, and the two older children are healthy, having never had any form of skin disease. I found on inspection that there was no evidence whatever of any eruption, and the marks on the skin are all those caused by the nails in scratching. Repeated inspections by myself and by the lady—who is very intelligent—failed to disclose the least abnormal

appearance in any part of the integument, until she would scratch the same severely enough to cause the oozing of blood, which, when dry, would leave long dark lines of dried blood on the skin. The pruritus was most severe on the palms and soles, forearms and legs, but left no part unaffected, except the head. It was most severe in the evening, and on going to bed, she was compelled to use very little covering, and to keep away from the fire, as when she became warm, the itching became extreme and agonizing. The integumentary surface was not normally sensitive, as she could take up implements in use about the stove, or fire, that would crisp the skin without observing the least painful sensation from it. Other parts of the integument than that of the palms were also hyperæsthetic, but less so.

Her prejudice against the use of remedies prevented any effectual treatment. Externally a solution of chloral hydrate was used, but without any marked benefit, and she continued in this distressing condition until March 15, 1876, when I delivered her of a small and feeble male infant, which, however, now flourishes finely. For two months prior to delivery, the digestive organs failed to perform their functions normally, as she was troubled with much pain in the stomach, flatulence, acidity, etc., but not more than we see in thousands of cases where no skin affection exists. As soon as she was delivered, relief from the pruritus was apparent, and by the third day no further itching was complained of. Such has been her history in brief through three separate periods of gestation, showing an undoubted relation between the skin disease and pregnancy. Dr. H. Y. Evans, of Phila. (see *Amer. Jour. Med. Sci.*, Jan., 1875, p. 139,) relates a very similar case. Dr. L. Duncan Bulkley, under the head of "*Herpes Gestationis*," relates (*Amer. Jour. Obstet.*, etc., Feb., 1874, p. 580) a very interesting case of a similar trouble in a pregnant woman, but in his case there was a distinct vesicular eruption. He has also, in the same very interesting article, related in brief the history of eight other cases previously put on record, and which had been variously named by the different authors, according to the characteristic appearance of the eruption; but the histories of all of them present more or less similarity. The anæsthetic condition of the skin in my case is not mentioned by either of

those reported by Dr. Bulkley, or the one related by Dr. Evans. The points of special interest in this case are, the one last above named, and the fact that Mrs. D.'s mother was similarly afflicted while carrying Mrs. D. in utero, but not with any other of her children, nor did any of Mrs. D.'s sisters become so affected in any of their pregnancies. Mrs. D. had a good getting up, and at this time enjoys very excellent health. Her digestive trouble disappeared as soon as confinement was accomplished. May not this symptomatic disease be a secondary result of the gastric disturbance caused by the reflex influence of the uterine contractions over the digestive process? Women are sometimes similarly afflicted when not pregnant, but when suffering from uterine disease. Such cases, however, do not occasion such extreme suffering as those occurring during gestation.

CASE OF RELAXATION OF THE SACRO-ILIAC SYNCHONDROSIS
DURING GESTATION.

BY

J. HENRY FRUITNIGHT, A.M., M.D.]

New York.

THE patient, Mrs. H. M——y (who was also examined by Dr. Charles A. Leale of this city), æt. twenty-six years, is a woman of medium stature and is quite broad across the hips.

After she had entered upon the seventh month of her first pregnancy she was scarcely able to walk. She experienced great pain in her right hip and thigh, as also in the right lumbar region. When attempting to stand or walk, her body would be sustained on the right leg, which corresponded with the painful side. Her body was also inclined in the direction of the affected side. She could stand more easily upon one leg than she could upon both legs. When sitting she arose with difficulty; she also had great trouble in lifting any object from the ground. It was nearly impossible for her to ascend an elevation. In raising her right foot pain was produced. When she lay in a prone position she suffered pain, which also

was the case when her right thigh was flexed; hence, her decubitus was diagonal, and with thighs extended.

On a physical examination it was discovered that there was present an abnormal mobility in the region of the right sacro-iliac joint, for the component parts of the articulation could be felt gliding (to and fro) under the hand when the limb was manipulated. During her gestation she was subject to much gastric irritability, as also to repeated and habitual attacks of syncope.

From the foregoing clinical history it was concluded that the patient presented a relaxation of the right sacro-iliac synchondrosis.

On April 12th, 1875, she was delivered of a healthy female child, gestation having continued its normal length of time. Though her pains were good and strong, the progress made was but slow, the labor being a tedious one. During the period of confinement to bed nothing worthy of note occurred.

On April 25th, this being the thirteenth day after delivery, she arose from her bed for the first time since the birth of her child. She walked with considerable difficulty and with a wavering or tottering gait; it was better, however, than it had been just prior to her delivery.

May 2d.—She walks quite well and with but little difficulty, though in a somewhat toddling manner. She still complains of an inability to lift articles up from the floor.

May 23d.—She walks as well as ever she did; in fact, recovery may be considered as established.

The treatment consisted of a very tightly applied bandage or hip binder, and of exercise duly and gradually proportioned to her progressively increasing strength and improving locomotion. The bandage or binder was discarded on June 21st. She has been seen quite frequently since; the bones are firm and give no intimation of previous trouble whatever.

It may not be out of place to consider in a few general terms what influence this condition of relaxed pelvic articulations may have upon parturition.

1. In pelves of normal dimensions it can hardly affect parturition in any manner.

2. In contracted pelves this relaxation might possibly be of some advantage, since most probably the pelvic capacity would

be thereby somewhat enlarged, and hence, to a certain extent, labor might be facilitated.

In these cases, therefore, one would think *a priori* that this relaxation, if not of constant, would be at least of frequent occurrence. Scanzoni believes that, whilst the uterus is developing and pushing upwards against the abdominal parietes, some counter-pressure must be exerted downwards upon the parts below, and hence the articulation will be forced asunder in consequence of this counter-action of a narrow pelvis.

The mass of evidence and experience, however, is opposed to this theory; for just as soon as the progress of its development demands it, the uterus *rises out* of the pelvis, this period being of necessity earlier in a narrow than in a broad pelvis. The uterus being thus lifted out of the pelvis, its pressure downwards must be diminished, and hence the pelvic articulations remain intact, very rarely becoming relaxed in these very cases where the most benefit would accrue from its occurrence.

3. In capacious pelves relaxation of the symphyses would be the cause of more harm than good, and it unfortunately happens that it is just in these very cases of broad pelves with capacious straits that it most frequently occurs. Owing to the instability resulting from the relaxed articulation, the uterine contractions will fail of fully accomplishing what they should, and in all probability would accomplish if the joints were firmer. Hence, in those cases labor will be prolonged and tedious, which seemed to be the *rationale* of the case herewith reported.

TRANSACTIONS OF THE NEW YORK OBSTETRICAL SOCIETY.

Reported by PAUL F. MUNDÉ, M.D., Secretary.

Stated Meeting, April 18th, 1876.

*The First Vice-President, DR. A. JACOBI, in the Chair.**

DR. A. J. C. SKENE showed the specimen from, and related the history of a

CASE OF EXFOLIATION OF THE MUCOUS MEMBRANE OF THE UTERUS, AS THE RESULT OF INTERRUPTED GESTATION.

"A lady 33 years of age: married 15 years and has had 5 children, the youngest one being now $2\frac{1}{2}$ years of age. Twelve years ago she had chills and fever, and now has phthisis pulmonalis in the first stage. She has been losing flesh and strength for a year or more. Since the birth of her last child menstruation has been normal, with the exception of slight pains which attended it.

She menstruated last on the 18th of November, 1875. With the exception of occasional nausea, there was no material change in her condition until Jan. 10th, 1876, when she was seized during the night with severe uterine or pelvic pain, which soon subsided. On the 12th she was again seized with violent pain, which required the free use of opium to relieve.

A few days afterwards, on examination, the uterus was found to be enlarged. The cervix was soft, granular, and studded with small cysts containing cheesy matter. There was tenderness over the lower portion of the abdomen. The breasts showed no signs of pregnancy. The diagnosis made then was threatened abortion and pelvic peritonitis. The patient was confined to her bed until March 29th, and during that time her pulse and temperature ranged about one hundred. She had less or more pelvic pain, slight nausea, and occasional vomiting, loss of appetite, etc. Any attempt at walking or standing

caused violent attacks of uterine or pelvic pain, which greatly prostrated her. On the 20th of February, I saw the patient in consultation with Dr. Jewett, and my examination confirmed the Doctor's views as to the enlarged diseased uterus and evidence of pelvic peritonitis.

March 29th.—Had a discharge of bloody water, which continued for a few hours, and was followed by fresh hemorrhage. On the morning of the 30th a mass was expelled, which the doctor at first supposed was an ovum.

It was found to be a sac composed of one membrane smooth on its outer surface; the under like the free surface of a mucous membrane. In shape it looked like a cast of the uterine cavity. The inner surface bearing some resemblance to the chorion, the doctor thought that it might have been inverted during its expulsion; but the small size of the opening in the sac raised doubt as to that.

No trace of an embryo was found.

DRS. RAYMOND and SEGUR made a hasty examination of it with the microscope, and found it composed of small spindle and round cells, with very little interstitial substance. This led them to suppose that it might be sarcoma.

After this specimen was expelled there was a flow for several days, resembling menstruation. The patient improved in health, and is now able to move about the house. The products of the pelvic peritonitis still exist.

The chief questions raised by this specimen are: Is it a product of gestation, and if so, is it not an unusual one, even as a result of embryonic disease? Or is it altogether pathological, the product of some uterine disease?

Without having any positive views of my own on the subject, I will (in the hope of raising discussion) take the ground that it is the mucous membrane of the uterus, changed in structure, which was exfoliated and expelled in mass.

If there is any ground for that view, the specimen is one of interest, and may throw some light on the question of the behavior of the mucous membrane of the uterus, which is now, as it always has been, a marked subject for discussion."

The specimen was referred to Dr. M. D. MANN, for examination, who subsequently made the following report:

"In examining the specimen presented by Dr. Skene, an answer to three questions is to be sought: 1. Is this the lining membrane of the uterus? 2. If so, is it a decidua menstrualis or dysmenorrhoeal membrane; or, 3. Is it the decidua of pregnancy? Other questions of minor interest are: which was the free and which the attached surface, and what is the condition of the tissues composing the membrane?"

The specimen as it came into my hands consisted of several pieces of a soft spongy membrane, torn in several places, and with a round hole in the thinnest portion of the larger fragment about the size of a nickel cent. On holding up the membrane by the sides the hole was seen to be at the bottom of a pouch.

The membrane on one side was smooth but somewhat convoluted in appearance. On the other side it was ragged, rough, and presented a sort of villous formation.

It was very uneven in thickness, reaching in some spots a thickness of .4 inch.

Having previously frozen a portion, I made a thin transverse section and found the following appearances:

The upper portion or part near the smooth surface consisted of a tissue made up of large cells, with large nuclei resembling in some respects epithelial cells. Here and there in this tissue open spaces or alveoli were seen, these spaces being quite empty, except here and there a few cylindrical epithelial cells. The lower half on the part nearest the ragged surface was made up of a meshwork, the trabeculæ consisting of connective tissue with large cells. There were some traces of epithelial cells. The whole tissue was very fatty, particularly near the free surface.

This membrane corresponds, then, in all particulars with the description given by Engelmann (see *Amer. Jour. Obst.* for May, '75) of the decidua of pregnancy. The smooth surface being the free and the rough side the attached or uterine surface, the opinion advanced that the membrane is the result of pregnancy, and from the history, an extra-uterine pregnancy is upheld.

It is the decidua, as it resembles nothing else in the slightest degree, and corresponds exactly with the appearances which that membrane ordinarily presents. It is not a menstrual decidua, because it is too large, too thick, and the cells composing it are too large. Again, the glands or follicles, instead of being nearly straight or only slightly twisted and small, are very much twisted and enlarged, with only a trace of epithelium.

It must be, then, a decidua of pregnancy; and we come to that diagnosis not only by exclusion, but because its microscopical character corresponds very nearly to what we find in the third or fourth month of utero-gestation, and more exactly to that found in cases of extra-uterine pregnancy.

The fundi of the glands are not found, so they must have remained in utero. When found the membrane formed a sac open at one point, and presenting its rough or villous surface on the inside.

As the rough surface is evidently the portion which was torn

in the detachment of the membrane, its being found on the inside of the sac when extruded can only be explained by supposing that the membrane separated at the fundus first, and that the portion near the cervix remaining attached, the rest was expelled by uterine action—the part from the fundus coming first, passing through the still attached lower or cervical portion, thus inverting the membrane. Since the meeting I have noticed a similar case reported in the *Obst. Jour. of G. B. & I.* for Feb. '76, p. 774."

DR. SKENE also related a

CASE OF CONGENITAL CHLOROSIS OR ANÆMATOSIS.

"DR. STUART called me to see a tedious case of labor, which terminated a few minutes before my arrival. While the Doctor was caring for the mother I examined the child, a male, the seventh one of the family. It was small, but apparently healthy and well formed. The skin was the usual pinkish red color of a new-born child.

I learned from the Doctor subsequently, that the child did well during the first week after its birth, except that it had a slight attack of jaundice. When it was two weeks old I was called to see it, because, as the mother stated, it had become so pale and slept so much. I found it exceedingly anæmic—so much so that I suspected that it had had hemorrhage; but there was no evidence of its having had any bleeding from the umbilical circle, bowels, or kidneys. The child nursed quite well, and beyond slight indigestion, indicated by the character of the discharges from the bowels, I could find nothing to account for its bloodless condition.

The pulse was small and very rapid, and the action of the heart was feeble. Whether this was due to the small size of the heart and arteries or to the imperfect quality of the blood, or both, I was unable to say.

The mother told me that her sixth child—a boy—was affected in the same way; and the Doctor, who saw him when he was over two weeks old, stated that he was the most anæmic infant that he had ever seen. That child is now three years old, is quite well, and but very slightly chlorotic. The fifth child, also a boy, was said to be anæmic or chlorotic, but not so much so as the other two. He died of chronic pneumonia at the age of two years.

The parents were healthy, and apparently free from any diathesis.

I ordered the child to have pepsine after nursing, and gave the mother phosphates and iron, in the hope that she would transmit them to the child through the milk.

Fourteen days afterwards I saw the child again, and found that it was doing tolerably well, but was still very anæmic.

This case is related in the hope of raising discussion which may settle the question whether it is congenital chlorosis or anæmiosis."

DR. JACOBI said that he would be inclined to consider this case one of congenital chlorosis. In this disease the blood-vessels are smaller than natural, especially the veins; therefore venous obstructions are common. The heart is found to be unusually small, and probably was so in Dr. Skene's case. He has certainly diagnosed this condition in five or six cases. Lately he was called to a child eight months of age, of strong, fleshy habit, which had been perfectly well until within a few weeks, since which time it had had some fifty or sixty slight convulsions, distinguished by moderate twitchings of the face and extremities. There had been no rise of temperature, except during the last attacks. There was no apparent cause for the convulsions. Finally, he discovered, on percussion, that the heart was smaller than it should be, and that the heart-shock was unusually weak. There were no special signs of rachitis. The superficial veins were generally very large, and Dr. Jacobi attributed the high development of the adipose and connective tissue to a possibly unduly active venous circulation. The treatment adopted was cold to the head, hot foot-baths, quinine and ergot, all with the object of contracting the venous portion of the circulatory apparatus. Since then the child had had but a few convulsions.

Stated Meeting, May 2d, 1876.

The President, DR. T. G. THOMAS, in the Chair.

DR. J. FOSTER JENKINS exhibited a

UNIQUE SPECIMEN OF CONGENITAL ULCERATION OF THE UMBILICAL CORD.

He attended the lady in premature labor from albuminuria at the end of the eighth month. On examination, he found the membranes intact. When the membranes burst the liquor amnii discharged was seen to be almost pure blood. The child was dead and very pallid, apparently exsanguinated. He estimated that there was at least a quart of blood in the amniotic cavity. On looking for the origin of the hemorrhage, a small perforation was found in the umbilical cord about half an inch from its abdominal insertion; the cord was much narrowed at this point, but not twisted.

DR. A. JACOBI was appointed by the President a committee to examine and report on the specimen, and subsequently presented the following report:

"The specimen transmitted to your committee consists of a part of the umbilical cord, one and a half inches in length. The circumference is normal. Wharton's jelly is absent in one spot of a diameter of about one-sixth of an inch. In the depth of the defective mass is an opening into the umbilical vein. The three laminae of the vessels are very distinct on the section near this spot. On the opposite end they are very indistinct, inasmuch as the pressure of the ligature has compressed the whole mass. Thus I have not succeeded in re-opening the vessels from end to end. In the part surrounding the abnormal opening into the vein I find unstriped muscular fibre in fatty degeneration, distinctly and unmistakably. To what extent this anomaly was complicated with fatty degeneration of the placenta of the foetus is impossible to say. Cases of fatty degeneration of both are no longer exceptional, since Hecker and Buhl's discovery twenty years ago. As neither placenta nor foetus are any longer accessible, the questions cannot be answered, but there appears to be no doubt but that the fatal hemorrhage into the amniotic sac resulted from rupture of the umbilical vein depending upon fatty degeneration and consecutive fragility.

In G. Chantreuil's recent book, "*Des dispositions du cordon qui peuvent troubler la marche régulière de la grossesse et de l'accouchement*," Paris, 1875, I find the only case of fatty degeneration, which, moreover, he quotes from Simpson. The foetus was expelled in the fifth month. The placenta was healthy, as also the body of the foetus.

Thus, Dr. Jenkins' case is almost unique.

Other changes of the umbilical blood-vessels have been noticed. A varicosity of the vein, with rupture six inches from the placenta insertion, and death of the foetus, was reported by Dr. Pluskal in the *Oesterr. Med. Woch.* of 1844. Deneux (*Journ. Gén. de Méd. Chir. et de Pharm. Franc. et Étrangère*, Vol. 71, May, 1820) published a case of rupture of the umbilical vein, which was obstructed by a fibrinous clot.

Logan (*Dublin Jour.*, Vol. I., p. 248) reports a case of extreme fragility of the cord, which was short, thick, and of almost cartilaginous consistency. He suspects that such a condition might prove fatal.

Complete atresia by torsion and amniotic bands have been reported.

Atheromatous condition of the arteries has been noticed repeatedly.

Moreau quotes a case of induration, almost scleromatous, of the umbilical cord, with compression of the blood-vessels.

Thrombosis and phlebitis in the veins and aneurism in the arteries are mentioned by Chantreuil, from whom I have collected the above notes.

Dr. G. H. Sabine, of Vermont, has studied the transverse sections of umbilical cords in the Anatomical Institute of Strasburg. (*Archiv für Gynäkol.*, IX., 1876. p. 311.) He found almost constantly a small space running through the whole length of the cord, besides the three vascular laminae. It is mostly eccentric near the surface, and sometimes distinctly patent, and covered with epithelium in several layers, sometimes, however, in the form of an almost obliterated epithelial canal, which is difficult to find. Its epithelium is altogether like that of the urachus and bladder. From this fact, and from the further fact that in dogs and the ruminantia, the allantois remains open in the umbilical cord until the maturity of the foetus, he concludes that the body he describes is a remnant of the allantois. He did not succeed in finding a remnant of the ductus vitello-intestinalis.

These investigations were made in 1874, and were mentioned publicly by Prof. Waldeyer in the subsequent winter. Since that time Ahlfeld asserts positively to have found a ductus omphalo-mesentericus in the umbilical cord. Thus, it is possible that both allantois and ductus omphalo-mesentericus remain in some more or less distinct shape in the umbilical cord—the more so as the accounts of the two investigators relating to the position of the canals do not point to the same spot.

However, no matter how the discrepancy of opinions will terminate, it appears that the examination of the vessels is not rendered more difficult by the multiplicity of parts in the neighborhood. On the contrary, as none of these numerous parts carry fat normally, the presence of fat can be determined upon even by an observer of minor capacity.

Besides, the umbilical vein is one of those which carry an unusual amount of muscular tissue, which may undergo fatty degeneration. Like the canal, popliteal and mesenteric veins, the umbilical vein carries both an external and internal longitudinal and an internal annular layer of muscular fibre. Their changes can be found the easier the more bulky their mass."

DR. JAMES S. GREEN read the following

HISTORY OF A FATAL CASE OF ICTERUS GRAVIDARUM, occurring in a lady advanced to the seventh month of pregnancy.

"I was called to meet Dr. Crane, of Elizabeth, N. J., in consultation, April 15th, 1876, to see Mrs. H., aged 31 years, in the seventh month of pregnancy with her third child.

She had been married nine years, and had had two natural labors, not having suffered through her previous pregnancies from any remarkable disturbance; her principal annoyance having been a very marked ephelis of the face, which also existed in a marked degree in this, her last pregnancy.

She had been the subject of repeated attacks of malarial fever.

At the time I first saw her, April 15th, she was suffering with severe neuralgic pains in the lower limbs, in the pelvis, and occasionally in the back, recurring with violence every second night.

She had been placed fully under the influence of quinia by Dr. Crane, and the severity of her pain had been relieved by anodyne suppositories.

At this time there was no marked jaundice; the conjunctivæ were natural in color, as also the skin, except upon the cheeks and forehead, where the ephelis was remarkable.

The administration of quinine was continued, and a thorough catharsis advised.

I saw her again three days later (April 18). She complained then of intense headache, loss of appetite, great prostration, restlessness, nausea, with vomiting of brown, bilious matter, and of pain in the abdomen. Pulse 110, temp. 99, resp. 20. She was relieved from the pain in the limbs, and was suffering with pain, principally in the pelvis.

Upon examining her per vaginam, I found the rectum and sigmoid flexure loaded with feces, and upon the right side of the neck of the womb a circumscribed spot very tender to the touch; the os uteri was soft and patulous; there were no signs of labor having commenced. It was agreed to give her one compound cathartic pill, U. S. P. (freshly made), every four hours until free catharsis was produced. Quinine, milk diet, and champagne continued.

The pain and jactitation to be treated with hypodermic injections of morphia.

April 19.—Saw patient again with Dr. Crane. Bowels have been thoroughly evacuated, and large amount of fecal matter voided. Has slept during the night without use of anodyne. Pulse 115, temp. 98 $\frac{3}{4}$ °. Intellect unclouded. Complains of pain in both hypochondria, nausea, with vomiting of dark matter; conjunctivæ yellow, and skin becoming jaundiced; occasional pain in the pelvis. Urine having been retained, was

removed by catheter; of a dark color, but not scanty. Inclination to drowsiness, but is easily aroused.

Calomel, gr. x. given, and a lotion of nitro-muriatic acid applied over the right hypochondria.

To decide the propriety of inducing labor as a relief for her jaundiced condition, Dr. T. G. Thomas was called in consultation, but could not see the patient till next day.

April 20.—About twelve o'clock last night she became partially comatose, and was aroused with great difficulty, and then lapsed immediately into profound sleep with stertorous breathing.

About 10 A.M. was seen by Dr. Thomas. Pulse 110, temp. 99. Urine tested; did not contain trace of albumen.

Dr. Thomas pronounced the case one of *cholæmic* poisoning, and advised the induction of labor.

It was agreed to produce delivery of the fœtus, and Dr. Crane and myself would have taken the necessary steps at 3 P.M., but at 12 o'clock, noon, labor commenced spontaneously, and Mrs. H. was delivered safely, at 1.30 P.M., of a dead seven months' fœtus.

There was no hemorrhage. The fœtus appeared to have been dead about two days, and was not at all jaundiced.

6 P.M.—The patient's symptoms are not improved by her delivery. Pulse 105, temp. 99, respiration 15. Tendency to profound coma increasing; vomiting of dark matter continues. Gave bismuth subnitr. gr. x. every three hours. Milk, quinine, and brandy.

April 21.—Coma more profound. Pulse 110, temp. 98, respiration 15. Jaundice increased. Urine retained and removed by catheter; very dark.

In the afternoon she is perfectly unconscious, very restless, with sighing and spasmodic breathing. Lochia have ceased, but were induced to return by hot vaginal injections.

April 22.—Pulse 110, temp. 99, respiration 12. Has been unconscious and restless all night, crying out, and tossing about with considerable violence. Have used chloroform by inhalation to induce quiet, with good effect. Will not take any nourishment; still vomits brown matter; bowels moved.

Dr. Thomas saw patient at 8 o'clock P.M. Pulse 110, temp. 99, respiration 8, marked in the interval with three or four spasms of the glottis.

April 23.—Patient died at 7.30 A.M. Pulse and temperature remaining the same until within a short time of her death; in an effort to throw off some of the contents of the stomach, they passed into the trachea, and death ensued suddenly.

No post-mortem examination could be obtained, and in its

absence we are led to infer, from the great rapidity with which this case came to its fatal issue and its close repetition of the symptoms of a few cases of the same kind in which autopsies were made, that it was one of those rare cases of "*Acute Atrophy of the Liver*," occurring as it does most frequently in pregnant women, between the third and seventh months of utero-gestation, and between the ages of seventeen and thirty years.

In this case the disease ran its course in less than ten days and notwithstanding the occurrence of labor, steadily progressed without amelioration of its symptoms.

This case bears a striking resemblance in its history to the case reported by Frerichs of Berlin, in his "*Clinical Treatise on Diseases of the Liver*," an abstract of which is given in Tanner "*On Signs and Diseases of Pregnancy*," p. 398.

In Frerichs' case the disease ran a more rapid course of six days, which may be accounted for by the profuse uterine hemorrhage which followed spontaneous delivery of the fœtus. With the exception of the duration of the disease and the occurrence of post-partum hemorrhage, the cases are almost identical. Both women at the seventh month of pregnancy were suddenly attacked with loss of appetite, fugitive pains, intense headache, constipation, low spirits, restlessness, nausea, followed by vomiting of dark bilious matter. Immediately after these symptoms came jaundice, pain in the hypochondria, retained urine of dark color without albumen. Clouded intellect, drowsiness, greater restlessness, spontaneous delivery of a dead fœtus which was not jaundiced. No relief of the symptoms from delivery, increasing unconsciousness, accompanied with incessant jactitation and outcry, ending in profound coma and death.

In both cases the temperature was about normal during the whole course of the disease, and the pulse ranged between 80 and 115 until a few moments before death, and the respiration became less frequent, as the poisoning of the nerve-centres became more profound. The autopsy of Dr. Frerich's case developed "nothing noteworthy about the brain, lungs, or heart. The stomach was free from ulceration; it contained a matter like coffee-grounds. The large intestine contained scybala faintly tinged with bile. The liver lay collapsed against the posterior wall of the abdominal cavity; it was dry and soft, while its capsule was puckered and opaque. The dimensions of the gland were diminished in every direction, particularly in thickness. The gall-bladder contained a small quantity of gray mucus. The tissue of the liver felt flabby and dry; the ramifications of the portal vein surrounding the lobules were dis-

tended, whilst the centre of the lobules presented a citron-yellow color; here and there were ecchymoses. The secreting cells were disintegrated, numerous drops of oil and brownish-yellow molecules being found in their place; only a few isolated cells loaded with oil being detected in the rounded border of the right lobe. The weight of the liver was 1.807 lbs. avoirdupois, and that of the entire body 123.898 lbs., making the ratio of the former to the latter as 1 to 68.5. In healthy females of the same age and weight, the weight of the liver is about 4.409 lbs., and the ratio is as 1 to 28. Thus the organ had lost in six days 2.601 lbs. in weight. It was also considerably diminished in size."—Tanner, "Signs and Diseases of Pregnancy," p. 309.

A post-mortem examination of Mrs. H. would in all probability have presented a similar condition.

"Of 31 cases of acute atrophy of the liver, 22 were females, and of these 22 cases one-half were attacked during pregnancy. The latter is a striking fact, yet 33,000 cases of pregnancy analyzed by Spaeth afford only two examples of this disease."—Flint's Practice of Medicine, p. 535.

These cases should not be classed with those where the jaundice and cholæmia are due to direct mechanical obstruction of the bile-ducts from pressure or gall-stones."

DR. THOMAS said that when he saw the case in consultation he found it impossible to examine the liver on account of the restless condition of the patient, and therefore could obtain no information as to its size. He noticed a very peculiar manner of respiration, the number of respirations being about 16 to the minute, but only 8 of these 16 were deep and full, the other 8 being ineffectual attempts; 2 or 3 attempts were always followed by a deep inspiration.

DR. JENKINS mentioned a case, which he had seen in 1874, where a woman of 30 years, after delivery, showed symptoms similar to those in Dr. Green's case, high temperature, intense jaundice, delirium. Death ensued after 3-4 days. There was no autopsy, and the urine was not examined, but his diagnosis was acute yellow atrophy of the liver.

DR. CHAMBERLAIN referred to a case reported by him in the N. Y. Med. Record, p. 265, Vol. VI., 1871, in which an autopsy was obtained. The disease commenced with diarrhœa and vomiting 24 hours before labor began, and at that time a disease of the liver did not occur to him. After delivery there was mania and sopor, and death ensued on the second day post-partum. The child was born living, and is now alive. A remarkable circumstance is that the infant suffered from cholæmia, and was in a semi-narcotized condition for six weeks,

with persistent constipation, and a sulphurous odor from the skin.

ENDEMIC NON-PUERPERAL PELVIC CELLULITIS.

DR. M. D. MANN remarked: "During the last two months I have seen a very large number—that is, proportionately large—of cases of pelvic inflammation in my class for women at the N. Y. Dispensary. Ten cases presented themselves between February 25th and April 5th. As many of these had been suffering for some time before applying for relief, by tracing back the histories to the origin of the attacks, I find that they all originated between February 1st and March 15th, and that several originated in the week beginning February 11th.

They were all but one cases of pelvic cellulitis, presenting large, hard, localized exudations in the immediate neighborhood of the uterus.

The principal point of interest in these cases is the question of their cause.

In only one could any definite cause be determined, and that one was evidently pelvic peritonitis, the history and the results of the physical examination being quite different from the others. The common history was that the patient, being in average health, or suffering at the most from some slight uterine trouble, was suddenly taken with severe pain referred to the neighborhood of the uterus.

This pain growing continuously worse, they were obliged to apply for relief. There seemed to be no constant relation between the commencement of the attack and the menstrual period.

In default of a better explanation, we are led to think of some atmospheric, telluric or endemic influence which might produce the disease. Five of the women resided very near to each other in two neighboring streets. Others came from distant parts of the town. That great vicissitudes in the weather had any causative influence is hardly likely, as the month of February was exceptionally mild, if I recollect rightly; at any rate, not so severe as in other years when very much fewer cases of pelvic inflammation presented themselves.

My friend, Dr. Dessau, district physician to the dispensary, where patients are drawn from the same district as a majority of mine, tells me that he has met with more cases during the months of February and March this year than in the past two years together. I call attention to the subject as an interesting fact, and in order to elicit a discussion which may tend to throw some light on the etiology."

DR. PERRY said that he had seen two cases lately in which

no other cause than antecedent nervous depression could be ascertained.

DR. THOMAS thought that the inclement weather usually occurring in February and March should be looked upon as the cause of these frequent cases of pelvic cellulitis and other inflammatory affections. Such, at least, has been his experience at the Woman's Hospital, where he has been exceedingly careful not to operate much during these months. Especially has he avoided performing ovariectomy during these two months, except when unpostponable. He is disposed to attribute in a measure the greater success of ovariectomy in England over our own country to the more equable temperature and much less severe climate of that country. In cases of chronic pelvic cellulitis or peritonitis recurring at the menstrual epoch, he has found as much benefit by sending the patient to a warmer climate as is experienced by people in the first stages of pulmonary disease.

DR. PERRY said that by simply sending the patient out of the city, to the neighboring sea-shore or country, he had obtained excellent results.

In answer to a question by Dr. Hanks whether pelvic cellulitis is more common on the left side, and if so, why? DR. PEASLEE said that in his experience it was so, the cause being perhaps the same as the more frequent congestion of the left ovary, viz. (as in the left testicle), the return of venous blood by the spermatic or ovarian vein proper, and not direct into the ascending vena cava by a short vein, as in the right ovary and testicle, whereby a venous stagnation and hyperæmia might be more readily induced, and a left-side cellulitis brought about.

DR. CHAMBERLAIN said that this fact should be more generalized. He had noticed that the whole left side in women is more liable to disease (neuralgia, &c.) than the right.

DR. PEASLEE said that this is the case in both sexes—for very few people are ambidexter—and was probably owing to the inferior development of the right cerebral hemisphere.

DR. JACOBI said that the reason for the superior development of the left hemisphere is the better and more direct supply of blood which it receives by the anatomical arrangement of the large vessels springing from the left side of the aortic arch, which increased arterialization renders it more fit to innervate the right half of the body than is the case with the right cerebral hemisphere. The left side of the body being less perfectly innervated, is therefore also more liable to pathological changes.

DR. E. R. PEASLEE related a

CASE OF UTERUS DIDELPHYS SEPTUS ET VAGINA SEPTA,

which he had lately observed in a married woman who was sterile after six years of marriage, for which reason she consulted him. There were two vaginae, with perfectly developed septum, the right vagina being larger than the left, owing, doubtless to greater use. There were also two perfect cervixes and ora, the right os being slightly lower than the left. Each half of the uterus was straight and normal; the septum ran through up to the fundus uteri, as is always the case when the vagina is double. The uterus was not bicornis. The vaginal septum was removed by the galvano-caustic loop, and scarcely a trace of it could be felt when the wound was healed. With her two uteri and perfect ora it would seem as though there could be no obstacle to impregnation.

Stated Meeting, May 16th, 1876.

The President, DR. T. G. THOMAS, in the Chair.

DR. MATTHEW D. MANN was appointed Pathologist to the Society.

DR. THOMAS ADDIS EMMET presented the specimen from and related the history of a

CASE OF MYO-ADENO-CYSTOMA OF THE OVARY.

The patient, a lady 40 years of age, first noticed the tumor in 1869, and consulted him in 1870. For the previous year she had been menstruating every three weeks, which fact partly induced him to think the growth a fibrous tumor of the uterus. He therefore advised non-interference. When he saw her again last fall he found fluid in the abdominal cavity, which fluid accumulated in an umbilical hernia, distending it to the size of a fist, and annoying the patient very much. He still entertained his first diagnosis of uterine fibroid. She was seen at this time by Dr. Peaslee, who could come to no definite conclusion as to whether the tumor was uterine or ovarian. The abdomen continued enlarging and the patient was tapped last March, rallying slowly from the operation. Early in April she was tapped again, and the third time at the end of April. The diagnosis as to whether it was an ovarian or a uterine tumor was still uncertain, but a distinct pedicle could be detected apparently connecting the growth with the uterus.

On the preceding Saturday the operation of ovariectomy was performed, the patient being in a very debilitated condition;

twenty-eight pints of fluid were removed—only about half as much as at the previous tapplings. A tumor of about the size of an adult head was found attached by a broad pedicle to the fundus uteri and by an adhesion to the crest of the ilium; the tumor was evidently one of the left ovary and multilocular in its character, the numerous small cysts being filled with myxomatous matter. The umbilical hernia was removed with the abdominal incision. The patient never rallied from the operation, and died very rapidly twenty-six hours afterwards, as is frequently the case in malignant tumors.

DR. PEASLEE said that he had seen the case twice—the second time last fall. She then had considerable ascites; the uterine cavity was about normal. He was unable to differentiate between uterine fibroid and ovarian tumor. But he advised the operation and agreed with Dr. Emmet that, if the patient could have decided upon the operative removal of the tumor sooner, while she was still in fair health, she might readily have been saved. He also thought that if she had been seen by Dr. Emmet immediately after one of the tapplings, which were made by other physicians, a true diagnosis might have been reached. The small size of the tumors complicated with ascites is no sign of their malignance, as is shown in this case, where the tumor was growing over seven years.

DR. M. D. MANN (at a subsequent meeting) presented the following microscopical report on the tumor:

“The portion which I removed for examination—a piece as large as two fists, was made up of a dense stroma enclosing numerous follicles or alveoli, ranging in size from that of a head of a pin to that of a goose-egg.

These loculi were filled with a colloid-like substance, which varied in color and consistency in the different varieties, in some being clear like white of egg, and in others yellowish like pus.

Examined under the microscope, the character of the contents was found to differ somewhat.

In some of the cysts there were large, rounded, flat epithelial cells, which had undergone either fatty or colloid degeneration.

Those which were fatty resembled very much colostrum corpuscles, being filled with fat globules and varying much in size.

The colloid cells presented a very delicate outline and a perfectly homogeneous, highly refractive body without nucleus. They also varied much in size.

In other cysts the fluid showed large numbers of cylindrical epithelial cells, very long and distinct, with large distinct nuclei. They were either alone or joined together in large clusters, or surrounded little villous prolongations.

Besides the epithelial cells there were a certain number of long spindle-shaped cells, narrow and with distinct oblong nuclei. They were undoubtedly smooth muscle-cells. They floated free with the other contents of the cavity.

By freezing a portion and making through sections, I was enabled to get the relations of the parts.

The epithelium of both kinds were then seen to line the walls of the alveoli, though in many cases it was detached.

By appropriate methods I was able also to demonstrate that this stroma was made up of fibrillary connective substance, containing in many places large bundles of smooth muscle fibres.

The diagnosis of adeno-cystoma, or, if we give it a name corresponding to its anatomical elements, of myo-adeno-cystoma, is therefore very plain. The presence of the muscle-fibres in the stroma is of very rare occurrence, and their existence floating free in the fluid contents of the cysts is of some considerable clinical importance. If these cells had been found in a portion of the fluid removed for diagnostic purposes, separated from the epithelial cells, as might readily have occurred, the diagnosis of fibro-cystic tumor of the uterus would undoubtedly have been made.

As regards the existence of the cells in a tumor of the ovary at all, this occurrence is not unique, they having been found by several observers, both in cystomata and in myo-fibromata originating in this organ.

Such an occurrence is, however, of extreme rarity; though from the great difficulty of recognizing them unless separated from the surrounding tissue elements, they may occur oftener than is supposed."

DR. E. R. PEASLEE related

A CASE OF GASTRO-HYSTEROTOMY,

which he had performed at the Woman's Hospital last February. He first saw the patient six years ago, and found the abdomen enlarged to about the size of the seventh month of utero-gestation by a uterine fibroid. The uterine cavity was about one-half inch longer than normal. About two years ago the girth of the abdomen was forty inches, respiration and appetite were interfered with, and the patient begged for an operation, which he refused until urgent symptoms should show themselves. The woman went to Roosevelt Hospital, and was tapped there, under the impression that the tumor was partly filled with fluid, but only a few ounces of fluid were obtained. A delusive semi-fluctuation was imparted to the examining fingers by the semi-gelatinous contents of the tumor, which was of the

variety called "geode," by Cruveilhier. Only after repeated consultations, and at the earnest solicitation of the patient, did Dr. Peaslee consent to operate. Her girth at that time was forty-five inches. She was in a bad condition for taking ether, and this fact, as well as the nervous shock and loss of blood, came very near proving fatal during the operation itself; but by means of some forty hypodermic injections of brandy she rallied, and was taken from the table in a fair condition. The tumor was of enormous size, a cysto-myxoma in composition, and attached to the fundus uteri by a slender pedicle, which was ligated and the main growth removed. The uterus was then found to contain some ten or twelve smaller fibroids, extending as far down as the cervix and enlarging the organ to the size of two fists. A double silk ligature was passed through the cervix, either half tied separately, and the uterus removed, together with the ovaries. For twenty-six hours the patient did well; then her temperature suddenly rose and she sank rapidly, and died six hours later. After death her temperature was 107° . At the autopsy no particular cause for the sudden death could be detected, except a moderate peritonitis and a slight intra-peritoneal exudation. The tumor weighed forty-five pounds.

DR. PEASLEE said that two points in the operation were open to criticism: 1. Why was the uterus removed, when leaving it in would have made the operation less dangerous? Because the patient had made him promise that the whole tumor should be removed; but principally because the uterine fibroids were so numerous that they would undoubtedly have sooner or later invalidated the benefit derived from the present operation. 2. Why were the healthy ovaries removed? Because their ablation scarcely increased the danger of the operation, and their presence would later be of no benefit, but only a source of annoyance to the patient. The rise of temperature shortly before death might have been caused by the nervous influence attending impending dissolution.

DR. NOEGGERATH said that the rise of temperature during and subsequent to the act of dying was explained by two theories: 1. Paralysis of the inhibitory nerves of the vascular system before death; and 2. A chemical process commenced in the system during life and continuing after death. These sudden cases of death with high temperature might be due to *septicémie foudroyante* (d'Espine). He had seen several cases of puerperal fever this winter, which very much resembled those described by that writer. No cause for the rise in temperature could be found until the second day, when the perineal wounds were found covered with foul diphtheritic deposit. The signs of the absorption

of the poison were thus present before the local lesion showed any pathological change.

Dr. T. G. THOMAS related the following

CASE OF EXTRA-UTERINE GESTATION—GASTROTOMY—RECOVERY.

Five weeks ago he was called to see a patient of Dr. James Hadden, who seven months previously had noticed an abdominal enlargement, accompanied by nausea and other symptoms of pregnancy, which she supposed to be her condition. No labor-pains, however, came on at the normal period, and all signs of pregnancy, except the enlarged abdomen ceased. Then Dr. Thomas saw her. He took the case to be one of ovarian cyst, notwithstanding the cervix uteri resembled that of a uterus in the third month of pregnancy. The uterus could be mapped out and appeared to be of the size corresponding to the third month. The pelvic roof was hard and firm, as in pelvic peritonitis. The woman and her husband both asserted that they had distinctly and frequently seen, and the former, that she had felt the movements of the fœtus, and persisted in this assertion, notwithstanding Dr. Thomas's opinion that there was no pregnancy. A positive diagnosis, however, was reserved after the tapping of the abdomen, at which two gallons of fluid were removed by the aspirator. This fluid was examined under the microscope, and declared to be ovarian fluid. During the aspiration the trocar was suddenly plugged up by fibrinous masses, which necessitated the removal of the tube, although all the fluid had not been removed from the abdomen. A body resembling a fœtus could now be quite plainly mapped out, and the diagnosis of abdominal pregnancy was therefore made.

A week ago the operation for the removal of the fœtus was performed. When the incision reached the peritoneum, that membrane was found to be very much thickened and to resemble the ordinary sac of an ovarian tumor, and could be so easily stripped from the muscular layer as to render the mistake of peeling it off, in the belief that it was the adherent ovarian sac, exceedingly easy. On opening the peritoneal cavity, a fluid resembling pea-soup and flaxseed tea mixed and containing large masses of flocculent fibrin, was poured forth. The examining hand readily discovered a large child in the abdominal cavity, and removed it by traction on the breech. It weighed six pounds fifteen ounces, and was a female. The umbilical cord ran to the left iliac fossa, where it was apparently inserted into the peritoneum, no placenta being discernible. If there had been one, Dr. Thomas would have left it intact to

be expelled by nature, preferring the risk of septicæmia by so doing, to the danger from hemorrhage likely to follow the forcible detachment of the placenta. The child was dead, and its death was evidently caused by a sharp constriction of the umbilical cord at about its middle by a long hair wound round it again and again. The peritoneal end of the cord was easily peeled off with the finger-nail, without hemorrhage. The drainage tube was introduced, the wound closed, and the patient put to bed. She has been doing perfectly well, her temperature on this day being 98.8° her pulse 107; whereas, before the operation, the pulse was 120, the temperature always 100° and higher. The abdominal cavity has not been washed out.

At the meeting, June 20th, 1876, five weeks and six days after the operation, Dr. Thomas reported the conclusion of this case. The patient continued doing well until the 14th day, when she showed signs of septicæmia. A glass tube was introduced into the small abdominal opening which had been left and from which pus had been oozing constantly, and the peritoneal cavity thoroughly washed out, whereupon all symptoms disappeared. At the middle of the fourth week, when the patient had already been discharged from treatment, the temperature suddenly rose to $103-104^{\circ}$, the pulse to 130. The finger passed into the abdominal wound felt a foreign body, which, on its removal with a dressing forceps, proved to be the placenta. It had a shrivelled, shrunken appearance, and was of the ordinary size. The temperature went down within three hours, and the patient recovered. This is the plan he would always follow in these cases in future. He would only be still more careful not to touch the placenta, and to keep the abdominal wound well open until that organ has been expelled.

DR. GEORGE T. HARRISON presented a paper on

THE TREATMENT OF POST-PARTUM HEMORRHAGE.¹

DR. BACHE McE. EMMET read a paper on

DISEASE OF THE BLADDER CONNECTED WITH UTERINE DISPLACEMENTS.¹

¹ See Original Communications in this Number.

TRANSACTIONS OF THE PHILADELPHIA OBSTETRICAL SOCIETY.

Reported by W. H. H. GITHENS, M.D., Secretary.

Stated Meeting May 4th, 1876.

DR. J. L. LUDLOW, *Vice-President, in the Chair.*

A CASE OF PELVIC CELLULITIS TREATED WITH HYPODERMIC INJECTIONS OF QUINIA.

Dr. JOHN M. KEATING reported: "I present to the Society this evening the following history of a case which occurred in the wards of the Philadelphia Hospital, during my term as resident accoucheur, two years ago:

Agnes M——, aged twenty-two, a native of Philadelphia, single, and pregnant for the second time. The first child was born two years ago in this hospital, after a hard labor, as she expresses it, which finally required the use of the forceps. Her convalescence was rather long, but she recovered perfectly. The present labor was a short one, lasting but two and a half hours, uncomplicated, for the child was rather under weight.

As she complained of some after-pain, Barker's mixture of iron, ergot and nux vomica was ordered, and the application of hot poultices, alternating with turpentine stupes to the abdomen. As puerperal infection was rife in the ward at the time, stringent measures were adopted in this and other cases, with regard to local cleanliness and the free use of disinfectants. On the third day (the 24th of the month), she had a chill, another followed on the fourth day; at each time I ordered her thirty grs. of quinia (in three doses at intervals), with morphia. The bowels were opened on the third day. At this date also she complained of the nausea following her medicine; it was changed and a mixture of sp. ætheris nitros., tinct. opii deodor. and acid. nitro-muriat. dil., in lemon syrup, substituted. This was taken three times daily. She craved acids from the first, and lemonade was freely given.

On the 26th (A.M.) she had some abdominal pain, but the uterus was well contracted, the lochia normal, not offensive in the least.

26th, P.M.—Pulse 96. Throbbing pain in head. The bowels had not been open for twenty-four hours. She has been taking twenty-five grs. of quinine in divided doses, morning and evening, since confinement. Her temperature has been recorded as follows:

22d, P.M., 99°.

23d, A.M., 99°; 23d, P.M., 100°; 24th, A.M., 99°; 24th, P.M., 103 $\frac{3}{5}$ °; 25th, A.M., 102°; 25th, P.M., 103°; 26th, A.M., 100°. This evening it is 102 $\frac{2}{5}$ °; pulse, 96. She suffers continued nausea and vomits everything she eats. Hop poultices have relieved somewhat the abdominal pain; but she still cannot bear any pressure. There is some tympany. The pulse is quite hard; the respiration normal. She had a chill this afternoon. Chlorodyne ordered in small, often repeated doses.

27th, A.M.—Temperature 102°. There is much tympany; the pain is more severe in the left iliac fossa; still vomits; stopped medicine and ordered ice, morphia hypodermically, lime-water, and counter-irritation, but they have not controlled it. One sixth grain calomel powders, which were repeated hourly this morning, opened her bowels. I may state in this note that I saw her last evening at seven o'clock, after a very severe attack of vomiting, during which she had thrown up about a half-ounce of bright frothing blood. The pulse was hard and full, the belly tympanitic and very tender. The tongue was furred in the centre, with red edges; gave one-third gr. morphia hypodermically. Saw her again at eleven. She had had a sound sleep; the nausea was relieved and she was free from pain. Her temperature was then 100°, pulse 108; respiration 20. Ordered during the night f $\bar{3}$ iij. of whiskey in punch.

28th, A.M.—Temperature, 101 $\frac{3}{5}$ °; pulse, 100; somewhat better generally than yesterday. Has vomited but once since daylight. The nausea is still present. Gave in powder small doses of pepsin, bismuth and strychnia, Valentine's beef extract, iced milk, lime-water and milk punch, as she is beginning to get exhausted. Discharge normal.

28th, P.M.—Vomiting not yet arrested; temperature, 100 $\frac{3}{5}$ °; pulse, 112; respiration, 24; the abdominal pain much increased by continual retching; has left but little upon her stomach to-day. At eight o'clock gave a hypodermic of quinia gr. iv., morphia one-sixth gr.; her temperature was then 102 $\frac{4}{5}$ °; at nine o'clock it was again taken and was 101 $\frac{2}{5}$ °.

29th, A.M.—Her temperature had gone up to 105°. 29th, P.M., carbonic acid water, curaçoa and whiskey every hour, checked the nausea; temperature at six o'clock was 104°; at eight it

was $102\frac{3}{8}^{\circ}$; has taken beef extract with the other mixture all the afternoon.

30th, A.M.—Temperature, $99\frac{3}{8}^{\circ}$; pulse, 98.

30th, P.M.—Quinine by hypodermic.

May 1st, A.M.—Pulse, 112; temperature, 103° .

“ 1st, P.M.—Pulse, 120; temperature, 103° .

“ 2d, A.M.—Pulse, 100; temperature, $102\frac{2}{8}^{\circ}$.

“ 2d, P.M.—Pulse, 118; temperature, 101° .

“ 3d, A.M.—Pulse, 104; temperature, $103\frac{1}{8}^{\circ}$.

“ 3d, P.M.—Pulse, 112; temperature, 103° .

May 4th.—She has taken nothing but milk and whiskey, and such nourishment as she could retain, since the 27th. The abdominal tenderness probably diminishes and localizes itself to the left iliac region. Last night I ordered her a blister over seat of pain, which has relieved her partly. Every night she has had a hypodermic injection of morphia, with occasional ones of quinine, grs. iv.

At 8½ o'clock last evening her temperature was $103\frac{1}{8}^{\circ}$. Pulse 112. Gave hypodermic of quinia grs. iij., and one-fourth gr. of morphia. The temperature at 9½ was $102\frac{3}{8}^{\circ}$.

May 4th, P.M.—Temperature again up, and once more considerable abdominal tenderness. No attributable cause but change in weather. Severe N.E. storm. At 8 o'clock the thermometer marked $104\frac{4}{8}^{\circ}$. At 9 o'clock, after a hypodermic of quinia gr. iij., and $\frac{1}{8}$ gr. of morphia, it was $103\frac{3}{8}^{\circ}$.

May 5th, P.M.—Passed a good day; her appetite seems much improved. It was thought advisable to try small doses of citrate of iron and quinia. After the first dose she vomited, and the sickness continued for two hours (until evening). At 8 o'clock her temperature was 106° , pulse quick and weak. She was vomiting continually, and nothing checked it but the carbonic acid water, curaçoa, and brandy. At 8 o'clock (temperature 106°) a hypodermic injection of 6 grains of quinia and $\frac{1}{4}$ gr. of morphia given. At 12 the temperature had gone down to 103° . As there was considerable pain in the belly, poultices were renewed.

May 6th, P.M.—Much better; has no pain; has taken brandy all last night and to-day.

May 7th, P.M.—Rested well last night. Seemed comfortable during the day, with the exception of some slight nausea. To-night she has some pain.

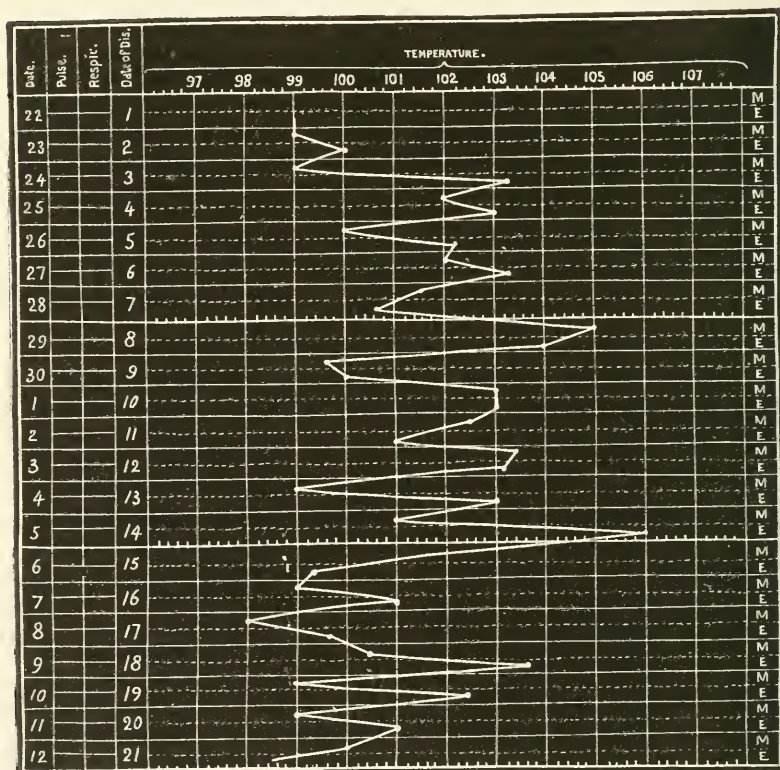
May 8th, P.M.—Pain continues. Temperature at 8½ P.M. was $101\frac{1}{8}^{\circ}$; pulse 120°. Says she feels much better, and were it not for the local trouble would like to get up. Takes 6 ounces of whiskey during the twenty-four hours.

May 9th, P.M.—Pain quite severe. In the afternoon she vom-

ited her nourishment. Temperature at 7½ P.M. was $103\frac{3}{5}^{\circ}$. Gave 6 grs. of quinia hypodermically, with $\frac{1}{4}$ gr. of morphia. At 8½ thermometer marked $101\frac{3}{5}^{\circ}$.

May 10th.—Passed a comfortable day.

May 16th.—Sits up every day; still very weak, but is improving daily. Suffers much from constipation. A tumor about the size of a small foetal head, situated deep in the left iliac fossa, and projecting into the Douglas's cul-de-sac, is all that remained to prevent a very rapid recovery. Repeated blisters, tonics, and laxatives soon promoted its absorption, and our patient left the hospital, completely recovered, some time after I came off duty.



The repeated introduction of quinia by injections tended only to produce but slight irritations of the arm into which the hypodermics were given. I believe this was due to the fact that in the first place no glycerine was used in the preparation.

The quinine was dissolved in the minimum quantity of diluted sulphuric acid, the needle inserted deeply into the subcutaneous tissue, and afterwards the arm was immediately wrapped in cloths saturated with a lead water and landanum lotion.

No abscesses whatever were formed, although it seemed probable that the peculiar conditions of our patient would render her particularly liable to the accumulation of pus."

DR. JOHN M. KEATING also reported

A CASE OF PUERPERAL FEVER, WITH POST-MORTEM EXAMINATION.

"The following case I deem of sufficient interest to read before the Society, as it presents in its simplest and most virulent form the disease described as *puerperal fever*. It occurred when puerperal poisoning was rife in the obstetrical ward, and when the temperatures taken daily of all the cases confined within a period of two months showed, to a greater or less degree, the influence of the poison, independent of any local trouble. The visit of the epidemic was abrupt, following no appreciable cause, and the termination had the same characteristic. The most careful measures were adopted to perfect the hygiene of the wards; the mildness of the spring weather permitted free ventilation, and the use of disinfectant vaginal injections was vigorously enforced; but all seemed to no purpose, until the virulence of the poison had expended itself. Then suddenly the line of demarcation became developed, and the patients afterwards confined had a perfectly normal convalescence. I make this statement because I think it bears upon the etiology of this affection—the same accoucheurs, the same ward influences, the same nurses, and the same number of tears and rents in perineal tissue, certainly point to the fact of a heterogenic cause, and leave much room to doubt the theory of the *localists*, or even that of autogenesis. I have been struck, while reading this subject in the admirable book of Dr. Fordyce Barker, with the absence of any allusion to a microscopic examination of the heart-tissue in the reports of the post-mortems of these cases. The very rapid pulse rate, and the other symptoms mentioned in most of his cases, certainly point to an advanced degree of cardiac fatty degeneration—so complete, in fact, as to be in reality the cause of death. This, with the almost total absence of any invariable local lesion, certainly points to a specific fever, resembling to a certain degree those already familiar to us as resulting from a specific cause. If such is invariably the case, which I believe to be true, our treatment should be based upon the symptoms presented. If given a case of typhoid fever, with no marked intestinal lesion, with a quick, gaseous pulse, absence of the

muscular sound of the heart, and every other evidence of fatty degeneration, what would be our treatment? Absolute quiet, digitalis as a heart tonic (and certainly not *veratrum viride*), alcohol in large quantities, and nourishment in every form. We have no specifics; we treat symptoms, based upon pathological conditions. The same in puerperal fever; the symptoms are identical. The treatment should from the first be the same in the two diseases.

We have all seen cases of typhoid fever taking alcohol in amounts measured by the pint, proceeding rapidly towards convalescence, and we know that from some sudden exertion death has resulted from a cardiac failure; that a microscopic examination revealed a heart composed throughout of fatty granules in lieu of muscular fibre. In this condition I think quinia avails but little. Our main stay should be rest, alcohol, milk, and beef tea, with acids, if desired, and digitalis and opium in small quantities, if required.

I should save the quinia for cases of pyæmia, where its action would certainly be followed by more positive effects.

Bella B—, æt. twenty-one, a native of Ireland, single, had been in the house for some time and had passed on to full term without any trouble whatever. She was a strong, healthy-looking girl, of an exceedingly cheerful disposition, and never exhibited at any time during her residence in the house the least anxiety as to the future of herself, nor regret on account of the illegitimacy of her offspring. She was delivered April 20th of a nine-pound girl, at eleven and a half o'clock P.M., after a painful labor, though in every way a normal one, requiring no instrumental assistance.

There was some slight laceration beyond the fourchette, of about one-quarter of an inch.

Uterus contracted well.

April 21st, P.M., temperature 99°. In the evening it was 99½°.

April 22d, morning temperature 101°, evening 103°. Appearance of milk. She is taking ten grains of quinine in the morning, and fifteen grains in the evening, which treatment has been applied to all the confinement cases in the house within the last few weeks, on account of the great tendency of all the cases to high temperature.

April 23d, A.M. 103°, P.M. 105°. To-day, careful examination showed for the first time some diphtheroidal patches in the vagina. She has had some slight abdominal pain, but it has all passed off. She was also troubled for two days with retention of urine. To-day her tongue is furred on the edges, with a red tip and centre line. Her bowels have become loose and offensive, the stools dark and liquid, and attended with

much tympany. She has no pain whatever, even upon pressure.

April 24th (fourth day), the morning temperature is 102° , evening 101° .

April 25th, A.M., pulse 120, temperature 101° . Evening pulse 112; temperature 101° .

April 26th, A.M.—In addition to the quinine which she has been taking, and with the nourishment of beef-tea and milk, she is ordered lemonade, which she craves, and a mixture of

R Spts. ætheris nitros. f 3 ss.
Tint. opii deodorat. gtt. x.
Acid. nitro-muriat. dil. gtt. xx.
S. Limonis q. s. every three hours.

Evening 26th, pulse 120° , temperature $100\frac{2}{3}^{\circ}$. She has taken her medicine all day, and says she “likes it because it is sour.” Her bowels are pretty loose, but nothing has been given to check them. Takes her nourishment well. Face dusky.

April 27th, A.M.—Temperature 104° . Ordered whiskey punch (but in what amount my note fails to state). Bowels not so open, and the stools not at all offensive. Lochia are still present; not offensive.

The manner of giving the quinine had been changed to gr. iij., with tinct. ferri chlor. gtt. x., every three hours. Has much tympany, for which turpentine stupes alternating with poultices have been applied. The tongue is very much furred, and her face is very characteristic of her condition. Sleeps well at night, and seems cheerful.

April 27th, evening.—Temperature 101° , pulse 136, gaseous; the first sound of the heart quite feeble.

April 28th.—This morning the bowels have been checked by opium suppositories. The temperature is $102\frac{1}{2}^{\circ}$; pulse 136. She is taking six ounces of whiskey in punch, quantities of milk and beef essence (Valentine’s). Has no pain, is bright, cheerful, and not in the least alarmed about herself.

April 28th, P.M.—Bowels became this afternoon again loose. Ordered subnit. bismuth gr. xx. with f 3 ss. of tinct. opii camph., to be repeated at midnight if necessary. Temperature 101° , pulse 130. Marked tremor.

April 29th, A.M.—Temperature $100\frac{2}{3}^{\circ}$. Pulse somewhat stronger, but still very compressible. Face exceedingly dusky. Discharge disappeared yesterday afternoon, but returned again this morning. Increased the punch for to-night. Evening temperature $102\frac{2}{3}^{\circ}$. Pulse 136. Cardiac fatty degeneration, noted as seeming more marked.

April 30th, A.M.—Pulse 130. Temperature 102° . She

suffers no pain. Diarrhœa constant, and is only checked for a time by the bisulph and opium. Seems decidedly worse.

April 30th, P.M.—This evening at six o'clock the temperature was $103\frac{2}{3}^{\circ}$. When seen at ten and a half o'clock a great change had taken place. She was breathing rapidly, thirty-two; the temperature had risen to $105\frac{2}{3}^{\circ}$. She was slightly delirious at times, imagining she saw strange things about her bed.

The heart sounds were very weak, the first scarcely audible, and the pulse so weak that I could scarcely count it. Ordered $\text{f}\frac{3}{4}$ iij. of whiskey at twelve o'clock with thirty drops of digitalis (tinct.), and fifteen drops to be repeated every two hours. At ten and a half, when I saw her, I gave ten gr. of quinine, which in the course of an hour reduced the temperature but three-fifths of a degree. The first dose of digitalis she vomited, and the nausea and vomiting has continued throughout the morning, not permitting the retention of any nourishment or stimulants. These were given by enemata.

May 1st.—Temperature this morning $104\frac{1}{2}^{\circ}$. Pulse 160. Respiration 36. Is perfectly rational, has no pain. Urine albuminous. I saw her at twelve o'clock mid-day, when, calling me to her bedside, she asked the cause of the increasing coldness of her feet, which had become cyanotic. Sitting up in bed, with no evident fear of death, which she thoroughly understood was rapidly approaching, this patient presented one of those spectacles so horrible to behold, and which leave an impression time can never efface, nor diminish the dread which attaches itself to a disease so insidious and so far beyond our control.

Autopsy made at 8 o'clock, A.M., 20 hours after death. Rigor mortis well marked; marked hypostatic congestion of the surface; abdomen very much distended with gas. Vulva very much tensified, and mucous membrane of it and vagina of a dark purple color. The fourchette was lacerated, and the mucous membrane torn for almost half inch into the vagina. This torn surface was the seat of a rather deep ulcer, which was *not* healing; there was no deposit upon it. The rest of the mucous membrane was intact.

Thorax.—The lungs collapsed readily upon opening the thorax. There was no effusion to the plenræ nor in the pericardium. The exposed surface of the lung was remarkably anæmic. Before removing the lungs the posterior surface was found to be moderately congested (hypostatically). Lung tissue healthy, no thrombi, no deposits. The heart was flabby, dilated, not hypertrophied. Upon removing it, the vessels leading to it were found full of dark liquid blood. In the pulmonary artery there was the usual yellow clot, which extended some distance into the lung; it also extended into the right auricle

and ventricle. The valves were perfectly healthy. The tissue of the organ seemed pale, and under a careful microscopic examination proved to be *decidedly* fatty, the striated appearance being entirely destroyed by the granular degeneration.

Abdomen.—It was found that most of the tympanitic distension was due to the accumulation of gas in the large intestine, although the small intestine was much distended also. The large omentum was spread over the intestines, and presented no evidences of inflammation on its upper surface. On lifting it up the small intestines were found covered with thin patches of a yellow gelatinous lymph, which extended between the coils of the intestines, gluing them slightly together. These deposits were easily removed, but the opposing surfaces of the intestines were also found to be slightly glued together. The diaphragmatic surface of the liver was coated with a thin layer fastening it loosely to the diaphragm. The peritoneum in general was dry and finely injected. The mesenteric glands were not enlarged. The ileum was examined, and the mucous membrane was very anæmic; there was no enlargement of the solitary glands, and Peyer's patches were with difficulty even found. After removal of the small intestine and its mesentery, upon attempting to tear the gut from its attachments, it was found that the peritoneal covering and part of the muscular coat remained with the mesentery, while the mucous membrane and the sub-mucous layer were "telescoped" out. Scattering muscular fibres remained upon the outer surface of the inner tube.

Kidneys.—Weighed, one 5 ounces, other $5\frac{1}{2}$. They were extremely soft, very congested, and the capsules not at all adherent. Microscopic examination revealed the epithelial cells so far advanced in fatty degeneration that their outline was barely discernible.

Spleen.—Pulpy, large, dark red.

Liver.—Fatty, and *very* soft.

Uterus and appendages were soft, the fundus presenting two deep impressions ($\frac{1}{4}$ inch) of two coils of intestine. The left-hand ligament and ovary were bound together by patches of lymph, soft and yellow, around which about one-half teaspoonful of pus was found. This was the only fluid found in the peritoneal cavity. The Fallopian tubes were free. The ovaries were very much softened, congested, and œdematous—the left one presenting a large hypertrophied corpus luteum. There were a few small patches of lymph over body of uterus, especially on its posterior surface. There was but little œdema of the pelvic connective tissue. The uterus weighed 1 lb. $2\frac{1}{2}$ oz., and measured, cavity, from os to fundus, 6 inches, and at widest part 3 inches; walls $\frac{3}{4}$ of an inch (thickest). The cervix

was not torn at all. The mucous membrane seemed to be perfectly healthy, of a normal pink color, the arbor vitæ being quite distinct, and the placental thrombi apparently normal. There was no pus whatever to be seen in the muscular structure of the uterus. The sinuses were filled with dark liquid blood. No evidence anywhere of inflammation or ulceration.

Bladder.—Empty. The mucous membrane was the seat of localized congestions and patches of ecchymosis."

Stated Meeting, Thursday, July 6, 1876.

DR. J. L. LUDLOW, *Vice-President, in the Chair.*

OVARIAN CYST.

DR. ELLIOTT RICHARDSON exhibited a specimen of commencing ovarian cyst, showing the early absorption of the ovary.

DR. GITHENS then read the following report of a case of

VIABILITY OF A SIX-MONTHS FŒTUS.

"Mrs. E. P. had been under my treatment for menorrhagia, the menstrual flow being profuse and occurring every two weeks. Her husband had been living in another city, kept there by business. He made a short visit, the first in many months, to his home on the twenty-fourth of November last. The menstrual flow appeared the succeeding week, and lasted about five days. Early in January it appeared again slightly, passing off in one day.

Fœtal movements were felt about the first of April, and on May twentieth my patient was delivered of a female child, about thirteen inches in length. It had but little hair, and the nails of the fingers and toes were imperfect; respiration was established without artificial assistance, and the child is living and growing nicely. I did not allow it to be washed, and had it wrapped in cotton, which was changed daily. The cotton dressing was continued for two or three weeks. It was nourished on milk and cod-liver oil until able to nurse, the secretion of milk having meanwhile been encouraged by the loan of a sister's child.

This infant has shown its ability to support an independent existence at the early age of one hundred and seventy-eight days, or six calendar months."

ABSTRACT OF THE TRANSACTIONS OF THE AMERICAN GYNECOLOGI- CAL SOCIETY.

FIRST ANNUAL MEETING.

HELD IN THE CITY OF NEW YORK, SEPTEMBER 13TH, 14TH, AND 15TH,
1876.

First Day—Morning Session.

THE Society met, pursuant to call, at the Hall of the New York Academy of Medicine, at 10 o'clock A.M., September 13, 1876.

The President, DR. FORDYCE BARKER, of New York, called the meeting to order.

There were present Drs. Barker, Peaslee, Emmet, Thomas, I. E. Taylor, Noeggerath, Lusk, and Mundé, of New York; Lyman, Richardson, Bixby, and Chadwick, of Boston; Atlee, Goodell, A. H. Smith, and Drysdale, of Philadelphia; Byrne and Skene, of Brooklyn; Trask, of Astoria; Johnson, of Washington; Wilson and Howard, of Baltimore; Byford, of Chicago; White, of Buffalo; Parvin, of Indianapolis; Jenks, of Detroit; H. F. Campbell, of Augusta, Ga.; and J. G. Engelmann, of St. Louis.

DR. T. G. THOMAS, Chairman of the Committee of Arrangements, welcomed the invited guests who had honored the Society by their presence, and especially the gentleman from England, whose name was known wherever obstetrics are practised as a science, Dr. Robert Barnes, of London. He also congratulated the Society upon the fact that the names of so many eminent men were associated with it in its infancy, and that so many were present at its baptismal ceremony.

On motion made by DR. THOMAS, Dr. Barnes was invited to a seat of honor by the President.

The following gentlemen were elected members by invitation:

Drs. Trenholme, of Montreal; Hodder, of Toronto; Tagliaferro, of Georgia; Rosebrugh, of Hamilton, Canada; and Reamy, of Cincinnati.

The reading of papers being the next business in order, Dr. T. ADDIS EMMET, of New York, read a paper on the

ETIOLOGY OF UTERINE FLEXURES, WITH THE PROPER MODE OF
TREATMENT INDICATED.

In this paper were given the results of a careful study of the pelvic organs of the female, beginning at the commencement of menstrual life. The results obtained justified the conclusion that the origin varied in different cases of flexure, hence the treatment, if intelligent, should be varied accordingly. The doctor's observation had been made upon 345 cases of different forms of flexures treated in his private hospital, and the tabulated statistics upon 2,447 cases of different diseases and injuries peculiar to women were used by him as a standard for comparison. With reference to flexure, Dr. Emmet took the position that there were two classes of cases. There was one class in which the flexure was below the vaginal junction, was a congenital condition; and in those cases the pain, if present, was most severe before the flow. In some of these cases there was no suffering at all, for the reason that the congestion of the cervix straightened it sufficiently to permit an unobstructed discharge of fluid from the uterine cavity. It was in these congenital cases that the operation of incision was justifiable and beneficial, and could be most advantageously made by means of scissors. The subsequent treatment should be both local and constitutional. The incision must be kept from uniting. It was beneficial, because the circular fibres were severed, and the longitudinal ones would draw the neck back, thereby assisting to render the canal permanently straightened.

In the other class of cases the flexure involved the body of the organ to a greater or lesser extent; was above the vaginal junction and due to obstructed circulation; occurred at some date subsequent to puberty; was due to a variety of causes; might follow impregnation; and in those cases the pain was during the flow. In such cases local treatment was to be instituted first, and continued until the case was cured, before the operation of incision was performed. Dr. Emmet regarded the operation of incision in this class of cases, before the congestion and inflammation had been removed, as malpractice. By hot vaginal injections, local application to the uterine canal and the use of glycerine in the vagina, etc.; by daily lifting the organ up to the health line with the finger, etc., etc., the case was to be cured. When a flexure had been of long standing a loss of tissue often took place at the angle, so that a permanent deformity remained, as after caries of the spine. At the close of the treatment, to allow of free escape

from the canal, an incision, both through the posterior lip and at the angle of the flexure, would be eminently proper. In all cases when there was doubt whether the flexure was confined to the neck or involved the body, it was regarded as advisable to be very careful before proceeding with the operation. The doctor does not divide the cervix laterally, except in the treatment of fibroids or for the opening of a partially closed os, and does not extend the incision beyond the crown of the os.

The paper was received with great favor.

The President remarked that this important paper was then before the Society for discussion, which he hoped would be frank, bold, and decided, and yet free from all personalities. As many of the members were well aware, this question had been made the subject of a most vigorous paper by Dr. Peaslee, and indeed there was a diversity of opinion regarding the necessity of the operation and the manner of its performance. It was expected that Dr. Sims would be present and participate in the discussion, but in his absence he would call upon Dr. Peaslee, whose name came next upon the announcement.

DR. PEASLEE remarked, with regard to Dr. Emmet's paper, that he had sanctioned its practice as far as was possible, and he was very happy to have the opportunity to make that statement there, because he had been misunderstood with reference to his ideas upon this subject expressed in the paper to which the President had done him the honor to make allusion. He had been supposed by some to take the ground that dissection of the cervix uteri should never be performed in any case of flexion. "I stated," said the doctor, "as follows, and I take the liberty to repeat it, because I regard the practice of great importance and value." In addition, however, to what is here said, I might have added that Dr. Emmet is entitled to full credit for the operation." Dr. Peaslee then read from a printed copy of his paper, "I know of no other condition in which bilateral dissection alone is justifiable, etc., . . . but in such cases the incision should be closed as soon as the fibroid is removed. "Posterior dissection," continued the doctor, "was first performed by Dr. Sims, but he became so dissatisfied with it that he entirely discarded it, and Dr. Emmet took it up and has shown its real value."

Dr. Peaslee then made the following personal explanation: It had been supposed by some persons that he claimed priority in the use of the superficial incision of the cervix uteri. He disclaimed this entirely, and had spoken of it as a method and nothing more, and called it superficial trachelotomy.

The superficial incision, however, he had defended, and on

scientific grounds, as more beneficial than the deep, and in that respect, perhaps, he was entitled to the claim of priority. He had been told that his own paper was to be discussed, and he was extremely disappointed that Dr. Sims was not present, for he had learned that the doctor had promised to annihilate his paper, and he (Dr. P.) had invited some of his friends there as mourners upon the occasion. He was, however, above all personal consideration in the matter, and hoped that every person would criticise the paper as severely as he might deem proper. As it would not be proper for him to take the lead in discussing his own paper, he would give way to others.

The President invited Dr. Barnes to participate in the discussion.

DR. BARNES responded by saying that the paper by Dr. Emmet required examination with very great care, and that therein had been suggested certain points which he did not then feel prepared to discuss. With regard to the opinion expressed in the paper that many cases of flexure began as retroversion, and became cases of retroflexion about the age of puberty, he believed it to be a difficult point to decide, for the reason that opportunities for examining patients to settle this question were very rare. He was satisfied that many cases of flexure were congenital, but he could hardly understand how they began as cases of retroversion and subsequently became cases of retroflexion.

Dr. Barnes maintained that in many cases there was no retroflexion to contend with; that there was nothing more present than stenosis, associated, it might be, with some degree of anteflexion, scarcely more than normal, however, and which disappeared after division of the cervix uteri for the cure of the stenosis.

The decided opinion was expressed that, practically stenosis of the os internum did not occur, for he had never met with a case in which the sound could not be easily passed, and Dr. Barnes regarded that as evidence that nature had provided sufficient opening for the escape of the menstrual fluid. But in a great majority of cases there was closure of the external os, and the dysmenorrhœa would be relieved by enlarging that opening. If flexion was present to some extent, it would be relieved by the same operation. The pain in those cases began at the commencement of the flow, because the fluid could not easily escape, and such a condition was found not only in married but single women. There could be no doubt but that incision of the cervix was a dangerous operation, and that it should be performed with great care.

Division of the os externum, however, could be easily ac-

complied with the scissors, need not extend as far back as the vaginal junction, and was competent to relieve one of the most distressing conditions to which women were subjected. In some instances a certain amount of retraction might follow, and it might be necessary to repeat the operation, but success usually attended the first cutting. The operation, although simple, required previous preparation of the patient, and the woman should not be allowed to leave her bed within a week. The uterus, like other organs, required rest after being subjected to operation, and even after this modified form he had seen three fatal cases. In two of those cases, however, there were disturbing causes present which should not have been permitted.

DR. J. P. WHITE, of Buffalo, remarked that he thought Dr Emmet had given too little attention to stenosis of the cervical canal. True, it often depended upon flexion, but according to his own observation it was a condition that existed independent of flexion. But if flexion was present there was at the same time positive stenosis associated with it. It was to the treatment of that narrowing of the canal, no matter what it might depend upon, that Dr. White directed his remarks. The condition was productive of great pain and dysmenorrhœa. For its relief he had found no treatment to be compared in value with operative procedure upon the neck of the uterus. He made the bilateral incision more often than any other, especially where there was simple stenosis, and perhaps somewhat deeper than the superficial incision recommended by Dr. Peaslee. For that purpose he employed a small knife, something like a tenotomy knife, with a long handle, and believed it to better than any instrument arranged to cut to a certain depth. After the incision had been made, the important part of the treatment was to keep the canal dilated; and how could that be done? Immediately after the operation a piece of cotton might be introduced, but permanent dilatation could be effected by the daily use of an instrument he held in his hand, which was constructed very much upon the plan of the common glove-stretcher. The blades of the instrument were thin and yielding, and could do no injury. Almost all cases of painful dysmenorrhœa could be immensely relieved by that operative procedure, and a very large proportion previously sterile became fruitful. The Doctor further urged that such procedure should be instituted early, and he did not wait to first cure the case and then perform the operation. He made the operation, and expected the cure of the case to go on *pari passu* with it, whenever the dysmenorrhœa depended upon any of the conditions to which allusion had been made.

DR. HOWARD, of Baltimore, entered a criticism upon Dr. Peaslee's paper, to the effect that Dr. Sims had long ago abandoned the bilateral incision of the cervix, which Dr. Peaslee had criticised as a dangerous operation, except in the removal of fibroids, when Dr. P. himself resorts to it. His experience agreed with Dr. White's with regard to stenosis of the os internum, and he had seen a case within a few months where the opening was so small that it would not admit more than the smallest pocket-probe. In that case the narrowing was produced by the use of nitrate of silver.

Of late he had used Dr. Peaslee's instrument, and success had followed the operation in only a few instances. He had kept the cervix dilated with cotton until the wounds had nearly healed, and then used Dr. Peaslee's instrument for dilatation, and the cure had been continued in one case for about three months. In that case he introduced a seatangle tent, and effected sufficient dilatation to give some relief, but latterly the trouble had returned.

His experience had been that there was an almost uniform tendency to contraction when the operation was performed by means of Simpson's instrument. He had the same experience with reference to the bilateral incision, or Dr. Emmet's method, and it was for that reason that Dr. E. objected to it. Dr. Peaslee, on the other hand, objected to the operation because the edges of the wound remained gaping.

DR. HOWARD remarked that he had seen but few cases in which the os externum had been the seat of stricture. His experience also corroborated that of Dr. Emmet in the statement that this condition was almost always congenital. He also agreed with Dr. White in the statement that flexure at the os internum was accompanied by congenital stenosis, and in some cases which had fallen under his observation the narrowing had been extreme. The stricture was at the os internum, and was not due simply to flexure, although in some cases flexure accompanied it. The occurrence of pelvic cellulitis and peritonitis, in some cases, was believed to be unavoidable.

DR. ISAAC E. TAYLOR, of New York, recommended that in certain chronic cases, in which the tissues of neck and body had become dense and indurated, instead of incision, amputation of the cervix should be performed. It might be presupposed that it would be a serious operation, but in his experience it had not been, and the after-treatment had been simply to use warm water injections and keep the woman in bed for two or three weeks. Amputation of the cervix permitted the uterus to assume a better position, and the functions of the organ could be performed more properly.

Dr. Taylor also maintained that the cervix would be reproduced, just the same as reproduction of the tonsils was seen after excision, and that the new cervix would be changed both in form and structure. Dr. Taylor denied the existence of longitudinal fibres of the cervix, hence the retraction mentioned by Dr. Emmet could not take place, that is, dependent upon the action of such fibres.

DR. WILSON, of Baltimore, inquired with regard to the occurrence of conception after amputation of the cervix.

DR. TAYLOR replied that he believed the prospect to be better than after the posterior operation.

DR. WILSON remarked that he had amputated the cervix a number of times, and in no case had conception followed. His operations had all been for the relief of procidentia. With regard to the operation of incision he had had no serious trouble from hemorrhage, notwithstanding he had cut the circular artery four or five times. He had had one case of cellulitis and one of peritonitis after seventy-five or eighty operations. With regard to stricture of the os externum, he had not seen more than two or three cases, except when caustics had formerly been employed. He was not able to recall a single case in which he had found contraction at the internal os without flexure. He had been accustomed to make the incision to such an extent as to allow liberally for contraction, and after five or six days, had maintained dilatation by means of Nott's dilator. He had never performed the bilateral incision.

DR. HOWARD inquired how amputation of the cervix could relieve the stricture when seated above?

DR. TAYLOR replied that the changes effected in the remaining portion by the operation would be sufficient to relieve it. The operation set up a new and derivative action, and the cervix which was reproduced did not contract, was closed, and had a perfectly free mucous membrane.

DR. BYRNE, of Brooklyn, remarked that he had amputated the cervix thirty-nine times, and that to his certain knowledge pregnancy had followed in three instances, and doubtless in many more whose histories he had not been able to follow.

DR. TAYLOR added three more cases to the number in which conception had followed the operation.

DR. CHADWICK, of Boston, was of the opinion that, when the cicatrix, following the superficial incision recommended by Dr. Peaslee had contracted, the calibre of the canal would be reduced to almost its previous condition.

DR. PEASLEE remarked that he could have no intention of misrepresenting Dr. Sims when speaking of dissection of the cervix. He had simply fallen back upon Dr. Sims' own book for his

authority. It was Sims' operation, and he had not given us any published account of another to take its place. It had been stated that he disclaimed the operation within a few months, but Dr. Peaslee had understood from the best authority that Dr. Sims had performed the operation of late; and that was another reason he regretted that Dr. S. was not present to give the requisite information upon that point. Dr. Peaslee further remarked with regard to the superficial incision that, if followed by proper dilatation until healing was complete, there was but little tendency to contraction; by no means to the same extent as in the other operations. If there was actual stenosis, not produced by flexion, he would perform the operation, otherwise he would not.

Dr. EMMET remarked, in closing the discussion, that he had satisfied himself there were longitudinal fibres on the neck of the uterus. And at all events the fact remained that the neck became shorter after the operation, no matter whether it was done by the longitudinal fibres or not. With regard to the other operation, that in cases where congestion has been present, he expected more or less of contraction to follow, but he did not resort to it until all congestion had been removed by some means. If the uterus was heavy and sagged in to the pelvis, it must be lifted up; if the vessels are congested, that condition must be treated; and if retroversion is present, the uterus must be put into position, even into the anteverted position; for he did not regard anteversion as a malposition. When the circulation had become so relieved that it could go on without obstruction the uterus would get smaller, and when all symptoms of congestion had subsided, the operation might be performed.

The Society then adjourned to meet at 3 P.M.

First Day.—Afternoon Session.

The Society was called to order by the President, and the following gentlemen elected members by invitation:

Drs. Wm. Fox, of Madison, Wis.; G. E. Sussdorf, of Macon, Ga.; Charles Shepherd, of Grand Rapids, Mich.; F. N. Evelith, of Waldoboro, Me.; T. P. Seeley, of Chicago, Ill.; John T. Hodgen, of St. Louis.

Dr. A. J. C. SKENE, of Brooklyn, then read a paper on

CICATRICES OF THE CERVIX UTERI AND VAGINA.

The object of the paper was simply to notice the subject in order that it might receive the attention it deserved when open for discussion. Brief allusion was made to the causes

and symptoms, physical signs, and the measures to be resorted to in guarding against the occurrence of cicatrices. Dr. Skene advised immediate closure of lacerations, and by means of the silver-wire suture. If it became necessary to use caustics, a portion of the mucous membrane should be left untouched, if possible, so that the eschar would not completely circumscribe the canal.

The chief indications in treatment were to relieve pain and tenderness, prevent contractions, and when deformities were present to correct them. These requirements could be most perfectly fulfilled by removing the entire cicatrix and obtaining as nearly immediate union as possible. Such procedure, however, was not always practicable. Excision should not be undertaken in any case unless the scar was movable. A scar could be prevented from shortening by dividing it in one or more places, and then keeping the cut surfaces apart by means of a tampon or pessary. In that way the scar was lengthened, while the process of narrowing still continued. When the cicatrix surrounded the os externum it should be divided perfectly on both sides of the cervix, and a seatangle tent worn until healing was complete, and should be sufficiently long to enter the os internum.

The slippery-elm tampon, carbolized, was spoken highly of as a means of preventing contraction of cicatricial tissue in the vagina. To the pain and tenderness incision sometimes gave immense relief. Tincture of opium, aconite, and iodine combined was mentioned as a means for softening cicatrices.

Several cases were then cited in which the above principles had been practically applied.

The paper being open for discussion,

DR. BYFORD, of Chicago, spoke in favorable terms of the elm tampon, and related a case in which the vagina of a girl fourteen years old became completely closed as the result of acute inflammation following scarlatina. The case was successfully treated so far as all the purposes required at that period of life demanded.

DR. EMMET remarked that there was one point which he regarded as important, which Dr. Skene had not touched upon, and that was the effect upon the nervous system produced by cicatrices. It was regarded as probable that very many of the neuralgias from which women suffered, when not due to anæmia, were due to the presence of cicatricial tissue. Dr. Emmet believed that a surgeon succeeded in treating uterine diseases in proportion to the small amount of cicatricial tissue he left behind him. An erosion of the cervix might be cured, but the cervix may not have been benefited because of the cicatricial

tissue that has been left. Unquestionably, as the tissue became dense, nerve filaments became involved, and to such condition an immense amount of the neuralgias was to be attributed from which women suffered, especially when the cicatrices were about the neck of the uterus and the urethra. In those cases where a cord-like band was present, he had followed the same principle mentioned by Dr. Skene, namely, that of dividing the bands, and then placing the parts upon a stretch, instead of leaving it to heal undisturbed. Dr. Emmet's plan had been to cut in six or eight places, separate the edges for a half-inch or more, and then bring into the gap healthy tissue from the sides, to be secured by at least two sutures, for a single suture would not answer. To Dr. Sims, he believed, was due the idea of using the glass vaginal dilator, and it must be regarded as the most valuable means at our command for placing those parts upon the stretch.

DR. BYFORD moved that Dr. Joseph A. Eve, of Augusta, Ga., who was doubtless the oldest lecturer on obstetrics living in this country, be invited to take a seat by the President, and participate in the discussions. Unanimously carried.

DR. E. W. JENKS, of Detroit, then read a paper on

VIBURNUM PRUNIFOLIUM: ITS USES IN THE TREATMENT OF DISEASES OF WOMEN,

and made special reference to its value as an agent for preventing abortion, especially the habitual variety. As a uterine sedative its action was said to be as pronounced as that of ergot in producing uterine contraction. It was also said to be serviceable in all uterine disorders characterized by a loss of blood. In treatment of menorrhagia and metrorrhagia depending upon constitutional conditions, it was especially applicable. In Dr. Jenks' hands the remedy had afforded marked relief in all forms of dysmenorrhœa, except the spasmodic or neuralgic, where it was not sufficiently sedative. The bark of the root was said to be the preferable portion to be used in making the fluid extract, which could be given in drachm doses three or four times a day.

DR. BATES, of New York, remarked that viburnum was a drug largely employed by the Eclectics. He had also used it to some extent, and was certain that it had power to prevent abortion, especially in cases where the accident had become habitual.

DR. WHITE, of Buffalo, had known that the remedy was used by the Eclectics, but he had had no confidence in it. He advocated its use, however, for a uterine sedative was certainly desirable. It was pretty well established that in a majority of cases

habitual abortion depended upon disease of the ovum, and how the drug could arrest that he was not able to understand. Still, the drug should be tried.

DR. MUNDE, of New York, inquired whether Dr. Jenks would regard the remedy as a safe substitute for the only uterine sedative we now have, namely, opium? A substitute for opium in cases of threatening miscarriage would be exceedingly desirable.

DR. JENKS replied that he would not hesitate to take the remedy as a substitute for opium, and, indeed, regarded it as preferable. He had prescribed the drug empirically, and had no theory concerning its physiological action.

DR. T. PARVIN, of Indianapolis, then reported a case of

ABNORMAL MENSTRUATION,

in which the oozing of blood was from the lower lip and gums.

The Secretary, Dr. Chadwick, next read a paper received from DR. HORATIO R. STORER, of Boston,

ON THE IMPORTANCE OF UTERINE EBB AS A FACTOR IN UTERINE SURGERY.

It was maintained in the paper that there was an ebb and flow in the female body, which should be regarded, if the greatest success was to be obtained in operations upon her pelvic organs. It was claimed that the different results obtained in similar cases did not depend altogether upon the surroundings or the operator, but that there were at work other influences, the exact nature of which had not been reached. The selection of cases, and the employment of skilled assistants, went far towards securing good results; but even then patients would often perish. The uterine ebb was regarded as analogous to the ordinary periodicities manifested in the human body, occurred regularly like day and night or the tides, and should not be disregarded in surgical operations upon the pelvic organs. All things being equal, the most favorable time for performing operations upon the pelvic organs was about one week subsequent to the occurrence of the menses, or at a period corresponding to that as nearly as possible, in cases in which the catamenia were absent.

DR. H. F. CAMPBELL, of Augusta, Ga., remarked that he endorsed the principle advocated by Dr. Storer, and that he had operated in accordance with it for many years. His favorite time for operating was about a week after the cessation of the menstrual flow.

The discussion being closed, the Society adjourned until 10 A.M., September 14th.

Thursday, Sept. 14th. Second day—Morning Session.

The Society was called to order at 10 A.M., by the President.

The Secretary acknowledged the receipt of two communications; one from Dr. E. Seguin, and the other from Prof. J. C. Dalton.

On motion made by Dr. Parvin, the following gentlemen were made members by invitation:

Dr. Ira Russell, of Massachusetts; Dr. Ward, of Alabama, and Dr. B. B. Brown, of Baltimore.

DR. BYFORD, Vice-President, was called to the Chair.

The President's

INAUGURAL ADDRESS

being next in order, DR. BARKER delivered an able and interesting discourse. He first alluded to the history of the Society, and its conception. Attention was then called to the fact that the status of the Society would be determined by the character of the papers, and the tone and ability of its discussions. Papers required discussion at the meetings, and their effect for evil should be exposed, and their positive value receive a prompt endorsement. Certain papers which would be presented would be valuable only upon careful perusal and quiet study, for they would embrace questions which could not be properly handled in forensic discussion. It was believed that the demand for a high degree of excellence in papers from candidates for membership would be advantageous. The discussions should be conducted orally. Dr. Barker then instituted a comparison of the period at which this Society commenced with the period which this centennial year commemorates, and drew a striking picture of the progress that had been made in all that pertained to gynecological and obstetric science. How uncertain must have been the diagnosis of pregnancy, for example, in absence of a knowledge of the pathological changes which take place in the cervix, ballottement, auscultation, etc., etc.

It is now no longer safe to publish as original what has been published previously by some German author.

Who would undertake an attendance upon a case of confinement with all knowledge of anæsthetics and the mechanism of parturition blotted out? Yet, with all our knowledge, no definite future had been obtained, and no man could safely rely upon his present acquirements. In conclusion, the hope was expressed that the Society might receive the approval of the highest and most cultivated judgment of the scientific world, and not incur the reproach which Job in his bitterness uttered, "Ye are all physicians of no value."

On motion made by Dr. Chadwick, a vote of thanks was extended to Dr. Barker for his able and interesting address.

The President then introduced DR. ROBERT BARNES, of London, England, who read a paper on

SOME OF THE RELATIONS OF PREGNANCY TO GENERAL PATHOLOGY.

The position taken by the author was that it had been too much the practice to detach the study of such phenomena as menstruation, ovulation, pregnancy, etc., from each other, and the influence which they exerted upon the general system. There had been a lack of comprehensive consideration in our studies. In many instances it was necessary to study the phenomena of disease, not only in the female, but the male organism, in order to fully understand its nature and manifestations. For example, if Bright's disease were studied alone in men, and in women outside of pregnancy, it might be inferred that organic change in the kidney was essential to albuminuria. Unless our study of this disease had been extended to the pregnant woman, where we find that all the manifestations of the disease may be present, such a conclusion would be legitimate. The transition from physiological to pathological and back again is very rapid in pregnancy, and the author glanced at some of the changes that had been found to occur in the blood, glandular system, lymphatic system, and nervous system.

In the blood the chemical changes were first noted, such as increase of fibrin, diminution of albumen, etc. The difference in time of coagulation was noted as bearing upon the question of thrombosis. Reference was made to the relations existing between the respiratory organs and the pregnant uterus, and the changes which take place in the exhalation from the lungs, consequently changes in the blood.

The dynamics of the circulation were referred to, and the hint given, that, could we determine beforehand, by means of the sphygmograph or other means, that the arterial tension was rising too high, we could with propriety undertake the removal of the pressure by drawing a little blood.

The changes in the glandular system were noted, especially those occurring in the thyroid and spleen. The relation of enlargement of the spleen to ague and pregnancy, in the mind of the author, constituted a broad field for study. What relation existed between enlargement of the spleen, blood degradation, exalted centric nerve-irritability, and ague? The relation of the disease to pregnancy must be studied if we would fathom the cause of the disease.

Pregnancy might be regarded as a test for the cure of chorea.

The glands of the neck might undergo a remarkable growth, and the woman suffer from hydrorrhœa. Excessive salivation was sometimes seen in pregnancy. There was probably a marked increase in the glands of the stomach to account for the extended vomiting of pregnancy. The sebaceous and sudoriparous glands received a notice. The work of the liver and kidneys was increased. Sugar in the urine in albuminuria occurring in pregnancy was not infrequent, and the relation of glycosuria to pregnancy was another question left for future solution. The same factors were noted in puerperal eclampsia as were present in the spasms developed in a frog by a poisonous dose of strychnine; namely, exalted centric irritability, exaltation of the irritability by a poison in the blood, and eccentric irritation.

Light might be thrown upon diseases of the eye by studying them in connection with pregnancy, and a form of amaurosis was referred to which occurred with pregnancy and disappeared when that condition passed away.

In noting the changes occurring in the lymphatic system, the unusual activity of the glands and the vessels in removing superfluous material was mentioned, and the three factors found which favored the process of blood coagulations; namely, some degree of arrest in the circulation, a peculiar condition of the blood, notably excess of fibrin, and third the introduction of some foreign material acting chemically as a precipitator of the fibrin. A field was then opened, the proper study of which might shed light upon the subject of thrombosis in general. The hemorrhages of pregnancy received a notice, and they were regarded as evidence of arterial tension, and might be only a sanitary measure. In conclusion the doctor noted the probable effects upon the nervous system in determining certain pigmentations.

On motion made by Dr. Peaslee a vote of thanks was extended to Dr. Barnes for his able paper.

The President remarked that the Society appreciated most warmly the kindness of Dr. Barnes in giving it so scientific and able a paper, and it was one so replete with suggestions in every direction in connection with general practical pathology that there would be great difficulty in discussing it. Still, respect for the paper demanded that an effort should be made in that direction, and he would therefore call upon Dr. Peaslee, who had been so distinguished a physiologist before he became so eminent an obstetrician, to open the discussion.

DR. PEASLEE, after making a few complimentary remarks, turned attention to the main proposition of the paper with the statement, that a careful study of special pathological condi-

tions which accompanied and were brought about by utero-gestation contributed largely to our knowledge in general pathology. For, when utero-gestation was going on, everything was made subservient to the development of the uterus and its contents. The functions of the body generally were modified. Ready acquiescence could be given to most of the statements made by Dr. Barnes, as "they have all been felt before, but ne'er so well expressed." Dr. Peaslee referred to the special nature of the paper as suggestive concerning certain conditions remaining after parturition, etc., and which he was pleased to denominate as *quasi* continued pregnancy. For example, when the uterus remained in a condition of sub-involution, or when it was in a condition of chronic passive congestion independent of utero-gestation, a condition was present which gave rise to more or less derangement of the liver, lungs, kidneys, and stomach, a fact which should never be lost sight of while such conditions remained. As far as effects were concerned such cases were cases of continued pregnancy. It should not be forgotten, however, that while in most instances the functions of the body were subordinated to the processes of utero-gestation, there were many cases in which the performance of the functions of the body was carried on perfectly; more so even than before the occurrence of pregnancy. The condition of the anæmic woman not infrequently was very greatly improved, and the phthisical woman might recover almost her usual strength, energy, and color; while the plethoric woman is almost always not so well when in the pregnant condition. So the proposition was not uniformly true, but as a matter of general study it was a subject of the highest possible importance.

The President then called upon Dr. W. T. Lusk, of New York, whose reputation was so well known on account of his former teachings and writings upon physiology.

Dr. Lusk remarked, with regard to the subjects which Dr. Barnes had so clearly placed before the meeting, that he was especially impressed with what had been said regarding the relation existing between malarial poisoning and pregnancy, and the history of a case was then related in which chill after chill occurred as soon as conception had taken place. The case resisted all treatment, and the chills did not disappear until parturition, after which they passed away spontaneously. Dr. Lusk further remarked that he was aware of the experiments that had been made showing that the blood became impoverished during pregnancy, but he believed that other experiments had shown that such was not uniformly the case, but on the other hand there was an actual increase in the volume of the red blood-globules. In the large proportion of cases, however, the oppo-

site condition predominated. Dr. Lusk inquired whether any experiments had been made which proved that carbonic acid gas exerted a special influence in producing abortion; whether its isolated action had been established?

DR. RICHARDSON, of Boston, referred to a case in which a peculiar pigmentation occurred in the breast after incision at the edge of the areola for mammary abscess, the pigmentation following the line of the incision. He suggested the expediency of making the incision in such cases, either quite within or entirely outside of the areola.

DR. NOEGGERATH, of New York, spoke of the similarity which existed between epilepsy and eclampsia. Dr. Barnes had alluded to the fact that external irritation would give rise to eclampsia in a frog poisoned with strychnia, which would seem to establish the existence of an aura in eclampsia as well as in epilepsy. The doctor related a case in which the application of ice to the neck immediately developed attacks of eclampsia.

DR. BARNES, in closing the discussion, remarked that the conditions Dr. Peaslee had mentioned were forcible illustrations and valuable, because they demonstrated that the true gynecologist could not in reality be a specialist; for his observation must be extended to the general system, as well as his attention directed to the organ at fault. The true gynecologists were the "all-around" practitioners. Dr. Barnes remarked that Dr. Lusk's case was a confirming illustration of the fact that many conditions were developed in connection with pregnancy which would not yield to treatment, but which did disappear after parturition. In answer to Dr. Lusk's question, several instances were cited which seemed to prove that carbonic acid gas exerted a special influence in the production of abortion. It was also a well-established fact that the gas was a stimulant to inorganic muscular fibre.

The next paper was read by DR. BYFORD, of Chicago, on

THE SPONTANEOUS AND ARTIFICIAL DISINTEGRATION OF FIBROUS
TUMORS OF THE UTERUS.

This paper contained the history of three cases. In two of them the disintegration was believed to be due to the effect produced by ergot.

Discussion was postponed until Friday morning, at 10 o'clock.

Dr. S. S. Purple, President of the Academy of Medicine, was elected member by invitation.

The Society adjourned until 3 P.M.

Second Day—Afternoon Session.

The President called the Society to order at 3 P.M., and the

first paper was read by Dr. T. GAILLARD THOMAS, of New York, on

ABDOMINAL PREGNANCY TREATED BY GASTROTOMY.

The paper contained the report of a case, with comments.¹

The discovery of the ovarian corpuscle in the fluid removed by tapping the abdomen, in the present instance, so far proved the unreliability of this aid in diagnosis. The placenta was left in situ; the external wound kept open; a drainage tube inserted; and antiseptic injection freely used, as symptoms of septicæmia developed. Within the fifth week the placenta, partially decomposed, appeared at the opening, and was removed. The child was well developed, but its death had been caused by a hair tightly encircling the funis. When the peritoneum was reached, it was found so hypertrophied that had not the doctor felt positive in his diagnosis, he probably would have been led to regard it as a peritoneum thickened from irritation and inflammation induced by the presence of an ovarian cyst, and proceeded to tear it off from the abdominal walls. Such procedure would have probably been followed by a fatal result. The following points were noted: The importance of early diagnosis; prompt surgical interference—not uniformly, but non-interference in certain cases would be criminal. No positive rule, however, with reference to the latter point could be laid down.

Dr. BYFORD, Vice-President, in the chair, called upon Dr. Barnes to open the discussion.

Dr. BARNES made some interesting remarks relating to difficulties in diagnosis, and expressed the opinion that the ovarian corpuscle was an unsafe guide. He also believed that the placenta should remain unremoved, but did not feel quite so sure that the external wound should not be closed. The propriety of opening the wound upon the first development of symptoms of septicæmia was readily recognized. If the wound be closed, might not the process of decomposition be prevented, and the placenta removed by nature in some of those ways with which we are already more or less familiar?

Dr. ENGELMANN, of St. Louis, regarded the diagnosis of ovarian cysts by the presence of the so-called ovarian corpuscle as unreliable. This opinion was sustained by Dr. Chadwick.

Dr. DRYSDALE, of Philadelphia, stated that he had never failed in making a correct diagnosis, when he had carefully applied the proper tests to the cell. He had made something over fifteen hundred examinations. Dr. Byford's experience, as far as it went, corroborated Dr. Drysdale's.

¹ See this Number, p. 658.

DR. THOMAS remarked that he must do Dr. Drysdale the credit of making a correct diagnosis in every instance in which he had sent him fluid for examination, but that the best microscopists in New York had failed in numerous instances, and he had, therefore, come to regard the ovarian corpuscle as an unreliable aid in diagnosis. With regard to the propriety of leaving an opening for the escape of the placenta, he was fully convinced that it was the proper plan to pursue. The question was not whether nature would not remove it in some of her peculiar ways, but whether it would not be safer for the woman to have an opening for the discharge of detritus. He regarded the conclusion unsafe, that when the symptoms of septicæmia began to develop, an opening might then be made, for we could not be nearly so certain of reaching the *locus* from which poisonous material was passing out into the general system.

The discussion being closed, DR. H. F. CAMPBELL, of Augusta, Ga., presented the substance of a paper on

PNEUMATIC SELF-REPLACEMENT IN DISLOCATION OF THE GRAVID UTERUS.

The factors in Dr. Campbell's theory are: 1, inversion of gravity; 2, draught of the viscera; and 3, external *atmospheric pressure*.

The patient places herself in the knee-and-chest position, introduces a glass tube into the vagina, and the atmospheric pressure restores the displaced organ to its normal position. The doctor related several cases illustrative of this plan of treatment.

DR. EMMET spoke favorably of the method, and regarded the glass tube as an important acquisition in carrying it out.

DR. MUNDÉ remarked that, independent of any knowledge of observations made by Dr. Emmet or Dr. Campbell, he had effected replacement in one case by introducing a speculum into the vagina and then lifting up the perineum. His manipulations were not made originally for the purpose of effecting replacement by atmospheric pressure, but restoration of the uterus to its natural position occurred by that means while they were being made.¹

DR. A. H. SMITH, of Philadelphia, was unable to see what advantage temporary replacement by this means had over permanent replacement and retention by a properly fitting pessary.

DR. MUNDÉ had also found it necessary, in large vaginæ and with heavy uteri, to give the body of the uterus an additional impulse, either by slightly pushing it up or by pulling up the neck with a tenaculum, perhaps by both, and his experience in that direction had led him to believe that there were cases in

¹ See this Journal, June No., 1876, p. 292.

which gravity and atmospheric pressure combined failed, unless aided by manual impulse as above described.

The paper was discussed at some length by Drs. Chadwick, Peaslee, and Wilson.

DR. CAMPBELL remarked, in closing the discussion, that he had not introduced this plan of replacement expecting it would take the place of the pessary, for he always carried those instruments in his pocket, but he had simply introduced it that patients might have some method at their command for effecting temporary replacement without being compelled to call in a physician. He regarded it as a method possessing special value for overcoming displacements in virgins.

After certain announcements, the Society adjourned, to meet at 10 A.M. on Friday morning.

Friday, Sept. 15th. Third Day—Morning Session.

The hour of ten having arrived, the Society was called to order by the President, and Dr. Edward Wilson, of Philadelphia, elected member by invitation.

The discussion of Dr. Byford's paper being the regular order of business,

DR. WASHINGTON L. ATLEE, of Philadelphia, was introduced, and remarked that he should confine what he had to say to the natural efforts of the womb to disintegrate fibroid tumors. He was of the opinion that disintegration was confined to the submucous variety. In that form there was no doubt but that the capsule and mucous covering sometimes became inflamed and ulcerated, and that the uterus, either from stimulation or naturally, forced the tumor from its capsule into the cavity, and so out into the external world. The vitality of such tumors was not great, and when exposed to the atmosphere by rupture of the capsule, they took on a species of decay, peculiar in character, like the decay of fruit, extending from one point to another, like the dry-rot in wood, until perhaps the greatest portion of the tumor had undergone such change. Many years ago he had adopted for this change a term given us by Liebig, namely, *eremacausis*. When that condition was developed the tumor should be expelled rapidly or removed quickly by some artificial means, for if permitted to remain there was great danger of systemic poisoning by absorption. It was regarded as a matter of great importance to remove the mass as soon as possible, and also to guard against ill effects upon the general system by means of antiseptic injections. The action of the uterus, without doubt, could be very much facilitated by the use of means calculated to stimulate uterine fibre, and the most

efficient of all such agents was ergot. A brief allusion was made to certain cases in which uterine fibroids had complicated parturition. Dr. Atlee fully endorsed what Dr. Byford had said in his paper.

DR. GOODELL, of Philadelphia, referred to a case in which a fibroid, situated at the fundus, had complicated the puerperal condition by the profuse hemorrhage it induced. The woman was reduced in the extreme, but by perseverance in the use of ergot, and measures calculated to preserve her strength, complete convalescence occurred at the end of three or four weeks, and at the end of that time no tumor could be detected.

Another case was related in which the tumor was situated at the fundus, and, it being found impossible to enucleate it, a small incision was made through the capsule. Ergot was then administered and the tumor escaped in fragments, so that at the end of six weeks it was much reduced in size, and the woman had ceased to suffer from hemorrhagic attacks, which previously had been very severe.

DR. DRYSDALE, of Philadelphia, called attention to the use of large doses of ergot in this class of cases, and stated that he had administered $\frac{3}{4}$ ss. doses of Squibb's fl. ext. three times a day for more than a year without producing any deleterious effects.

DR. EMMET remarked that there were many cases of fibroids in which ergot did more harm than good, because the tumors were so situated that gravity could not aid in their expulsion. If gravity could not be brought to bear, the action of the ergot, so far as expulsion was concerned, would be inert.

DR. THOMAS remarked that the action of ergot under these circumstances was two-fold, and that the two should not be confounded. One unquestionably was that of causing expulsion of the tumor aided by gravity, and was available under circumstances where the tumor could be forced into the uterine cavity.

On the other hand, Hildebrandt had shown that there was an entirely different action of ergot from the expulsive. It was an action which the drug exerted upon blood-vessels in general; such an influence as it exerted in controlling hemorrhages of various kinds, and was particularly potent upon interstitial tumors. Such action was by no means secondary in importance to the expulsive action of the drug, and should not be overlooked.

DR. WILSON inquired whether the use of half-ounce doses of the fl. ext. of ergot for months gave rise to pain that required the use of anodynes?

DR. DRYSDALE replied that when the pain became too severe

the drug was discontinued, and then renewed when the pain had subsided.

DR. E. NOEGGERATH, of New York, then read a paper on

LATENT GONORRHOEA, ESPECIALLY WITH REGARD TO ITS INFLUENCE
ON FERTILITY IN WOMEN.

In this paper it was maintained that gonorrhœa, when once contracted, persisted for life in certain portions of the male genital organs, notwithstanding its apparent cure. It was believed that that fact explained, to some extent at least, why uterine disease was so prevalent in our large cities; why blooming girls faded so soon after marriage; why treatment of certain cases of uterine disease so often failed; and also why sterility was so prevalent. The germs of the disease remained concealed, and when the proper conditions were presented, it burst forth in all its virulence, and was followed by the disastrous consequences to which allusion had been made.

Dr. Noeggerath's paper was criticised sharply by Drs. Engelmann, of St. Louis; Chadwick, of Boston; Trenholme, of Montreal; and Johnson, of Washington.

DR. ENGELMANN, however, remarked that he had noticed, at post-mortem, evidences of salpingitis in numerous cases which to him had heretofore been unexplained, nor had he attributed much value to them, but which, perhaps, could be explained in accordance with the theory set forth in Dr. N.'s paper. That condition had been seen especially in those cases in which gonorrhœa had been present at some previous time.

The Secretary then read a paper that had been received from DR. ALFRED WILTSHIRE, of London, England, on

DEATH FROM URINÆMIA IN CERTAIN CASES OF MALIGNANT
DISEASES OF THE UTERUS.

The paper was discussed by Drs. Parvin, Campbell, Skene, Wilson, and the President.

DR. G. J. ENGELMANN, of St. Louis, presented the abstract of a paper on

MENSTRUAL HYSTERO-NEUROSIS OF THE STOMACH.

On motion, the Society resolved that it did not hold itself responsible for any views expressed in papers that might appear in the volume of Transactions. They were to be regarded simply as the views of the authors.

The Society then adjourned to meet at 3 P.M.

Third Day.—Afternoon Session.

The Society was called to order by the President. The first order of business was the report of a case by DR. PEASLEE of

SOLID UTERUS BIPARTITUS; BOTH OVARIES REMOVED FOR THE RELIEF OF EPILEPTIC SEIZURES BELIEVED TO DEPEND UPON OVARIAN IRRITATION.

Dr. Peaslee's operation was performed through the abdominal wall. Fatal peritonitis ensued. The doctor, however, would not hesitate to recommend the same operation in another case surrounded by the same circumstances.

DR. TRENHOLME, of Montreal, reported a successful operation for the removal of the non-enlarged ovaries in two cases. One was suprapubic and the other vaginal.¹

DR. THOMAS reported three cases of so-called "normal ovariectomy." In the first, the ovary was removed through the vagina, and the patient recovered without serious symptoms. In the second case the ovaries were removed through the abdominal wall, and the patient made a good recovery so far as the operation was concerned, but the general condition of the patient was not improved. In the third case, an attempt was made to remove the ovary by way of the vagina, but old adhesions prevented the completion of the operation in that direction, and the operation by abdominal section was then performed. The patient died of acute peritonitis at the end of fifty-six hours.

DR. NOEGGERATH remarked that the microscopical examination of the ovaries in Dr. Thomas' second case disclosed the presence of Pacinian corpuscles.

Now, if the ovarian nerves undergo such remarkable change, other nerves in the pelvic cavity may be affected by the same change, and, perhaps, that might explain why improvement did not in some cases follow the operation for the removal of the ovaries.

DR. PEASLEE, in closing the discussion, remarked that he wished to be understood as disapproving of normal ovariectomy, as generally understood by that term. In thus recommending the operation of removal of the ovaries, it would not be to avert mere physical suffering, but to prevent the woman from becoming idiotic, as in this case, through mental disturbances.

DR. EMMET remarked that he would not sanction the operation only in those cases in which epilepsy was present.

DR. GAENSLER, of Texas, was made a member by invitation.

¹ See this Number, p. 703.

DR. W. GOODELL, of Philadelphia, then read a paper entitled

CLINICAL MEMOIR ON SOME OF THE GENITAL LESIONS OF CHILD-BED.

In this paper the objections commonly urged against early closure of perineal lacerations were first considered, and the conclusion arrived at that the condition of the woman was very much improved by closing the rent at once, and by means of silver-wire sutures. With regard to the best treatment for rupture of the recto-vaginal septum, Dr. Goodell was not so certain. Many such ruptures escaped notice, because they closed spontaneously.

Drs. Emmet, Campbell, Wilson, and Skene favored immediate closure of perineal lacerations by means of silver-wire sutures.

Dr. Jenks remarked that he should not expect union to occur in cases where considerable manipulation of the parts had been necessary to effect delivery, if the parts were brought together immediately after the occurrence of the accident; but when the labor had been rapid, whether from forceps or naturally, good union had been invariably secured.

Dr. HOWARD remarked that he had been more successful with the primary than the secondary operation. His cases had also healed perfectly when united two or three hours after the accident, and if he came in contact with a case twenty-four hours after the occurrence, he would not hesitate to unite the wound with sutures. In a majority of cases which he had left to nature and trusted to position to bring the edges of the laceration in contact, union had not taken place.

THE PRESIDENT remarked that, should he attempt to discuss the question under consideration, he should be compelled to differ from the majority of opinions that had been already expressed.

THE PRESIDENT also acknowledged the receipt of a letter from Dr. Sims, in which he stated that it was one of the great disappointments of his life that he had not been able to be present. He had not yet sailed from Europe.

The following gentlemen were elected HONORARY MEMBERS:

United States—Joseph A. Eve, Augusta, Ga.; M. B. Wright, Cincinnati, O.; Great Britain—Robert Barnes, T. Spencer Wells, London; Thos. Keith, Edinburgh; A. H. McClintock, Dublin; Germany—Gustav Simon, Heidelberg;¹ Karl Schroeder, Berlin; France—E. Koeberlé, Nancy.

¹ The sudden and lamented death of Prof. Simon, of aneurism of the aortic arch, on August 28th, last, unfortunately deprives the Society of his valued co-operation, and medical science of one of its most gifted and active workers.

The following papers were read by title :

Battey's Operation for the Removal of the Ovaries, by Dr. R. Battey, of Rome, Ga.

Hydrate of Chloral in Obstetric Practice, by Dr. W. L. Richardson, of Boston.

A Case of Labor complicated with four large Uterine Fibroids and Placenta Prævia.

Also a paper on Masturbation in Women, with a report of seventeen cases treated with Bromide of Potash, by Dr. J. R. Chadwick, of Boston.

A paper from Mr. Lawson Tait, of England.

Cases of Cystic Tumors of the Pelvis, by Dr. G. H. Bixby, of Boston.

What is the History of Calculi formed in the Bladder after Operations for Vesico-Vaginal Fistulæ? by Dr. H. F. Campbell, of Augusta, Ga.

The Officers of the Society for 1876 were re-elected, viz. : President, Dr. Fordyce Barker ; Vice-Presidents, Drs. W. L. Atlee, W. H. Byford ; Secretary, Dr. J. R. Chadwick ; Treasurer, Dr. Paul F. Mundé ; Council, Drs. J. Marion Sims, Wm. Goodell, T. Parvin, Geo. H. Lyman.

The hour of adjournment having arrived, the President, with appropriate remarks, declared the Society adjourned to meet in Boston on the last Wednesday in May, 1877.¹

[For the benefit of gentlemen contemplating joining the Society, we append the following extracts from the Constitution and By-Laws :

" III. The Fellows shall not exceed sixty in number."

" Candidates for Fellowship shall be proposed to the Council one month before the first day of meeting, by two fellows, and on recommendation of the Council shall be balloted for at the annual meeting."

" A two-thirds affirmative vote of all the Fellows present, shall constitute an election."

" No one shall be eligible for active Fellowship until he shall have submitted to the Council a paper on some subject connected with gynecological science."

" VI. The titles of all papers to be read at any Annual Meeting shall be forwarded to the Secretary (Dr. James R. Chadwick, Clarendon and Marlborough streets, Boston), not later than one month before the first day of the Session.

No paper shall be read before the Society which has already been printed, or been read before another body.

All papers that may be read before the Society, and accepted for publication, shall become the property of the Society, and their publication shall be under the control of the Council."

EDITOR.]

¹ A full report of the proceedings of the Society will be found in the volume of Transactions already in press.

QUARTERLY REPORT ON OBSTETRICS, GYNECOLOGY, AND PEDIATRICS.

BY

DRS. D. B. HUNT AND C. WILLIAMS.

OBSTETRICS.

1. PERNICIOUS MALARIAL CARDIAC NEUROSIS OF PREGNANCY TREATED BY LARGE DOSES OF QUININE, WITHOUT DANGER TO MOTHER OR CHILD. By DR. EDOUARD BURDEL. (*Annales De Gynécologie*. May, 1876.)

IT has been claimed that quinine produces abortion, and that it may be used as an oxytocic. The researches of Dr. Burdel¹ seem to prove that no such special properties belong to quinine, and that any such result from its exhibition is far from constant.

He emphatically declares that quinine, above all, is par excellence a nervo-osthenic tonic, acting in a special manner through the vaso-motor nerves upon the smooth muscle fibres; and it is not extraordinary that after a long travail, the woman exhausted, the uterus fallen into a kind of inertia, quinine should revive the organism, and that the uterus, stimulated also with other organs, continues its mission of expulsion.

The author has several times in atony of the bladder, with incontinence or retention, relieved the condition with quinine. (It is understood that the disorder was functional, without any alteration of the organ itself). Also in passive hemorrhages, in hæmoptysis, hæmatomesis, etc., etc., has he caused the cessation of the accident by the same means, that is, by an awakening of the vaso-motor system, which is dulled or enfeebled.

Exceptions occur where, because of an idiosyncrasy, quinine exerts a subversive and perturbative influence. Some cases were noted when a very small dose produced veritable symptoms of intoxication; others, again, were given very large doses, with scarcely the production of its therapeutical effects.

In the disease in question, the cardiac and the vaso-motor nerves are principally affected. This terrible disease is sometimes mistaken for angina pectoris, or embolism of the heart. It only reveals itself by extreme rapidity of the heart beats, giving to the hand or ear the sensation of a dull and continuous vibration. The respiratory movements become frequent, but not anxious. In some cases there is an expression of extreme anxiety; in others the countenance is calm and serene. The skin at first is hot, dry and burning, becomes suddenly cold and humid, and again dry and hot.

Death ends the scene more or less quickly, unless the disease is opposed by the energetic exhibition of quinine.

The author desires to impress upon the practitioner the fact that all diseases of a paludal nature do not take the form of periodicity. He says that those who have not had the most abundant opportunities of observing and studying the multiform functional disorders dependent upon telluric poisoning

¹ Vid. Ann. de Gyn., vol. 1, p. 437.

of the vaso-motor system, can form no adequate idea of the disturbances which result from it. Action must be prompt and efficient, else death will remove the necessity for it.

CASE I.—Madame X., aet. 28. Sixth pregnancy, general health always good; residing in a chateau situated in the middle of a park, which was inundated through winter and part of the spring.

The author was summoned to see the patient, who had returned fifteen days before from Normandy. She was found panting for breath and oppressed, but attributed her distress to her approaching confinement.

The doctor was about retiring, having ordered only rest, when, upon feeling the patient's pulse, he was surprised and alarmed to find it very feeble and too rapid to be counted, appearing as a vibrating cord. Over the heart could be heard only a dull and rapid vibration. Skin hot, with slight moisture. On the next day Madame X. was delivered by a midwife, before the doctor's arrival. Upon examination of the pulse, it was found that all the symptoms had increased in gravity. Patient was perfectly lucid and cheerful. Dr. B. prepared to administer a large dose of quinine, but as an hour had hardly elapsed since the delivery, he concluded to wait a little, when profuse vomiting and diarrhoea set in, and the doctor, not having a hypodermic syringe with him, it was impossible to give the quinine.

Two eminent physicians saw the patient in consultation, but their prognosis was that death was inevitable, which indeed ensued a few hours afterwards.

CASE II.—Mme. Y., in the eighth month of gestation. Her property, upon which she was living, though in the same region of country as Case I., was not exposed, except part of the time, to malarial influences.

She had hardly been installed six days at the villa when she was suddenly seized with malaria, feebleness, and syncope. She could hardly stand erect without being much oppressed, and feeling as if her legs were giving way under her. Her physician, supposing that the accouchement was nearer (not without reason) than expected, ordered rest and anodynes. On the morrow the case was so much aggravated that the family insisted upon calling Dr. B. in consultation. He found Mme. Y. entirely lucid, but much enfeebled. Pulse filiform, could not be counted, heart appearing to tremble rather than beat. In the midst of this confused vibration coming from the heart and great vessels, the pulsation of the foetal heart could not be distinguished, and only convulsive movements in the form of tremors announced that the child was not yet dead.

As in the previous case, there was neither intermittence or remission, or chills, or sweats, but the enfeeblement was such that the family called the priest, thinking death very near.

Dr. B. then gave six hypodermic injections in the pectoral region of quinine dissolved in brandy (one gramme to brandy six grammes). After the first injection the countenance, which was pale, became colored, and the patient cried with pain; the pulse became more sensible, and after the sixth injection, could be counted (175). An hour afterwards, four injections were given containing about 0.80 centigrammes.

A half an hour after the last injection the patient slept about an hour, and upon awaking the body was covered with sweat, the physiognomy was better, pulse not more than 150, and a cup of soup with some wine was taken with pleasure.

On the next day the pulse was 130. Nevertheless, four injections were given containing one gramme of quinine; and in the evening about 0.70 centigrammes more in two injections.

Forty-eight hours after the pulse was 92; the patient demanded food, and upon auscultating at the abdomen the foetal bruit was heard. The injections were then stopped, and for several days only a small glass of "vin de séguin" was given three times a day. Twelve days after Dr. B.'s first visit Mme. Y. was safely delivered, with considerable flooding, though easily checked.

Five days afterwards she had an access of intermittent fever, which was

promptly cured with 60 centigrammes of quinine three times a day by the stomach.

The administration of the quinine was not approved by the physician in attendance in this case, or by the family, and was looked upon as more than foolhardy; but after success had crowned his efforts, Dr. B. was enabled to cry with the poet:

Audaces fortuna juvat.

The article closes with the declaration of the author, of his conviction of the innocuity of quinine upon the uterus, during gestation, even though parturition be near. C. W.

2. ON RUPTURE OF THE PERINEUM, ESPECIALLY ON THE DIVISION OF THE PERINEUM AND RECTOVAGINAL SEPTUM, BY POST PARTUM SLOUGHING. BY J. MATTHEWS DUNCAN, M.D. (*Edinburgh Med. Jour.*, April, 1876.)

The author has met three examples of central laceration of the perineum, one of which had been treated by immediate stitching; in the other two he operated long after the occurrence; both easily healed.

Dr. Duncan desires to call particular attention to a class of cases in which the parts are entire after complete delivery—it may be for days—conditions being produced apparently identical with those which result from direct laceration during the second stage of labor.

In the first case, after easy delivery by forceps, the fourchette not having been torn, a spot about three-fourths of an inch in diameter was observed, slightly livid, and prominent midway between the fourchette and anus.

The tissues of the vagina were deeply lacerated above the prominent part of the perineum, and the persistent textures, including the skin, were very thin at this point.

The discharges always appeared to be healthy and without fœtor. On the ninth day, in the middle of the swollen part was an opening about one-eighth inch in diameter, admitting a small cedar pencil into the vagina; it healed before death. On the twelfth day after delivery, peculiar pyæmic symptoms manifested themselves, and the patient died fifteen days afterwards. She had been a fine, healthy woman.

In the second case, patient æt. twenty-eight. Primipara; ill-made, four feet eleven inches in height; pelvis contracted, rickety; sacrum projecting; brim reniform, with scarcely two and one-half inches in conjugate diameter.

The labor lasted about thirty-six hours. The waters had been discharged about a day before labor began.

Perforation was done after podalic version, and no difficulty met in completing the labor, after the base of the skull had passed the brim; child weighed six pounds ten ounces. The perineum was lacerated, but as there was nothing apparently peculiar about the accident, the part was not particularly examined at the time.

On the day following delivery, a linear fissure of the skin, apparently not through the corium, extended to the verge of the anus, whose sphincter was entire.

The extent of the split skin was half an inch; the vagina was entire for at least one-quarter of an inch farther forward than the entire part of the perineum including the split skin. Wound looks healthy, but around it and the labia there is redness as seen over diffuse cellular inflammation; urine acid, albuminous, has to be drawn off. The history of the case for four days was some fever, slight jaundice, vomiting, albumen disappeared, perineum looking well. Then for five days the perineum was not examined; at the end of that time it was inspected, and found to be completely divided in its whole length, the fissure extending through the sphincter, and for an inch beyond the verge of the anus. The wound was clean, but small portions of soft slough were observed about it.

Except this fissure, no appearances of disease were to be seen. The further history of the case is of no special interest. C. W.

3. HYOSCYAMIN IN THE CHOREA OF PREGNANCY.

DR. OULMONT treated five cases of chorea with Merk's hyoscyamin.

The patients were from twenty to thirty years of age. Three of them were either at the beginning or near the end of pregnancy.

One case, a woman in the second month of her pregnancy, became affected with chorea; it had existed a month. Valerian and potassium bromide were without avail.

She was given, morning and night, 0.002-0.004 gramme hyoscyamin; four days afterwards material benefit was observed. The administration of the hyoscyamin was interrupted by the appearance of intoxication symptoms, (redness and dryness of the throat;) the chorea appeared again as severely as before. After ten days' intermission of the hyoscyamin treatment, it was again renewed, and the dose gradually increased from 0.004-0.008 gramme; the improvement appeared again immediately.

Ten days afterward the intoxication again appeared, but the use of the drug was persisted in till the choreic symptoms disappeared, ten days later.

The chorea was associated in this case with hysteria.

In a second case the chorea had lasted eighteen months, during which time the patient had been twice delivered.

After five days' use of the hyoscyamin, a striking improvement was manifest; during an eleven-day intermission the disease became worse, and again there was improvement three days after its resumption; and ten days after, when the patient left the hospital, the disorder was nearly cured.

In three other cases the hyoscyamin was used with good result. The dose of 0.01 gramme per diem was in no case overstepped. The beginning dose was 0.001 gramme morning and night, and the improvement usually appeared on the eighth or ninth day; in one case, on the fifteenth day; and in one very inveterate case, not until the twentieth.

C. W.

4. THE PHYSIOLOGICAL CONDITION OF THE URINE DURING THE NORMAL PUERPERAL STATE. (*Arch. f. Gyn.* IX. 3.) PROF. LUDWIG KLEIN-WACHTER, of Prague, details his extended investigations on this but little discussed subject, and arrives at the following conclusions:

The quantity of urine during the whole lying-in period exceeds the average, particularly on the first day; from the second to the fourth day it diminishes, a decrease attributable to the drain of fluid caused by the lacteal secretion, which has become established during that time, to the usual profuse puerperal diaphoresis, and the lochial discharges, and thereafter the amount increases and remains at about the same height until the eighth day. The absolute excretion of urea corresponds very nearly to the normal proportion. On the first day post-partum it is somewhat diminished, on the second it falls, on the third and fourth days it is most considerable, and then gradually returns to the normal average.

The absolute amount of chloride of sodium is exceedingly variable, following closely as regards its daily excretion the amount of urine. The absolute quantity of phosphoric acid is diminished. On the first day its excretion increases, on the second and third days it diminishes, and on the following two days an increase not reaching the normal average ensues. On the last three days of puerperality, the excretion of phosphoric acid falls below the average. The urine is clear, pale yellow on the first day, later bright yellow, on the last day yellow. The average specific gravity is 1.015 to 1.016, and generally is in inverse proportion to the quantity of urine. With increasing age the daily amount of urine, and the absolute quantity of chloride of sodium and phosphoric acid falls, the color of the urine deepens, the specific gravity rises. The excretion of urea does not depend so much on the advance in age as on the period of sexual activity or reproductive life, during which it is most considerable. Before and after this period it decreases. The duration of uterine contractions (labor) manifests itself in a temporary increase in the

quantity of urine, but the excretion of the three constituents above mentioned is not affected by them. Commencing lactation is accompanied by a diminution in the quantity of urine, as well as the relative and absolute amount of urea. The commencement of mammary activity scarcely, however, appears to influence the discharge of phosphoric acid, and the excretion of chloride of sodium is affected by it only to a very inconsiderable degree.

5. CASE OF EXTRA-UTERINE FETATION, SUCCESSFULLY TREATED BY ABDOMINAL SECTION. By JOHN A. MACDOUGALL, M.D. (*Edinburgh Medical Journal*, July, 1876.)

Elizabeth Fell, æt. 38, mother of eight children, shortly after a menstrual period, in the early part of January, 1874, had symptoms which led her to believe that she was again pregnant. The progress of her pregnancy seemed to her entirely natural, with these exceptions: the abdominal enlargement was noticed first upon the left side; the foetal movements were early of a pronounced character, and later so violent that she dreaded their occurrence; at mid-term there was a sanguinolent discharge, freer than her usual menstrual period, which ended in the passing of a considerable quantity of blood clot. At no time was there marked abdominal pain.

After a hard day's work at the end of August, she was seized with vomiting, which lasted for some time. Labor pains supervened, and after sixteen hours of much suffering, she had two strong expulsive pains, with bloody discharges from the vagina; after this, there was no suffering; foetal movement ceased, and for the next four days she was able to move about. The doctor, who had visited her repeatedly during this attack, recognized the pains as "false." The abdominal swelling from this time diminished; the distended breasts became flaccid; menses reappeared at the end of September, and all signs of approaching childbed passed away.

From the date of this "spurious labor" until the latter end of November, 1875 (fifteen calendar months), she attended to her household duties without complaint of any kind. Then came a time of great mental and physical strain followed by exhaustion.

December 8th, the case was pronounced to be a "suppurating extra-uterine cyst," probably connected with the left ovary. There was a fetid discharge from the vagina. Condition was anxious, pulse quick, temperature 102° , much pain and tenderness in abdomen. Under the treatment by poultices, free use of opium, and vaginal injections of a warm carbolic lotion the pulse improved, temperature fell, tenderness disappeared, so that, after three weeks (although contrary to advice), she went home.

Upon examination six weeks after this, there was a small opening at the umbilicus, which upon pressure gave exit to a few drops of pus. She had lost much flesh, and septicæmia was recognized. She was again received into the hospital, and on the 18th of February, the following operation was done:

Beginning the incision just below the fistulous opening at the umbilicus, I carried it down for about four inches in the centre line; dissecting carefully down upon the cyst I opened it at the upper extremity of the wound, and finding on introducing my finger that the wall was firmly adherent to the parietes, I laid it open to the full extent of the superficial incision. Much sickly smelling pus escaped, and on gently introducing my hand, I found the body of the fœtus greatly disintegrated. Many of the bones, notably those of the cranial vault, were lying loose in the cavity, and were easily removed by means of a lithotomy forceps. Fortunately, the lower extremities remained intact, so, seizing the feet, I turned and readily removed the mass. This, when examined, proved the remains of what had been a well-developed male child, and was estimated to weigh four pounds.

All trace of the placenta had gone. A peculiar nodulated condition of the anterior wall of the cyst alone marked its site. The interior of the cyst was thoroughly sponged out with a weak solution of iodine. A funnel-shaped opening was discovered, which evidently communicated by means of a short

tube with the body of the uterus. A small-sized catheter, passed gently along this, found its way into the vagina through the os uteri. By means of this opening, a drainage-tube was carried from below into the cavity of the cyst. Two deep silver sutures were then inserted, so as to bring together the wound in its entire depth, and two passed superficially through the skin. She rallied quickly from the operation, and her progress towards recovery was steady and uninterrupted. Carbolyzed solutions were injected daily through the drainage-tube.

March 14, tube was withdrawn. March 24, external wound healed, and the track through which the drainage-tube was carried has evidently closed; all discharge has ceased. March 26, out of bed, and "very well."

In reviewing the case, the author calls attention to the absence of the usual train of symptoms, especially the sudden cramping pain in the abdomen, recurring often and without appreciable cause, and remarks that the history of the case rather bears out the belief entertained by him "that the case narrated was one of ovarian pregnancy." Left-sided ovarian pregnancies have been found most frequent. She had no abdominal pain, and she carried her child to and beyond term—circumstances which one does not often find associated with tubal pregnancies—and more than all, at the operation it was readily demonstrated that a tube, nearly two inches long, passed directly from the sac and entered the uterus near its fundus. This the Doctor believes to have been the Fallopian tube.

A deteriorated condition of the general health, dependent as in this case, upon severe mental and physical strain, may lead to changes in the vital condition of the sac, and bears upon the question of primary operation. By the use of the thermometer, we must watch for evidences of septicæmia, which if discovered, demand immediate interference.

In cases where the placenta is vital and unchanged, Macdougall thinks it advisably left *in situ*, and in reporting at length the case of Dr. Thomas, remarks that "good as the operation seems, and admirable as regards its success, had the placenta been left untouched, the recovery would, in all probability, have been safer and more rapid."

D. B. H.

6. ON THE STYPTIC ACTION OF CHLORIDE OF IRON, WITH SPECIAL REFERENCE TO INTRA-UTERINE INJECTIONS. By DR. GOLDSCHMIDT. (*Berlin Obs. Soc. Allg. Med. Central Ztg.*, Feb. 23, '76.)

At a meeting of the Obstetrical Society of Berlin, Dr. G. called attention to the need of experimental researches, with reference to the *distinct* modus operandi of various substances used for intra-uterine injections. He reported at considerable length four cases of endometritis with loosened and hemorrhagic mucous membrane, in the treatment of which he had had abundant opportunity to confirm the styptic action of liq. ferri sesquichlor.

In considering the objections made to the use of this substance, he observed as follows:

1. Liq. ferri sesquichlorati is not a powerful caustic. If a drop be placed upon the conjunctiva or lip of a rabbit, it leaves a yellow spot for two or three days; but produces no inflammation. After subcutaneous injection, a piece of skin was raised, and a brown discolored surface found.

2. That this substance penetrates into the tubes is very improbable, since by its astringent action the openings are still more contracted. The reported cases he does not consider demonstrative.

3. It does not exert its styptic action by the formation of thrombi in the vessels, but the chloride of iron coagulates the blood by diffusion, and exerts an astringent action upon the walls of the vessels, by coagulating the albuminous bodies they contain. Chloride of iron, even in weak solution (1 to 1000), added to defibrinated blood, changes it into a moderately hard, crumbly mass, like cheese or thick pus. One drop injected into the jugular vein of a rabbit killed the animal at once. The coagulum could be followed from the point of puncture to the heart, and further into the minute branches of the pulmo-

nary artery. From an injection into the femoral artery, temporary anæsthesia and paralysis were noticed. The clots were not washed away.

Since chloride of iron, although frequently used in practice, has never been followed by these evil results, we can assume, *a priori*, that it does not exert its styptic effect by direct contact with a bleeding vessel.

If a drop is placed upon the web of a frog, and the effect observed under a microscope, it appears to be as follows:

a. Irritation.

b. Relaxation of vessels.

c. Diffusion and formation of thrombi.

d. Constriction of the vessels by coagulation in the albuminous bodies in their walls.

Dr. G. recommends, during injection of the uterus, that pressure be made upon it from outside, in order to prevent the possible entrance of the substance into the open vessels.

4. The objection that liq. ferri forms lumps with the mucous contents of the uterus, and that these prevent its further action, is sufficiently refuted by experiencing its favorable styptic action. D. B. H.

7. ON THE HYPOTHESIS THAT THE HUMAN OVARIES CONTAIN MALE AND FEMALE OVA. By PROF. MAYRHOFER, of Vienna. (*Arch. f. Gyn.* IX. 3.)

In a short paper M. disproves, as he thinks, conclusively, the opinion advanced by Schultze in 1854, and recently supported by Ahlfeld (*Arch. f. Gyn.* p. 254), that the sex of the child is determined by the presence of male and female ova in the human ovary, the order of maturation and impregnation of which is a matter of chance. M. believes that the sex is determined solely during conception by a vital interchange of quality between the ovum and semen. A number of facts can be explained only by assuming that the human ovum during conception under all circumstances seeks to enforce the female sex, and the semen with equal persistence the male. Whether the product of conception becomes a boy or a girl depends upon which of the two contestants, the semen or the ovum, gains the ascendancy. These facts are the following:

1. The influence of the relative ages of the parents on the sex of the children is proved beyond doubt by statistical researches. Hofacker, Sadler, and Gohlert found that in those marriages in which the father was older than the mother a considerably larger proportion of boys were born; but that when the parents were nearly of the same age, or the husband was younger than the wife, the number of girls largely predominated. The excess of the male population in Germany is thus explained by the fact that in that country, as a rule, the male sex marries later in life than the female.

Therefore, the man as well as the woman takes part in the determination of the sex, and the human ovaries neither contain male or female ova, nor does the semen contain male or female spermatozoa.

In proof of the definite decision of the sex *during* and not after conception (that it does not occur *before* conception has been settled above), three observations taken from breeding animals, are quoted:

2. The degree of nutrition (physical development) of the male and female mammal appears to exert an influence on the sex of the fœtus. Martegoute found that those sheep which brought forth female lambs possessed an average greater weight than those delivered of rams.

3. Preservation of the semen by infrequent coition probably favors the formation of the male sex during conception. Thus, at the sheep-breeding establishment of Blanc in France, it was annually observed, that during the beginning of the breeding season, so long as the ram still possessed his full vigor, more male than female lambs were produced; as soon as the number of ewes in rut rapidly increased and the ram gradually exhausted his strength, female lambs predominated, to make way again for an excess of male lambs when the height of the season passed off and the powers of the ram were less taxed.

4. It is also exceedingly probable that mammals, cows, for instance, which have become impregnated during the early part of oestruation, more frequently bear female, others impregnated near the end of oestruation, male calves (Thury). This fact—that the time of impregnation, whether at the beginning or end of heat, exerts an influence on the sex of the fœtus—needs additional confirmation. It may be explained as follows: The longer the ovum has been separated from the ovary, and the nearer it, therefore, approaches its dissolution, the more it loses vigor to assert its sex during conception.

An interesting and important question for investigation bearing on this point, would be to ascertain the proportion of male and female children in orthodox Jewish families. If the supposition be correct, that among the children of *orthodox* Jewish parents there are more boys than among the children of Christian parents, the reason would probably be found in the law, which declares the woman unclean during thirteen days from the beginning of menstruation. The passage in question is in the Talmud, III., Book of Moses, Chap. 15, and reads at v. 19 as follows: "If a woman have a bloody flow, blood is the discharge from her pudenda; she shall then remain seven days in retirement, and whosoever shall touch her shall be unclean until evening." And further, v. 28: "And when her bloody flow ceases, she shall wait seven days, and then she will be clean."

The similarity of sex of twins springing from the same ovum, which was used by Schultze as a proof of his theory, is to be explained differently, as follows: Two very similar germinative vesicles in the same oölemma are necessarily aroused to development by the same semen; were they materially different, the twins would needs be of different sex, unless the influence of the mother on the sex of her child be entirely denied.

Hippocrates already believed that the man and the woman both took part in the determination of the sex of the child; that the meeting of the male and female semen develops the germ. Substitute "ovum" for female semen, and we have almost the same idea as the one advanced above. In contradiction of Hippocrates' belief that because the man is the stronger of the two, he must be formed out of the stronger semen, Ploss correctly says that the man is only stronger in a muscular sense, the woman, however, is shown by experience to possess a more tenacious vitality.

An interesting fact, that the daughter occasionally resembles the father, and the son the mother, proves that the parents may so divide the transmission of their peculiarities among themselves, that one party transmits the gender, the other the general physical resemblance. P. F. M.

This same topic is briefly discussed in a paper on

8. THE EXCESS OF MALE CHILDREN BORN OF OLD PRIMIPARÆ. By DR. F. AHLFELD, of Leipzig. (*Arch. f. Gyn.*, IX. 3.)

In a paper written several years ago (*Ibid.*, IV.), A. had already pointed out the singular fact that a greater number of male children are born of primiparæ at an advanced age. In the present article he makes use of a dissertation by Dr. Schramm, of Braunschweig, who collected the records of 16,346 births as a basis of his investigations, 1,038 of which occurred in old primiparæ. Of the 1,055 children born of these latter, 584 were boys, 471, girls = 124:100, thus demonstrating the excess of the male sex. A further table proves that the older the primiparæ are, the greater will be the excess of male children born to them, the proportion advancing at 35 years as high as 265.4 boys to 100 girls. The average proportion in women above 32 years of age (that being the age at which A. began to count), drawn from the researches of Schramm, Hecker (*Ibid.*, VII.), and Ahlfeld, is 139.8 boys to 100 girls.

Ahlfeld also examined the records of 500 marriages, in which the ages of husband and wife differed at least by ten years. In 439 marriages, in which the husband was older than the wife, 682 boys and 694 girls were born, thus 98.2 boys to 100 girls. Of 61 marriages in which the wife was older than the

husband (unions which one would scarcely expect to be very fruitful), only 22 were fertile; in these 30 boys and 26 girls were born.

Schramm likewise gives a table of births in which the man was at least five years older than the woman, and found the proportion to be 97.18 boys to 100 girls, almost identical with that of Ahlfeld.

A. suggests that it would be interesting to control these figures by ascertaining the proportionate percentages of the children in primiparæ of 26, 24, 22, and 20 years of age, and all subsequent years, which would probably result in accordance with the descending progression, 139 : 133 : 124 for women older than 32, 30, and 28 years, in the normal proportion of 106 boys to 100 girls, in women older than 26, 24, 22, and 20 years.

These results certainly militate strongly against the theory of Hofacker and Sadler, that the more the age of the father exceeds that of the mother, the greater will be the excess of male children, and, according to A., confirm the hypothesis (denied above by Mayrhofer) that the chief factors for the future sex of the child lie in the maternal system.

P. F. M.

GYNECOLOGY.

9. VERY LARGE CYST OF THE BROAD LIGAMENT; COULD NOT BE REMOVED AFTER ABDOMINAL INCISION; SEWED IN ABDOMINAL WOUND; CURE. By Prof. SCHATZ, of Rostock.—(*Arch. für. Gynäk.* ix. 1.)

Lina S., æt. 24, had suffered for three years and a half, at longer or shorter intervals, from severe pains over the whole abdomen, but especially upon the right side.

These pains, during the last year, had been more severe and frequent. Appetite had diminished, and there was considerable loss of flesh. Examination revealed a large abdominal tumor, reaching from symphysis pubis nearly to the border of the thorax; right and left side equally expanded, fluctuation distinct throughout, and no thickening of walls apparent. Uterus and right ovary could be easily felt by bimanual examination.

On December 21, 1874, no previous puncture having been made, the abdomen was opened low, and upon the right side of the tumor, near to it, and yet distinctly separate and freely movable, the left ovary was also found. It became at once evident that the tumor consisted of a large cyst of the broad ligament, which, in its growth, had not only expanded the broad ligament, but had also lifted the peritoneum, and pushed this before it, as its covering. The cyst had too, in a measure, grown into the mesentery of the S. Romanum.

Strictly speaking, there was of course no pedicle; and as the cyst could not be brought entirely out of the body, the complete emptying of it was impossible—the wall was secured into the abdominal wound. After a few days the retained cystic fluid underwent decomposition, and was with much difficulty washed out with carbolic acid solution. As some of the fluid of the cyst had escaped into the abdominal cavity, drainage was established, the tube passing through the Douglas cul-de sac, behind the uterus, and above the point where the cyst was fastened to the abdominal wounds. The peritoneal drainage tube was removed four days after the operation.

The abdominal cystic fistula closed at the beginning of April. Up to this time there had been almost continual fever and several discharges containing portions of the decomposed cyst wall. Patient was very much reduced in weight. She left the middle of May. In July there could be felt, through the abdominal wall, a remnant of the cyst—this was very hard and about the size of a fist. The abdominal fistula, with the cyst wall, again opened; this was then dilated, and, by means of a double catheter, daily injections made with a carbolic acid solution. A putrid, purulent fluid was discharged Sept., 1875. Patient well and strong, but the cystic opening not entirely obliterated.

The quantity of cystic fluid removed was $12\frac{1}{2}$ kilogrammes, and the result of its examination was as follows: Faintly alkaline, specific gravity 1012 at 15° C., and it contained, besides serum-albumen, a considerable amount of fibrinous

substance. Other albuminous substances were not present. There was especially no paralbumen, or, more exactly expressed, no albuminoid material, which forms a precipitate by the addition of carbonic acid, or is not dissolved by chloride of soda. The amount of serum-albumen was 1.78 per cent. ; "Aschenbestandtheile" (ashy products), 0.935 per cent. These last contained soda, potash, traces of lime, phosphoric acid, carbonic acid, chlorine, but no trace of sulphuric acid.

Carbonate of soda made up 0.16 per cent. of the fluid.

Of other substances there was a small quantity of fat in emulsion. Some urea. Out of three litres about 0.3 grm. saltpetre and some urea could be separated. No succinic acid, no sugar, and no definitely appreciable quantity of leucin. For comparison, we give in this connection the result of the examination of a multilocular ovarian cyst. The first table shows the result of examination of a small cyst; the second table that of a larger one. These cysts, although adjoining each other, were entirely separate, and the examination was made by Prof. Jacobson, who also examined the cyst of the broad ligament.

I. Colorless, very opalescent, slimy fluid, spec. gravity, 1022 at 15° C.; distinctly alkaline (0.053 per cent. anhydrous carbonate of soda).

| | |
|---------------------------------|--------|
| Sum of solid products..... | 4.827% |
| Among these, serum-albumen..... | 1.85% |
| Ashy products..... | 0.84% |

II. Brown, ropy, tenacious fluid, spec. gravity 1031 at 15° C.; distinctly alkaline (0.14% anhydrous carbonate of soda).

| | |
|---------------------------------|--------|
| Sum of solid products.... | 8.802% |
| Among these, serum-albumen..... | 5.44% |
| Ashy products..... | 0.84% |

Of albuminous bodies there was only serum-albumen and fibrinous substance; no paralbumen or other (albuminous) products; succinic acid was not present, but on the other hand traces of urea could be found.

We see, therefore, that in many cases we cannot by chemical examination distinguish between the contents of an ovarian cyst and one of the broad ligament.

The contents of two cystic cavities, in the same multilocular, ovarian cyst, when compared with each other, show wider differences than when one of these is compared with the contents of a cyst of the broad ligament.

Dr. Prochownick, assistant of Schatz, has collected a number of cases of operations of cysts of the broad ligament, among which we find the following statements:

Dr. Bird and C. Hawkins operated upon such cases as early as 1850. Dr. Clay still earlier. Clay treated forty cases by simple puncture, and in only six was there refilling.

Lawson Tait found among tumors, which had been removed as unilocular ovarian cysts, in three cases the ovary lying upon the cyst wall; in three cases mesovarium of different breadth between both; in one case the ovary and tube lightly attached to the cyst without belonging to it; in one case ovary and tube were left beneath the écraseur in the abdominal cavity; in one case the clamp was placed at least one inch above the same, the wall was very thick and contained many unstriped muscular fibres.

Analysis of the cases leads to the following facts:

All the cases occurred during the period of sexual activity, with one exception, which was after the 66th year.

The menstruation was regular in nearly all cases; yet there are of the twenty-four cases four which, from their history, do not permit us to exclude the possibility of sure etiological connection with the anomalies of menstruation.

Ten were unmarried, eight married, and of these three sterile; one had given birth to one child only; four had several children; chlorosis, cessation of menses, and the puerperal condition seem to have no etiological connection.

The affection had lasted in some cases three-quarters of a year; in some cases eleven years or more—average two to four years.

The cysts were chiefly unilocular. Yet in one case a small secondary cyst was found in the wall, and in one case five or six sacs were present. Kiwisch found several small cysts near each other. In one case a small cyst was found near a large one.

The size of the cyst was generally small as compared with an ovarian cyst; often, however, quite as large. Usually not larger than a man's head.

The wall of the cyst was frequently very thin and transparent, at other times thick and firm. In the latter case, the cyst proper, after an intermediate deposit of more or less dense connective tissue, was partly or entirely surrounded by the peritoneum as a second encasement. This encasement towards the cyst wall is often very movable. When the peritoneal covering is complete, the cyst has a pedicle like an ovarian cyst. If the peritoneum only partly covers the cyst, there is no pedicle.

The contents of a cyst having a thick wall are chemically and physically like, or similar to, an ovarian cyst; if the wall be thin, the fluid contains but little albumen, and is of light specific gravity. It is true that in the first case we find thinned epithelium, as distinct from ovarian cyst; yet this is not always so.

Simple puncture often effected a cure when the fluid in the cyst was thin and of low specific gravity, but not when it was rich in albumen and of high specific gravity. In such a case it must be treated as an ovarian cyst, and removed where the peritoneal encasement towards the pelvis contracts itself like a pedicle. Where the pedicle formation was absent, the cyst wall (a piece of it having been removed or not), was sewed into the abdominal wound, and a fistula established; and thus, through suppuration, a cure established.

As to the *diagnosis*, if we exclude the cystic myomata of the uterus, the dermoid cysts, echinococci, etc., because of their more ready diagnosis by means of puncture, it is highly probable that a cyst recognized as multilocular is not a cyst of the broad ligament. A cyst which appears single must be punctured; if after puncture it is found not to be single, or at least if its wall is thick and cordy, indicating a conglomeration of cysts, we can also leave out of consideration a cyst of the broad ligament. If after the puncture the cyst appears single, and its walls equally thick throughout, we cannot make the differential diagnosis between a cyst of the broad ligament and of the ovary.

When the fluid is of low specific gravity, clear and poor in albumen, it is probably a cyst of the broad ligament. If it is of high specific gravity, thick or rich in albumen, we must seek the differential diagnosis from other conditions.

If the tumor is developed outside of the years of sexual activity; if its development is slow and the tumor of moderate size; if there is but little disturbance of digestion or general health; if the uterus be relatively low; if fluctuation can be distinctly felt from the rectum and vagina; if the pedicle cannot be felt; if ciliated epithelium can be discovered upon the trocar, and a body felt upon the side of the tumor having the form of an ovary, there is an increased probability that the tumor is one of the broad ligament.

Yet a perfectly sure diagnosis cannot be made from the existence of only part of these conditions, since frequently elements of the parovarium from which cysts of the broad ligament develop, are found in the ovary; and on the other hand degenerated Graafian follicles grow into the broad ligament; but even independent of this exchange of elements between ovary and broad ligament, there are some ovarian cysts whose contents are of low specific gravity and poor in albumen; and, on the other hand, pure parovarian cysts whose contents are of high specific gravity and rich in albumen.

Without abdominal incision, we are therefore unable from our present knowledge to avoid diagnostic errors.

Therapeutics.—Every cyst which is probably a cyst of the broad ligament, or at least, appearing single, may be such, must be punctured. At the punc-

ture, have at hand the areometer, spirit lamp, and nitric acid. If the fluid is of high specific gravity, rich in albumen or its derivatives, let the cyst only be emptied to such an extent as to determine whether it be multilocular or unilocular, with its wall varying in thickness, or whether it be a simple cyst without any thickening of the wall. If the first be true, we conclude that it is an ovarian cyst and treat it accordingly. In the second case, as it is doubtful whether it is an ovarian cyst or one of the broad ligament, it is also better to proceed with the abdominal incision, for the chances are in favor of its proving to be ovarian; and even if it be of the broad ligament, it may have a pedicle like an ovarian tumor. In the event of the absence of any pedicle, or the impossibility of enucleating the cyst, it is necessary to sew the cyst wall (after a portion of it has been removed, or not) into the abdominal wound, and perhaps form a second fistula through Douglas's cul-de-sac.

If upon explorative puncture the fluid is found to be of low specific gravity and poor in albumen, and the wall single and of uniform thickness, the cyst should be entirely emptied, as it is probably a cyst of the broad ligament; and we can expect, by its simple evacuation and with the help of supporting measures, to accomplish a cure.

Only when a second puncture, combined with internal measures, fails to effect a cure, and the general health seems to demand it, should we proceed to the abdominal incision; and if now we find that the cyst has no pedicle, a piece of the wall can be cut out, which possibly may remain open, and keep up a communication between the cyst and abdominal cavity.

D. B. H.

10. RECTO-VAGINAL FISTULA. By PROF. RICHET, Surgeon to l'Hôtel Dieu. (*Annales de Gynécologie*, June, 1876.)

In discussing this subject the author calls especial attention to the great pressure made upon the rectum by the intestinal gas, and considers this the cause of the frequent rupture of the sutures after the operation for this form of vaginal fistula.

Nothing has as yet been found to completely overcome the obstacle arising from this source. While we can constipate the bowels by the use of opium, and check the mucons secretions by local astringents, the various means devised to get rid of the gas (such as metallic and rubber tubes, etc.) more or less excite contraction of the rectal sphincter, thus preventing the escape of gas, and defeating the object for which they are employed.

In view of these facts, Richet, as early as 1867, proposed and performed the operation of dividing the anal sphincter, either by an incision anywhere in its periphery, or by dividing the perineum at the height of the fistula; in fact, transforming the recto-vaginal fistula into a complete perineal rupture.

This first operation was successful with the exception that a small fistulous opening remained after the parts had healed. This opening, however, was readily closed by the application of nitrate of silver.

The next operation was done by forcibly dilating the sphincter as for fissure. This was entirely successful; in fact, the author urges that this division or rupture of the sphincter is the only method of insuring success in these cases.

In performing this operation a few months ago at the Hospital Clinique (from later reports we find that it was successful), the Professor makes the following suggestions as to the *modus op-randi*:

1. The forcible dilatation or incision should be made before uniting the edges of the fistula, otherwise the sutures are likely to be torn out.

2. In place of freshening directly upon the edges of the fistula, as practised in other autoplasic operations, R. freshens one or two centimetres from the edge, making a wound around the fistula. The strips removed are broad, and comprise the chief part of the vaginal mucons membrane, but do not include the deep surface of the rectal mucus. Metallic sutures are closely applied from above downwards and in front of the edges of the fistula, so that these may be turned back towards the rectum. To this last procedure R. attaches

a certain importance, as thus a prominent crest is formed upon the rectal mucous membrane. The fecal secretions pass upon each side of this crest, and are thus kept away from the healing surfaces, so also with the gas.

3. Constipation is effected by preparations of opium.

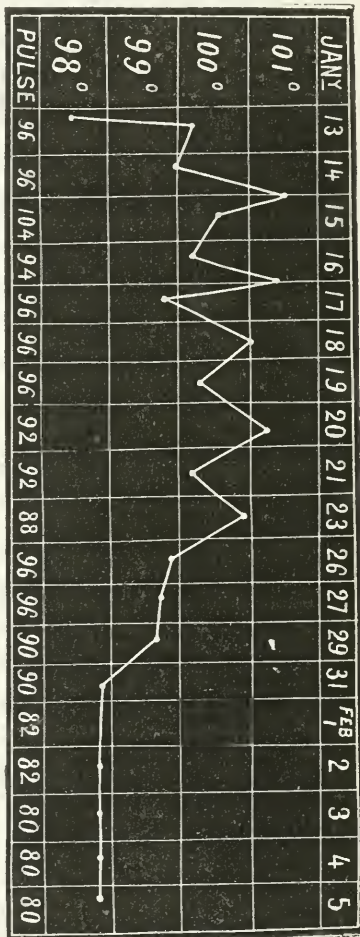
D. B. H.

11. A CASE OF EXCISION OF BOTH OVARIES FOR FIBROUS TUMOR OF THE UTERUS, AND A CASE OF EXCISION OF THE LEFT OVARY FOR CHRONIC OOPHORITIS AND DISPLACEMENT,

are reported by DR. E. H. TRENHOLME, of Montreal, in the *Canada Lancet for July, 1876*. The excision of both ovaries was performed on 13th January, 1876, upon a patient thirty-two years of age, and thirteen years married, who for seven years had been suffering severely from pains and hemorrhage preceding each menstrual period. Every effort had been tried to relieve the patient without avail; and, as her protracted and increasingly severe sufferings threatened at no distant period to terminate her life, it was determined to remove both ovaries. This course was adopted as being less serious than extirpation of the uterus, and as offering a reasonable hope that, with the absence of the ovaries, menstruation would cease, and the tumor remain quiescent. The operation was successful (the pulse and temperature ranging as shown in Curve I.), and although there were monthly flows during the three following months, such hemorrhages were entirely checked by astringents without injury to her general health. Such a result could not have been attained had the monthly hemorrhage been really menstrual. There seems to be strong grounds for believing that *nidation* and *denidation* can take place only under the influence of the ovaries. This aspect of the matter as illustrated by this case opens up a new view of uterine pathology and treatment.

The fact that uterine fibroids often cease to trouble patients who have passed their climacteric, indicates the probably successful treatment of such growths by premature age being artificially arrived at by the removal of the ovaries.

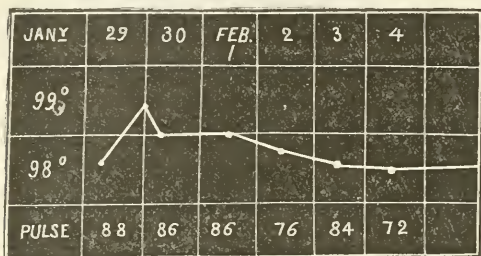
The second case, that of chronic inflammation of the ovary with displacement, is that of a lady twenty-seven years of age, who began to have ovarian trouble before her marriage, some seven years ago. Since marriage, the ovarian pains and pains down the left leg have become more and more severe, rendering her a confirmed invalid. There is dyspareunia and an absence of all sexual desire. The pelvis is shallow and the uterus low down in the pelvis. On examination a small, olive-shaped body is felt in Douglas's fossa. The



left ovary cannot be detected in its normal place, while the right organ is easily recognized where it should be.

Pressure on the body in the posterior cul-de-sac gives exquisite pain and almost causes fainting.

The diagnosis was chronic oöphoritis with displacement downward. The treatment suggested was excision of the organ, as every form of medical and surgical effort had failed to give the slightest benefit. The patient readily agreed to the proposal, and the operation was performed on 29th January, 1876. The cul-de-sac was brought into view by a Sims' speculum, and the posterior wall of the vagina divided longitudinally to the extent of about three-quarters of an inch. The peritoneum covering the ovary was then divided, and the organ



brought out through the incision, external to the vulva, and separated by the scissors after ligating its ligament with hempen thread (carbolyzed).¹ The wound healed without difficulty, and the patient made an excellent recovery. At no time was it necessary to use a catheter, and the temperature did not exceed 99.5°.

while the pulse never ranged beyond 88. (Curve II.)

So far as relieving the patient's sufferings, the operation did not succeed. This result, however, should not deter from the future performance of the operation in a similar case, as the long persistence of the inflammation of the organ had very probably induced a pathological condition of the nerve which might be avoided by an earlier resort to the knife.

¹ There were no folds of small intestine in the cul-de-sac in either this or the preceding case.

REVIEWS AND NOTICES OF BOOKS have been crowded out of this Number, and will appear in the January Number.

COMMUNICATIONS HAVE BEEN RECEIVED from DRs. S. C. BUSEY, Washington, D.C., on "Congenital Occlusion and Dilatation of Lymphatic Channels;" J. W. ROSEBRUGH, Hamilton, Ontario, on "Membranous Dysmenorrhœa;" A. F. A. KING, Washington, D. C., on "The Transitional Link between Physiological and Pathological Changes in the Uterus;" JOHN S. WARREN, N. Y., on "Dyspareunia;" THOS. ADDIS EMMET, N. Y., on "Removal of a Fibrous Tumor from the Uterus by Traction, with Remarks on the Operation;" REIMANN, Kiew, Russia, on "The Simultaneous Entrance of Twin Heads into the Pelvis."

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